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Subsidies for parental leave and formal childcare: be careful what you wish for

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Abstract

I exploit the introduction of a policy package in France aimed at helping parents with the care of young children. The reform affected all households with pre-school age children and had two dimensions: a short stay-home subsidy for first-time mothers wishing to take-up parental leave and an increase in childcare subsidies for parents using childminders—the main formal care option in France. Importantly, policymakers did not explicitly intervene in the childcare infrastructures. I rely on a diff-in-diff empirical strategy to evaluate the labour market outcomes of mothers with pre-school age children in the short-run and the long-run. The reform had negligible effects in the short-run. In the long-run though, first-time mothers—and particularly the lower-educated group—took advantage of the parental leave subsidies to reduce their employment rate. This freed up formal childcare places and allowed middle-class educated mothers of two children to use the more generous childcare subsidies and therefore work more. The fact that the effects take time to materialise and do not appear at the aggregate level for the targeted population suggests that the policy did not induce any net increase in the supply of care places and simply led to a re-allocation of care modes among mothers of pre-school age children.

Keywords Labour supply · Maternity leave · Parental leave · Childcare subsidies

1 Introduction

The impact of family-friendly policies on mothers' labour market outcomes have been vastly studied (see Olivetti and Petrongolo (2017) for a recent review). Public discourse and policymakers often support parental leave and cheap childcare provision without necessarily studying the interaction of these different programs on

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labour or childcare markets and public finances. In this article I rely on a French 2004 policy change to highlight the aggregate and distributional consequences of a broad increase in parental leave benefits and childcare subsidies when the policymaker does not explicitly adjust the supply-side infrastructures of the childcare market.

The reform offered a 6-month stay-home subsidy for first-time mothers wishing to take-up parental leave, independent of household income. At the same time, childcare subsidies were increased for all households with pre-school age children using childminders[1] and eligibility was restricted to working parents. Previous studies (Givord and Marbot (2015); Joseph et al. (2013)) have looked at each benefit modification while ignoring the other contemporaneous change, and adopted different methodologies in their analysis. I argue that the reform should be evaluated in its entirety with a consistent framework and not in a piecemeal approach to credibly identify the impact of each program. Differentiating the short-run and long-run effects of the reform is also crucial.

I take advantage of the variation in program eligibility across demographic groups to analyse the impact of each policy change and their potential interaction. To be more specific, I focus on mothers with one or two children whose youngest is of pre-school age (i.e., below 3 years old). I study the impact of the policy on the entire group and on the following subgroups: (1) first-time mothers with a child younger than 1 year old (mostly affected by the parental leave subsidy introduction), (2) first-time mothers with a child between 1 and 3 years old (affected directly by childcare subsidy changes and potentially by persistent effects of having interrupted their career during the short parental leave) and (3) mothers of a second child of pre-school age (solely affected by childcare subsidy changes). I rely on Labour Force Surveys to study the short and long-run reaction of mothers' labour market outcomes: employment, weekly working hours, hourly wages. The reform design allows me to define the short-run as the first 3 years of implementation and the long-run as the following 3 years. I adopt a diff-in-diff methodology where the control group are mothers of elder children unaffected by the reform. In the appendix I provide robustness checks on the common trend assumptions.[2]

I find that in the short run, mothers eligible to the new benefits did not react significantly along any dimension. However, in the long-run, mothers eligible to the parental leave ended up reducing their employment rate by 6.6% points to stay home with their new born child. I show that the effect was nearly twice as large for the lower educated as for the higher educated. Once eligibility to the parental leave expired, no impact of the policy package can be observed, meaning that the short parental leave had no persistent effect on these mothers' labour market outcomes and childcare subsidies did not boost their labour supply. Furthermore, in the long-run, mothers of two children whose youngest was between 1 and 3 years old increased their employment rate by 4.2% points as a result of the more generous childminder care subsidies. In the article, I find that these effects were concentrated among middle-class, educated and married mothers in that group.

The fact that the effects take time to materialise and do not appear at the aggregate level for the entire group of pre-school age mothers suggests that the policy did not

[1] This was the main type of formal childcare in France at the time of the reform.

[2] I also check that father's labour supply and fertility choices were not impacted by the reform.

induce any net increase in the supply of care places and simply led to a re-allocation of care modes among mothers. In the last section of the paper, I use household surveys and data on the supply-side of the childcare market to confirm this last prediction. I find evidence that among mothers with a child aged 1 to 3 years old, first-time mothers may have been crowded out by those with two children and childminders may have captured part of the childcare subsidy. In 2009, the total amount spent on the private carers subsidies by the government was 4.6Bn € (0.24% of GDP) while the figure stood at 2.3Bn € (0.14% of GDP) in 2003.

2 French pre-school policies and the reform

2.1 Reform announcement and justification

On April 29, 2003, the French government announced changes to the benefit system directed at families with children younger than 6 years old.[3] The reform would affect every child born after January 1, 2004 and hence mothers could not delay a pregnancy in order to enter the new system. The birth of a child after that date pushed the whole family into the new system—including for benefits claimed in relation to elder siblings of the new-born. This means that the family of a child born on December 31, 2003 would have received the benefits according to the old system for all its children, while the family of a child born at the start of January 2004 would receive the benefits according to the new system for all its children. Importantly, the changes in the system would not have impacted the amount of taxes or benefits received from other programs and there were no contemporaneous reforms focusing on other parts of the tax and benefit system that may have offset some of the policy changes studied here.

The reform was announced a year after the new centre-right conservative majority came to power, replacing a centre-left government. The reform officially aimed at supporting mothers in their childcare choices following the birth of a child. It should be noted that the reform impacted middle-class households with younger children who are likely to be marginal voters. The timing of the reform is also noteworthy as the reform would have been gradually phased-in and completed by the start of the next national election cycle in 2007. For an in-depth presentation of the tax-benefit system in France, de Muizon (2018) or Laroque and Salanié (2002) are useful references.

2.2 Childcare in France

In France, mothers can take up to 3 months of maternity leave after a child's birth,[4] and can stay out of work until the child turns 3 years old. Their employer has to offer a similar position when they decide to come back to work. Children can enter school

[3] Prior to the reform this benefit system was called APE ("Allocation Parentale d'Education"), with the reform the new benefit system was given the name of PAJE ("Prestation d'Accueil du Jeune Enfant").

[4] The maternity leave is normally 6 weeks pre-birth and 10 weeks post-birth, but 3 weeks pre-birth can be substituted for 3 weeks post-birth, bringing the maximum duration to 13 weeks post-birth.

Table 1 Main childcare modes, children aged 0 to 3 years old

%	Working mother		Non-working mother	
	2002	2007	2002	2007
Childminder	32.6	31.6	2.1	2.0
Nanny	1.9	2.1	0.3	0.0
Kindergarten	15.5	15.1	0.8	1.9
School	1.4	2.3	0.1	1.0
Other	48.7	48.8	96.8	95.1

Source: "Enquêtes mode de garde et d'accueil des jeunes enfants," DREES

from the age of three, where they can be cared for all day. Prior to that, there exists two main types of formal care: the "creches" (i.e., public kindergarten) and the "assistantes maternelles" (i.e., childminders). These childminders should be officially registered and are allowed to look after up to three children at the same time.[5] There also exists "Gardes a domicile" (i.e., nannies) who look after the household's children in the household's home.[6] Finally, some children may be able to start school from 2 years old.

In Table 1, I report the distribution of childcare modes in 2002 and 2007 for all children younger than 3 years old.[7] Childminders are the main type of formal care chosen by working mothers, followed by kindergarten. Nannies or early schooling are only chosen by a minority of parents. Mothers not working mostly rely on informal modes of care such as their own time and family.

The distribution of childcare modes before the reform in 2002 and after the reform in 2007 hardly changed among the population of mothers with pre-school age children. At the time in France, female labour supply was on a long-term structural rise, and Table 1 shows that the number of childminders and kindergarten places was rising in line with that trend to maintain a constant distribution of childcare modes. Further evidence that the supply of childminders was rising at a constant pace during that time is provided in the appendix.

2.3 Reform details

The 2004 reform modified the benefits specifically dedicated to households with pre-school children. These were mainly composed of two pillars:

(1) Parental leave with a stay-home benefit that is not means-tested.

[5] An assistante maternelle is "accreditated" (allowed) to look after a certain number of children by the local authorities. The number of children to care for increases with seniority and other criteriae.

[6] They are more expensive than childminders and the subsidies for this type of care were affected by the reform.

[7] Such information is only available for the years 2002 and 2007 around the reform and come from the household surveys described in the last section of the article.

(a) If the mother (or father) decides to stop work completely or to reduce her hours of work in order to look after the child, she would receive a fixed benefit every month unconditional of the household resources. The benefit transfer is highest if she stops work completely and lower if she reduces her working hours to 28 or 18 hours a week. In order to claim it, she needs to have been in employment for a minimum duration in the past.

(b) Prior to the reform, these benefits were available from the second birth onwards until the youngest child turned three years old.

(c) In 2004, women who had their first child became eligible, but only for up to 6 months. The transfer to part-time workers was also marginally increased and the conditions on past work experience became more stringent.[8] Table 5 in the appendix summarises the changes.

(2) Childcare subsidies to cover some of the costs incurred by using a professional registered childminder.[9]

(a) These are means-tested direct cash transfers to pay part of the childminder's salary. Their generosity depends on the household's income bracket. A minimum of 15% of the childminder's salary has to be paid by the household in any case. The daily cost of a child cared for by a childminder cannot exceed a nationwide upper bound. The generosity of the subsidy varies according to three income thresholds and the number of children that are being cared for.[10]

(b) With the reform, the childcare subsidies became available exclusively to working parents[11] and were paid every month (usually one or two weeks after the claim) instead of every 3 months. The reform increased the generosity of these subsidies by increasing the thresholds and the subsidy available at every bracket level. Figures 1 and 2 summarise the thresholds and maximum benefit available for couples with two incomes under the old and the new system.

Hence, the main changes could be summarised as follows: an extension to one-child families of the parental leave subsidies, and childcare subsidies when relying on registered childminders became more generous but were made conditional on working. Further details on each policy change are respectively provided in Joseph et al. (2013); de Muizon (2014); and Givord and Marbot (2015). Table 2 summarises how households were affected according to their demographics. I report in the appendix the evolution of the number of households claiming each benefit throughout the period.

[8] Having worked for two years prior to the child's birth, within a timeframe that depends on the number of existing children in the family.

[9] These childcare subsidies are also available for children between the age of three to six but are about half the value of those for children below three years old. Most of these children go to school, so these subsidies are less popular. I have looked at this group, but did not find any significant impact of the subsidies' changes and decided to focus only on pre-school children.

[10] Note that the government also pays 100% of the employer social contributions.

[11] To be eligible, one needed a monthly salary of at least €374 if a lone parent and €748 if in a couple.

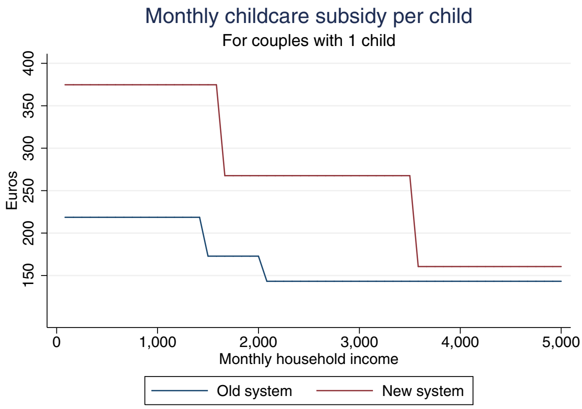

Fig. 1 Childcare subsidies schedule, one child

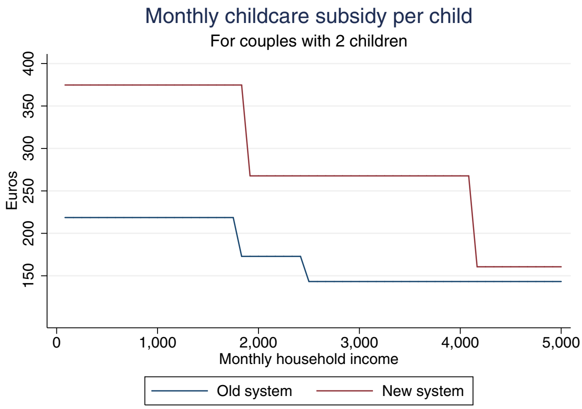

Fig. 2 Childcare subsidies schedule, two children

Table 2 Summary of the policy changes on different household groups

		Youngest child			
		Old system		New system	
		3–11 months	1–3 years old	3–11 months	1–3 years old
1 child	Parental leave	No	No	Yes	No
	Childcare subsidies	Yes	Yes	Yes, increased generosity	Yes, increased generosity
2 children	Parental leave	Yes	Yes	Yes	Yes
	Childcare subsidies	Yes	Yes	Yes, increased generosity	Yes, increased generosity

3 Literature review

3.1 Previous studies on the reform

The 2004 Paje reform was the focus of two recent studies that looked at the short-run impact of the parental leave and childcare subsidies separately.

Joseph et al. (2013) focused on the impact of the parental leave subsidy extension to first-time mothers looking at their employment rates and earnings in the period following the parental leave. They adopted DiD and propensity matching techniques while using mothers ineligible to the new scheme—because their child was born before the cut-off date—as a control group. They found that for mothers who took up the 6-month parental leave subsidy there were no negative effect on their labour market outcomes 12, 18 or 24 months after the birth. They also found that mothers who chose to stay employed part-time using the part-time transfer had higher probabilities of staying in employment 12 and 18 months after the birth but not 24 months, particularly for lower educated women.

Givord and Marbot (2015) studied the childcare subsidy changes of the 2004 reform. They focused on the short-term impact of the reform on participation, employment rates and official childcare spending with a DiD methodology.[12] They focused exclusively on 2 years old children in families with one and multiple children. This means that the families with only one child aged 2 years old are the same group as those studied by Joseph et al. (2013). Givord and Marbot (2015) found a small positive impact on mothers participation and reported spending on official paid childcare, particularly concentrated among mothers of two children.

The two previous studies of the reform focused solely on its short-term impact. Besides, each study looked at one of the two benefit changes in isolation without controlling for their potential interaction on mothers' decisions directly or indirectly. Finally, by relying on the ineligible mothers as a control group, their strategies do not account for the fact that the supply of childcare providers may not have reacted strongly in the short-run and that the policy could have simply led to a substitution of care places used by mothers under the old system to mothers under the new system, biasing the estimated results. In contrast, I study both the short-run and long-run effects of the policy package. I also disentangle the parental leave effect from the childcare subsidies by providing a clear break-up of the policy impact on demographic groups that were affected differently. Lastly, I rely on a control group of mothers with older children that would not have been affected directly or indirectly by the reform.

3.2 Broader literature review

The literature studying the impact of childcare provision on mothers' labour supply is relatively extensive (see Carta and Rizzica (2018) for a detailed review). The majority of micro studies exploit variation in eligibility over time across geographical areas or over children date of birth. Most articles study the expansion of childcare provision (via public schooling or kindergarten) for children usually aged three and above. These event-type studies tend to find stronger effects of childcare provision on mothers labour supply in countries where childcare provision was initially scarcer and more expensive (Fitzpatrick (2012); Gelbach (2002); or Cascio (2009) in the US, Baker et al. (2008) or Lefebvre and Merrigan (2008) in Canada, Nollenberger and

[12] Their dataset did not allow them to study working hours, wages or disentangle reported spending on official childcare between hourly cost and hours used of formal care. This last point should be important as the aim of subsidising childcare is generally to achieve cheaper hourly cost for parents or a higher number of hours used. Simply increasing the revenue of childcare providers via subsidies to parents is not usually the aim of such policies.

Rodríguez-Planas (2015) in Spain, Bauernschuster and Schlotter (2015) in Germany or Takaku (2019) in Japan). In an institutional setup with more favourable existing childcare institutions, these event-type studies report far more limited or constrained impacts (Havnes and Mogstad (2011) or Hardoy and Schone (2015) in Norway, Goux and Maurin (2010) in France). The evidence on simply providing childcare subsidies to parents without expanding care places at the same time is more limited. Lundin, et al. (2008) find that increasing already high childcare subsidies had no impact on Swedish mothers labour supply, while Brewer et al. (2016) report an impact only when the price of full-time care is reduced in the United Kingdom. Finally, Francesconi et al. (2015) shows theoretically that policymakers should be careful in designing childcare subsidies since they may have unintended consequences for single parents. My study contributes to the literature by focusing on a policy that explicitly changed the childcare subsidies but not the availability of places and was targeted at very young children (less than 3 years old).

Regarding parental leave, while relatively fewer studies have been published to date, evidence suggests that in a variety of set-ups, short parental leave may delay the decision of mothers to return to work but does not significantly affect long-term employment (Lalive and Zweimüller (2009) in Austria, Schönberg and Ludsteck (2014) in Germany or Dahl et al. (2016) in Norway). In France, Rodrigues and Vergnat (2019) studied the key determinants of parental leave decisions for mothers. Joseph et al. (2013) provide a thorough discussion of the literature.

A few papers try to highlight how the full set of policies available to support young mothers may affect their labour supply from a cross-country perspective (Olivetti and Petrongolo (2017) in the OECD or de Muizon (2018) in France and the United Kingdom). The evidence using event-type studies in a single country, hence providing full institutional control, to understand the interaction of these different policy dimensions is very scarce to my knowledge and provides further motivation for this article.

4 Mothers' labour market outcomes using the Labour Force Surveys

4.1 Data and baseline specification

To analyse the impact of the policy change on mothers' labour market outcomes I rely on the French Labour Force Surveys between 2001 and 2009, collected by the French National Statistical agency (INSEE).[13] These are representative continuous rolling-panels interviewing households in a particular week for six consecutive quarters. They provide detailed information, every quarter, about employment, hours of work, region of residence and demographics like gender, age, education attainment, marital status, number and age of children etc. Information on earnings from work are collected only in the first and last interviews. Questions in the surveys follow ILO recommendations and the number of observations per year is around 280,000.

[13] Prior to 2003, the surveys were collected once a year and were called Enquête Emploi. In 2003, it was replaced by a quarterly rolling panel (Enquête Emploi en Continu) where each household was interviewed for six consecutive quarters. The procedure for data collection was very similar between the two surveys and I control for potential differences in my specification.

The date of birth of the youngest child allows me to identify households with pre-school age children under the new or old regime. I focus only on households with less than three children, no twins and where the mother is older than 18 years old. The benefit system was further modified for families with three or more children in 2006 and the employment decisions of large families may also be affected by different considerations. The legal maternity leave duration is 3 months in France, so I omit observations where the youngest child is below 3 months old. I also drop from my sample mothers whose youngest child was born a month before and after the policy introduction to avoid any possible birth shifting around the reform cutoffs.[14] I define mothers as employed if their weekly working hours are positive and construct hourly wages using reported earnings and working hours information. Educational attainment is split into five categories: did not finish secondary school, secondary school graduate, high school graduate, above high school, university graduate. Table 6 in the appendix provides summary statistics of the main variables in the sample.

I estimate the short-run and long-run impacts of the policy on employment rates, weekly working hours and hourly wages using a diff-in-diff framework. The estimated parameters identify intention-to-treat effects. I run the regressions on the overall sample of mothers as well as on four separate sub-groups of the sample. The four sub-groups are defined by the number of children (one or two) and age of the youngest child (between 3 months to 11 months old and between 1 and 3 years old). The demographic groups were chosen to reflect the different program changes of the reform as highlighted in Table 2 and in the first section.[15]

4.2 Identification strategy

I define the short-run as the period during which the group of mothers with pre-school children under the old and new system co-existed: January 2004 to December 2006. The long-run is then defined as January 2007 to December 2009. In both periods, I recover the direct impact of the new policy by using a diff-in-diff strategy with the pre-policy period defined as January 2001 to December 2003. The treatment group is mothers of pre-school aged children (i.e., below 3 years old and under the new system). They are compared to a control group of mothers whose youngest child is in primary school (i.e., 6 to 10 years old). I choose this control group because the benefits and subsidies for families whose youngest child was aged 3 to 6 were also modified during the period under study.[16] I stop the analysis in 2009 because in January 2010, the youngest mothers in the control group (i.e., those whose youngest just turned 6 years old) would have been eligible to the new policy package at birth 6

[14] For example, for a parental leave reform introduced in Germany in 2007, Neugart and Ohlsson (2013); Tamm (2013); and Jürges (2017) showed that mothers successfully delayed births to enter a new generous system (through postponed C-sections and inductions)

[15] Because mothers might combine the maternity leave with some normal vacation leave, I define the group cut-off as having a child up to 11 months old instead of nine and half months old (13 weeks of maternity leave plus 26 weeks of stay-home benefits).

[16] Even though children can and usually go to school from the age of 3 onwards, households may still employ private carers to look after children outside of school hours for those younger than 6 when childcare subsidies are still available to parents.

years earlier. I study the impact of the policy changes on mothers' employment rates, working hours and hourly wages.

More formally, for the outcome variable y, the parameter capturing the policy impact in the short-run can be represented as:

$$\gamma^{SR} = (\bar{y}_{T,policy}^{04-05-06} - \bar{y}_{T}^{01-02-03}) - (\bar{y}_{C,older\ children}^{04-05-06} - \bar{y}_{C,older\ children}^{01-02-03}).$$

In the long-run, the total impact of the policy can be recovered by comparing the outcomes of the treatment group in 2007, 2008 and 2009 (3 years after the policy implementation) versus 2001, 2002 and 2003 with those of the control group during the same time-frame:

$$\gamma^{LR} = (\bar{y}_{T}^{07-08-09} - \bar{y}_{T}^{01-02-03}) - (\bar{y}_{C,older\ children}^{07-08-09} - \bar{y}_{C,older\ children}^{01-02-03}).$$

For both the short-run and long-run specifications, I run the following regression:

$$y_i = \alpha + \beta' Treated_i + \beta' Year_i + \gamma(Treated_i * Post_i) + \beta' X_i + e_i,$$

where the variable $Post_i$ is a dummy equal to one if the observation is in the period after the policy change (after January 2004) and $Treated_i$ is a dummy for the treatment group of mothers. I include yearly dummy variables instead of the $Post_i$ dummy to control for the transition from yearly to quarterly Labour Force Surveys in 2003 and common macro shocks—note that the long-run period contains the 2007 peak and 2009 trough years in the business cycle. Standard errors are clustered at the household level to control for the fact that households remain in the survey for 6 quarters. In all the regressions I include a set of control variables X_i that are likely to affect the labour supply decision, namely: the education level, age and its square, age of the youngest child in months,[17] a dummy for living in Paris, and dummies for the quarter of interview.

As with any difference-in-differences approach, the results rely on the assumption of a common trend and same distribution of unobservables between the treated and control group. To check the validity of the control group, I reproduce the trends of both groups' outcome variables, in the appendix (Figs. 11–16). I also perform an event-type study with yearly indicators. This analysis supports the assumption that the common trend assumption holds.

4.3 Estimates from the baseline specification

In this subsection I first discuss the results of the main DiD specification for the entire group of mothers and the four demographic sub-groups that are presented in Table 3 below. I then perform a series of robustness checks to ensure that the main findings are not relying on a particular choice of covariates or identification strategy. Having demonstrated the reliability of the main specification, I then report the heterogenous impact of the reform along educational and household composition dimensions. The estimates from the robustness checks and heterogeneous impact are reported by demographic group in Tables 7–11 in the appendix.

[17] This is particularly necessary in the short-run period as the child age composition of the treated group evolves throughout the quarters and years. For instance, in the year 2004, only the mothers with a child younger than one are receiving the new benefit and are hence in the treatment group.

Table 3 Overall policy impact on mothers of pre-school age children

			Short-run			Long-run		
Num. children	Age of last child	Main policy change	Emp rate	Work hours	Wages (%)	Emp rate	Work hours	Wages (%)
1 and 2	0–3 years old		−0.010	−0.196	−0.003	−0.003	−0.191	0.003
			(0.008)	(0.242)	(0.013)	(0.009)	(0.274)	(0.010)
			56,722	32,725	15,491	71,044	41,306	18,238
Among whom								
1	<1 year old	Parental leave	−0.020	−0.551	0.042	−0.066***	−0.199	0.031
			(0.015)	(0.450)	(0.023)	(0.018)	(0.526)	(0.020)
			16,095	9683	4525	17,872	10,845	4904
1	1–3 years old	Childcare subsidies	−0.019	−0.015	−0.013	−0.009	−0.032	0.027
			(0.013)	(0.399)	(0.020)	(0.015)	(0.405)	(0.015)
			19,842	12,487	6199	26,964	16,997	7671
2	<1 year old	Childcare subsidies	0.016	0.593	−0.043	−0.000	0.576	−0.022
			(0.015)	(0.555)	(0.030)	(0.018)	(0.614)	(0.026)
			23,631	14,428	6279	24,934	15,542	6647
2	1–3 years old	Childcare subsidies	0.022	−0.631	−0.032	0.042***	−0.504	−0.031
			(0.013)	(0.439)	(0.022)	(0.015)	(0.453)	(0.017)
			27,229	16,268	7365	32,949	19,671	8449

Estimates significant at the 95 or 99% confidence levels are reported with ** and ***, respectively. Standard errors in parenthesis and the sample sizes below

4.3.1 Main results

Table 3 presents the estimates from the main specification. The table reports the results along the extensive and intensive margins of work, as well as wages in the short-run and the long-run. The DiD regressions have been estimated on the entire group of mothers with one or two children whose youngest is of pre-school age, as well as on each of the four demographic sub-groups defined in Table 2.

In the short-run, defined as the 3 years post reform implementation (2004 to 2006), neither labour supply along the extensive or intensive margin, nor observed wages were affected by the policy change in any group. In the long-run, defined as the three following years (2007 to 2009), the policy had no impact on the entire treatment group along any dimension. Yet this apparent aggregate neutrality of the policy hides large movements in employment rates across the sample. On the one hand, the employment rate of mothers of a first child younger than 1 year old dropped by 6.6% points. This is a likely consequence of the generous parental leave subsidy extension towards this group. On the other hand, the policy pushed up by 4.2% points the employment rate of mothers with two children whose youngest was between 1 and 3 years old. To clarify, this group of mothers was eligible to the more generous childminder subsidy but not the parental leave subsidy extension. The absence of a policy impact on first-time mothers with a child older than 1 year old would imply that taking-up short parental leave in the child's first year had no persistent effect on mothers labour market outcomes. The policy had no impact on working hours or wages in any of the four demographic sub-groups.

4.3.2 Robustness checks

I now report a battery of checks to ensure that the main results presented above are not driven by the particular choice of specification.

4.3.3 Graphical evidence

Firstly, I report in Figs. 3 and 4 the de-trended time-series of employment rates for the two groups that experienced significant changes after the policy implementation.[18] The fall in employment rates of first-time mothers takes time to materialise as reported in Table 3 but is indeed large. Similarly, the pick-up in employment for mothers of two children whose youngest is 1 to 3 years old only materialises after a few years.

4.3.4 Different estimation specifications

Secondly, I test the robustness of the main DiD specification by modifying the estimation along three dimensions. I add a proxy for local labour demand using the

[18] For brevity, I do not report graphs for the other groups and variables as they confirm the absence of a significant impact from the policy. The graphs are available upon request and can be seen in a preliminary draft of this paper in chapter 1 of de Muizon (2014).

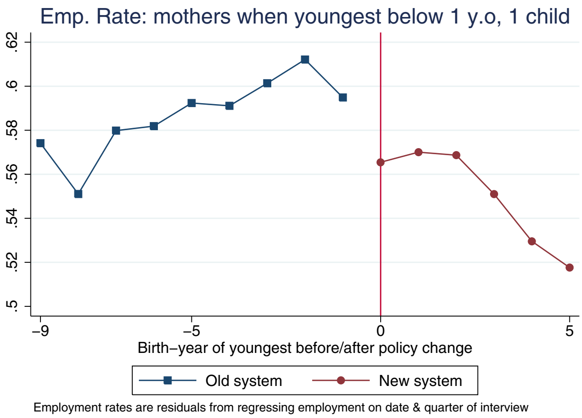

Fig. 3 Employment rates mothers, one child younger than 1 year old, per year of birth

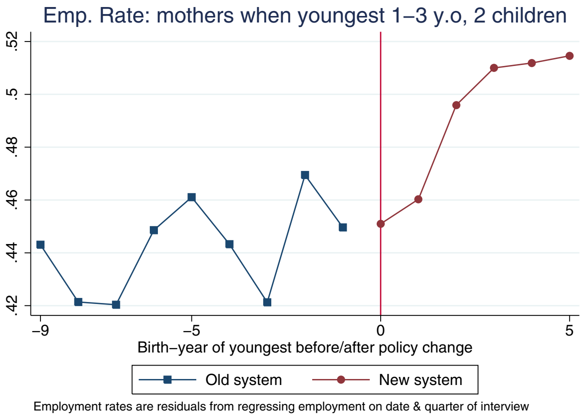

Fig. 4 Employment rates mothers, two children youngest 1–3 years old, per year of birth

local unemployment rate.[19] I also re-run the main specification regressions with only one observation per household. Households may be observed for up to six consecutive quarters in the French Labour Force Surveys. The main specification controls for that by clustering the standard errors at the household level. With this check, I ensure that the main conclusions are not affected by the choice to use as large a sample as possible in the main specification. Finally, I re-run the main specification regressions without any control variables.

The results of these checks are reported in the first three rows of the Robustness subsections in Tables 7–11 in the appendix. Each table reports the results for a different demographic group under study. Adding a local unemployment rate control does not alter the findings in any meaningful way.

[19] I use the local administrative area called "departement" that can usually be crossed within a couple of hours of driving. I did not include this control in the main specification because this is an imperfect proxy as it doesn't fully match with households commuting area if they live close to departmental borders.

When using only one observation per household, most results align with the main specification. Only for mothers of two children whose youngest is 1 to 3 years old do results differ to an extent (Table 11). In the short-run, the negative estimate on working hours is now significant. In the long-run, the negative policy impact on observed wages is now slightly larger and significant. The latter could reflect lower unobserved productivity of the mothers pushed into employment by the policy.

Lastly, I run the main specification regressions without any control variables. The impact in the short-run on employment for the entire group of mothers remains negative but is now larger and significant (Table 7). This is driven by the first-time mothers of a child aged 1 to 3 (Table 9). A close enquiry of this regression shows that the addition of the age squared variable is crucial in reducing the magnitude of this coefficient in the main specification. The other noticeable difference with the main specification concerns wages of first-time mothers with a child younger than 1 year old (Table 8). In both the short-run and the long-run, the positive estimates are now larger and significant. The educational covariates are responsible for the fall in the coefficients when adding controls to the regression. The explanation does not lie in differential trends in educational achievement between control and treatment groups. Instead, the culprit is a larger impact of the policy on lower educated mothers, resulting in a compositional change along the education level of the treated mothers working population. If the policy had a relatively larger negative impact on mothers with lower education, the sample of working mothers in the post period would be more educated. This is what I find in the sub-section further down discussing the heterogenous impact of the policy. All the other coefficients in the regression without control variables align closely to those of the main specification.

4.3.5 Different identification strategy

Thirdly, I rely on the data structure and the reform specificities to modify the identification strategy and check the reliability of the results in the short-run.[20] Indeed, the reform did not replace an existing system with a new one at a specific date. It replaced the existing system with a new one for the family of children born after the start-date of the reform. That is to say, a child born on 1 January 2004 would push his family to the new system while the family of a child born on the 31 December 2003 would remain on to the old system. I exploit this characteristic of the reform design to modify the identification strategy in two ways. Firstly, I maintain a diff-in-diff approach but substitute the control group of mothers whose youngest child is aged between 6 to 10 years old for mothers whose youngest child is in the same age group as the treated mother but not eligible to the new reform at the time of observation.[21] Secondly, I use a regression

[20] The checks presented here cannot be performed for the long-run period. By 2007, all children below the age of three would be born after 1 January 2004, implying that their mothers would automatically be under the new benefit regime. In chapter 2 of Jourdain de Muizon (2014) I used a structural labour supply model to simulate the impact of the stay-home subsidy on first-time married mothers' work choices. The results suggested that the long-run impact of the policy would reduce their employment rates by slightly more than 11% points, a similar order of magnitude to the one found here in Table 8. Also, the structural model approach predicted very little effect on working hours, in line with the results presented in the main specification.

[21] For instance, in July 2005 a mother whose youngest is 1 to 3 years old but born before 2004 would be in the control group, while the mother of a child born in 2004 would be in the treatment group.

discontinuity diff-in-diff (RD-DiD) framework similar to Canaan (2019) and Persson and Rossin-Slater (2019). In this approach, I basically compare mothers' labour market outcomes whose children are born within a 6-months window on each side of the policy cut-off calendar date. Using mothers of children born in the same months but in pre-reform years I difference out seasonality effects. I therefore compare the outcome of mothers whose youngest is born after 1 January 2004 to those whose youngest is born prior to 1 January 2004, relative to the differences in outcomes for mothers whose youngest is born in the same months in the previous 2 years (January–June 2003, 2002 versus July–December 2002, 2001 respectively).[22]

The results of these two set of checks are reported in the last two rows of the Robustness checks subsections in Tables 7–11 in the appendix. The main specification finds no significant impact of the policy on fifteen estimates. Each of the fifteen estimates from the different control group specification or RD-DiD method are also non-significant. The sign of the estimates between the main specification and the modified ones are not always the same but it is only for working hours of mothers with two children whose youngest is between 1 and 3 years old that both robustness checks coefficients may differ from the main specification one.[23] That estimate was negative and non-significant in the baseline specification, significant when only one observation per household was used, but turned positive when using a different control group or the RD-DiD framework.[24] The magnitude of the estimates remain relatively small (less than 2 h for an average work-week of 31 h pre-reform).

4.3.6 Heterogeneous impact of the policy

I now differentiate the impact of the policy for mothers according to their education levels or household status. I define a mother as higher educated if she at least graduated from high school and lower educated otherwise. The results are reported in the last four rows of Tables 7–11 in the appendix.

[22] I run a regression similar to Persson and Rossin-Slater (2019), that takes the following form:

$$y_{itp} = \alpha + \beta_1 1[t \geq c] + \beta_2 R_i * 1[t \geq c] + f(t-c) + 1[t \geq c] * f(c-t) + \theta_p + \beta' X_i + e_{itp},$$

where y_{itp} represents the mother's labour market outcome, R_i is an indicator set to 1 for children in the reform sample (i.e., born 2 quarters on each side of 1 January 2004), c denotes January 1 of any observation period, the dummy variable $1[t \geq c]$ is set to one for children born in the first two quarters of the year, $f()$ is a linear function controlling for different trends in the period before and after the policy cut-off date. I include fixed effects for each of the three periods of observation θ_p, note that the main effect of R_i is therefore absorbed by the period dummy. The months before January 1 are represented as $(c-t)$,and after as $(t-c)$.The control variables X_i are the set of controls used in the main specification. The coefficient of interest capturing the impact of the policy is β_2. I restrict the analysis to women with a child born within a 2 quarters window around the policy cut-off date.

[23] Looking at confidence intervals and using simple Z test.

[24] In these two robustness checks, the treated mothers are compared to a group of mothers whose youngest child is in the same age bracket but were not eligible to the new policy. This is a similar approach to Joseph et al. (2013) or Givord and Marbot (2015). As discussed in the literature review, their approach may bias the estimates if the non-treated mothers were indirectly affected by the policy changes. If the non-treated mothers found it harder to find childcare places for example, this may negatively affect their labour supply and could result in an apparent positive effect of the reform on treated mothers' labour supply. But this estimated positive effect would disappear when the treated mothers are compared to a group of mothers not impacted by the reform in any way, as in my baseline specification.

In terms of employment rates, starting with the group of treated mothers as a whole in Table 7, the policy package had a negative impact on the employment rate of lower educated mothers in the short-run (2.5% points) and in the long-run (3.5% points). This is driven by the particularly large impact of the policy on first-time mothers with a child younger than 1 year old who took up parental leave. In the short-run it is about 4.0% points albeit not significant, while in the long-run the impact is 9.6% points and strongly significant (Table 8). It is also worth highlighting that all the employment rate estimates for lower educated mothers across all the sub-groups in both periods are negative in Tables 7–11. This observation suggests that the policy package had no positive effect on employment for any group of lower educated mothers. For the higher educated mothers, there was no impact of the policy in the short and long run at the aggregate level (see Table 7). However, in the long-run this masks the drop in employment rate by 5.0% points for first-time mothers eligible to parental leave subsidies (Table 8) and the employment rate increase of 4.8% points of mothers with two children whose youngest is 1 to 3 years old (Table 11). In terms of policy impact by household composition, the policy also had a short-run negative impact on married mothers but not in the long-run (Table 7). Indeed, in the long-run this was likely compensated by the married mothers of two children whose youngest is 1 to 3 years old that increased their employment rate by 5.5% points (Table 11).

Looking at working hours, starting with the group of treated mothers as a whole (Table 7), the rise for the higher educated group in the short-run was compensated by lower hours in the lower educated group. This pattern was replicated across the four sub-groups and in the long-run (although not all estimates are significant). This complements the findings on the employment rate that suggested the policy package had no positive impact on the labour supply of any lower educated group. Also, single mothers weekly hours fell by more than 2 h in both short and long-run. The negative impact on single mothers is observed in both periods for all sub-groups but only significant in the long-run for first-time mothers of a child aged 1 to 3 years old (Table 9). These mothers may have either reduced working hours in the first year of the child to use the part-time stay-home subsidy and not adjusted their labour supply back up once it expired or they may have struggled to find adequate care options for their child.

Finally, when it comes to hourly wages, the policy appears to have had no impact across education groups or household types when focusing on the group of treated mothers as a whole (Table 7). For first-time mothers of a child younger than one, the estimate is positive in the short-run for the lower educated group and in the long-run for the higher educated group (Table 8). The former is unexpected as the same group reduced their working hours which is usually associated with part-time wage penalties. The latter effect likely reflects selection of mothers with unobservable low productivity characteristics out of employment. The positive impact reported in the long-run for higher educated first-time mothers of a child aged 1 to 3 years old (Table 9) remains hard to explain. The policy had no impact on the wages of any group of mothers with two children (Tables 10 and 11).

5 Understanding the mechanisms using childcare surveys

The previous section showed that in the short-run, the response to the policy was small overall but in the long-run, first-time mothers whose child was younger than 1 year old strongly reduced their employment to go on parental leave, especially the lower-educated group. At the same time, the higher educated mothers of two children with the youngest between 1 and 3 years old increased strongly their employment as a result of more generous childcare subsidies. Finally, the take-up of short parental leave subsidies had no persistent effect on mothers employment.

These findings may suggest that the childminder care subsidies were effective at pushing higher educated mothers of two children to work. But this was achieved as first-time mothers, and particularly lower-educated mothers, delayed their return to employment after birth and single mothers reduced their working hours. The fact that the effects take time to materialise and do not appear at the aggregate level for all treated mothers suggests that no net increase in the supply of care places was induced by the policy and the policy simply led to a re-allocation of care modes among mothers. I provide tentative evidence of such mechanisms at play in this section.

5.1 Data description

The French Labour Ministry collected surveys[25] in May 2002 (before the policy change) and November 2007 (what I defined earlier as the long-run when no households with pre-school children were eligible for the old benefit system) containing information on households' childcare arrangements. Both surveys are a representative cross-section of the population and interviewed 3343 and 8177 households (respectively) with at least one child younger than seven and a half years old. Each household was asked about their total disposable income as well as details on all the benefits they were receiving. These surveys were designed to understand how households used different types of care and how this changed with the reforms of 2004. Very specific information was collected on time spent in each mode of care during 1 week. Information on gross costs as well as net costs was reported. The survey also contains demographic variables such as the age of the parents, the date of birth of all the children in the household, and the education level of the parents. The survey contains information on the household's annual income but unfortunately it does not report any information on wages, even though it does report the employment status of the parents.

5.2 Income quartiles most responsive to the reform

The survey allows me to identify households by their reported income quartile and compare the pre and post reform employment outcomes of households with young children. The surveys do not allow me to repeat a diff-in-diff analysis, but the raw statistics are informative and confirm the results found in the previous section using

[25] "Enquêtes mode de garde et d'accueil des jeunes enfants" from the DREES (Directorate of Research, Study, Evaluation and Statistics) agency.

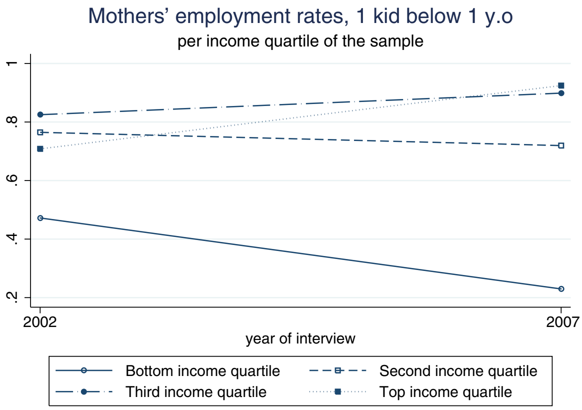

Fig. 5 Mothers of one child employment rates by income quartile

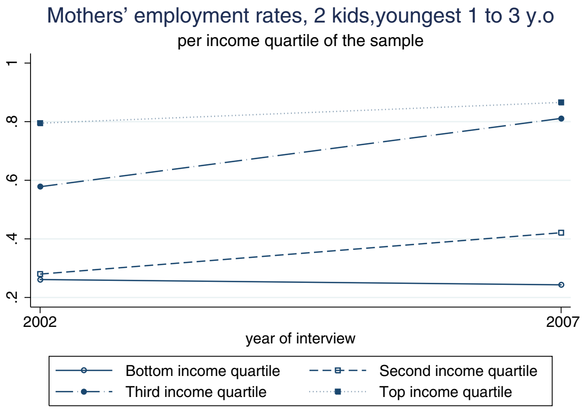

Fig. 6 Mothers of two children employment rates by income quartile

Labour Force Surveys. Among first-time mothers of a child younger than 1 year old the fall in employment was particularly concentrated among lower-income households (Fig. 5 below). Among mothers of two children with a child between 1 and 3 years old (Fig. 6 below), the increase in employment was concentrated among households in the second and third income quartiles (those falling within the monthly income bracket between 2000 € to 4000 € in Fig. 2 presented earlier).

Table 4 Proportion of mothers in each state

Youngest 1–3 years old		Two children		One child	
		2002	2007	2002	2007
Work	Private carer	17.3	22.0	26.1	29.3
	Kindergarten	5.2	9.3	15.5	12.9
	Other	25.1	30.2	32.8	37.8
No work	Private carer	1.1	0.7	2.9	1.3
	Kindergarten	0.2	0.9	0.4	0.9
	Other	52.1	35.8	22.4	17.8
	Total	100%	100%	100%	100%

Source: "Enquêtes mode de garde et d'accueil des jeunes enfants," DREES

5.3 Changes in the allocations of formal childcare across mothers

In this section, I exploit the information on childcare choices to shed light on the reasons why among mothers of a child aged 1 to 3 years old, the policy only increased employment for those with two children and not those with only one child.[26] In Table 4 for each year and demographic group I report the distribution of work status and childcare choices observed. For brevity I combine childminders and nannies as a group "Private carers" and include school as part of "Other" types of care.

The first row of Table 4, shows that in 2002, 17.3% of mothers with two children were working and using a private carer while in 2007, 22.0% of these mothers were working and using a private carer. The share of first-time mothers working and using private carers only increased by 3.2% points in the same period. The second row shows that among mothers with two children, the share working and using kindergarten rose by 3.1% points while it fell by 2.6% points among first-time mothers. The third row shows that the share of mothers working and using another mode of care rose by slightly more than 5.0% points for both types of households. From the last three rows of the table we can conclude that the share of mothers with two children not working fell by 16.0% points, a move about 10.0% points larger than for first-time mothers.

Hence, this discussion confirms that unlike first-time mothers, the working mothers of two children increased their use of formal care. Working first-time mothers compensated their reduced reliance on kindergarten by higher usage of childminders while mothers of two children increased their reliance on both types of care by larger proportions.

[26] I focus here on mothers whose youngest is between 1 and 3 years old as they were the most affected by the childcare subsidy changes in the main analysis. To check the arguments made in that paragraph and extend the analysis to mothers whose youngest was below one, I estimated probabilities of working and using a specific childcare mode by following a strategy similar to Baker et al. (2008). The results are presented in the appendix.

The policy increased the available supply of formal care places to working mothers, by restricting eligibility to childminder subsidies to them and encouraging first-time mothers out of employment via parental leave subsidies. Understanding the exact mechanisms in the childcare market is beyond the scope of the paper but a possible explanation to the resulting allocation of childcare places among working mothers could be as follows: the structural rise in female labour supply would have encouraged mothers of a second child to increase their employment. Those whose elder sibling was already in a kindergarten may have been given priority, crowding-out first-time mothers. At the same time, the more generous childminder subsidies encouraged further mothers to look for formal care and work. Mothers of two children may have used a particular childminder for their first child and kept on using her for the second child while first-time mothers would have had to find a fresh match in the childcare market.

5.4 Childminders capturing part of the subsidy payment

Each household using childcare was asked about the gross cost per hour used and the subsidies they received in the surveys. This allows me to compare the gross and net hourly costs in 2002 (pre-reform) and in 2007 (post-reform). In constant Euros, the median pre-reform hourly gross cost was 2.36 € and the net cost 1.20 €. In 2007, these numbers were respectively 2.82 € and 0.82 €. This suggests that the reform's modification of childcare subsidies was successful in decreasing the price paid by parents, but a non-negligible share of these subsidies was captured by childcare providers.[27]

This can be explained by the fact that the hourly rate paid to a childminder is not fully set by market forces. It cannot fall below a threshold defined as a proportion of the national minimum wage (28.1% of the minimum wage). There also exists an upper limit to the hourly rate a childminder could charge. The childcare subsidies can be claimed by the parents only if the childminder charges less than 63% of the hourly minimum wage. So the price paid to a childminder is the result of a bargaining process between parents and the carer, where the agreed hourly price is set between these two bounds. The price being bounded, it is likely that the allocation of places in the market is not simply the result of price adjustments. Priority for siblings, social networks, or previous employment history between the parents and the childminder may play an important role in the allocation of scarce childcare places. The weak correlation between the average wage of childminders across regions with the number of available places for young children in the region (reported in Fig. 7) and the apparent absence of an increase in the supply of childminders at the national level induced by the reform (as discussed in the appendix), suggests that the supply of

[27] To confirm these findings, I study the evolution of childminders' wages in the Labour Force Surveys. The hourly wage of childminders moved from 3.27 € in 2002 to 4.50 € in 2007 and 5.48 € in 2009, which represents a 24% increase in real terms by 2007 and 46% by 2009 when the minimum wage in real terms was increased by 11% and 13% respectively (from 6.75 € to 8.36 € and 8.77 €). Note that technical reports from French statistical institutes and agencies also found some evidence of these effects. For instance, see Marical (2007),"Les determinants des salaires des assistantes maternelles et les effets de la PAJE," Recherches et Previsions, 88: 35–52.

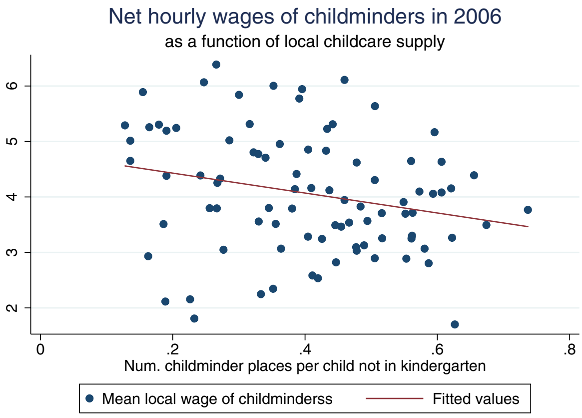

Fig. 7 Supply of childminders uncorrelated to their earnings

childminders is not very elastic and indeed, other factors than the price per hour explain the allocation of scarce childcare places.

6 Conclusion

I exploited the introduction of a policy package aimed at helping parents with the care of young children. The reform had two dimensions: a short stay-home subsidy for first-time mothers wishing to take-up parental leave and an increase in childcare subsidies. Importantly, policymakers did not explicitly intervene in the childcare infrastructures. I proposed a consistent broad empirical strategy to evaluate the labour market outcomes of mothers with pre-school age children to the new policy package in the short-run and the long-run.

In the short-run, the response to the new policy was overall negligible. In the long-run, the childminder care subsidies were effective at pushing middle-class, educated mothers of two children to work. However, this was achieved as first-time mothers—particularly lower educated mothers—reduced their employment via the take-up of parental leave subsidies. The policy package had a very limited impact on first-time mothers' labour market outcomes once the eligibility to parental leave subsidies expired, despite their eligibility to more generous childcare subsidies. Lastly the policy package had no positive effect on the labour supply of any group of lower educated mothers.

The reform did not induce a rise in the overall supply of childminders who ended up capturing part of the increase in childcare subsidies. The new policy appears to have impacted the mix of care modes between different types of households with mothers of two children getting enhanced access to formal care places.

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Compliance with ethical standards

Conflict of interest The author declare that I have no conflict of interest.

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7 Appendix

7.1 Details of the changes to the parental stay-home subsidies

7.2 Benefit take-up

The Agency in charge of allocating and paying the family-related benefits CNAF ("Caisse Nationale d'Allocations Familiales") reports every year the number of claimants to each of its benefits. This data is the source used to construct the graphs presented here. Prior to the reform, the female labour supply was generally increasing in France, and as a result, the number of claimants to childcare subsidies for private carers was increasing as shown in Fig. 8. Figure 9 reports the number of claimants in France to the stay-home subsidies for all types of households. The increase in the number of claimants in 2004 and 2005 by about 20,000 for the fully out-of-work transfer is mainly attributable to first-time mothers becoming eligible to the benefit (see Joseph et al. 2013). Each year there are about 300,000 first-time mothers in France. The generosity of that benefit was not modified for mothers of two and more children (Table 5 above). For mothers choosing to reduce their working hours post-birth and claim the part-time stay-home subsidy, about 10,000 first-time mothers claimed it and the large increase observed in Fig. 9 is driven by mothers of two or more children. Surprisingly, the number of mothers claiming the benefit to completely stay out of work started to fall after 2006. This is due to a fall in the number of

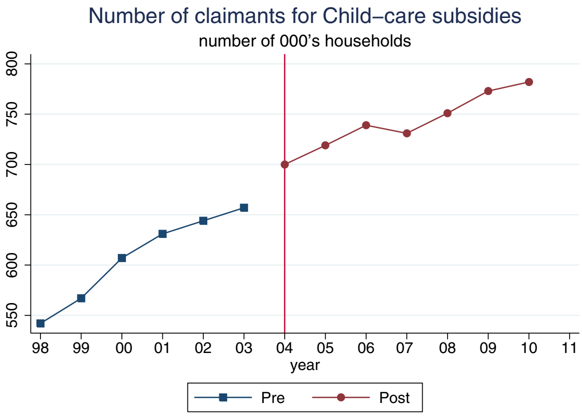

Fig. 8 Number of childcare subsidies claimants

Table 5 Summary of the stay-home subsidies changes

		Youngest child			
		Old system		New system	
	Monthly transfer	3–11 months	1–3 years old	3–11 months	1–3 years old
1 child	Stops working	Ineligible	Ineligible	531 €	Ineligible
	Works half-time	Ineligible	Ineligible	404 €	Ineligible
	Works 80% of full-time	Ineligible	Ineligible	305 €	Ineligible
2 children	Stops working	531 €	531 €	531 €	531 €
	Works half-time	351 €	351 €	404 €	404 €
	Works 80% of full-time	265 €	265 €	305 €	305 €

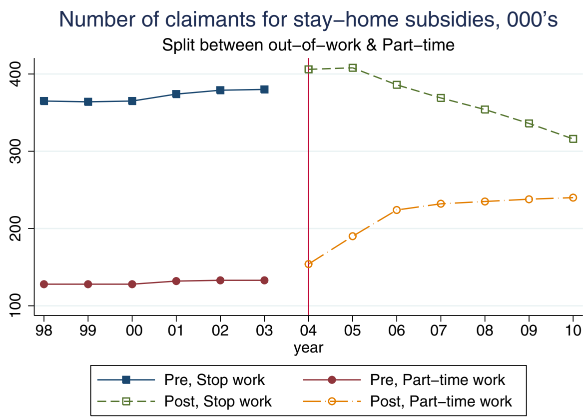

Fig. 9 Number of claimants to the stay-home subsidies

mothers with more than one child choosing to stay home fully while the number of first-time mothers claiming the full subsidy remained broadly constant.[28]

7.3 Summary statistics and regression results table

7.4 No reform-induced rise in childminders

At the aggregate level, the reform did not induce any rise in the supply of care places beyond the long-term trend associated with secular rise in female employment.[29] While the number of childminders kept rising after the policy was implemented, the growth rate was similar to the years prior to the policy change (see Fig. 10). A slight

[28] This is described in more details in the yearly bulletin of the DREES, see Vanovermeir (2011), "Les prestations familiales et de logement en 2009 Les bénéficiaires des aides á la garde d'enfants plus nombreux," Etudes et resultats, 769, DREES.

[29] The DREES (Directorate of Research, Study, Evaluation and Statistics) agency from the French Labour Ministry reports every year the number of childcare providers at the national and local level.

Table 6 Sample summary statistics

	Control group					Treated group				
	Mean	St. dev.	Min	Max	*N*	Mean	St.dev	Min	Max	*N*
Employment rate	0.79	0.41	0	1	49,691	0.61	0.49	0	1	49,882
Working hours	33.07	9.55	1	60	39,178	33.34	8.71	1	60	30,256
Hourly wage	9.86	5.24	3	91.8	14,909	9.89	4.90	3	88.0	12,689
Education										
Secondary school	0.35	0.48	0	1	49,691	0.25	0.43	0	1	49,882
High school	0.19	0.39	0	1	49,691	0.22	0.41	0	1	49,882
Above high school	0.16	0.36	0	1	49,691	0.20	0.40	0	1	49,882
University graduate	0.12	0.32	0	1	49,691	0.21	0.41	0	1	49,882
Num. children	1.63	0.48	1	2	49,691	1.47	0.50	1	2	49,882
Age	38.9	5.23	18	55	49,691	30.4	5.07	18	55	49,882
Paris	0.15	0.36	0	1	49,691	0.16	0.36	0	1	49,882
Married	0.63	0.48	0	1	49,691	0.54	0.49	0	1	49,882

Dummy for education level lower than secondary school omitted

Table 7 Main results for full sample of mothers with pre-school age children

Mothers with child < 3 years old	Short-run			Long-run		
	Emp. rate	Working hours	ln(W)	Emp. rate	Working hours	ln(W)
N	56,722	32,725	15,491	71,044	41,306	18,238
2001–03 average	0.593	32.476	2.123	0.593	32.476	2.123
Policy impact	−0.010	−0.196	−0.003	−0.003	−0.191	0.003
	(0.008)	(0.242)	(0.013)	(0.009)	(0.274)	(0.010)
Robustness checks						
Local unemployment rate	−0.011	−0.212	−0.003	−0.003	−0.205	0.003
	(0.008)	(0.242)	(0.012)	(0.009)	(0.274)	(0.010)
One observation per household	0.002	−0.196	0.008	−0.002	−0.361	−0.007
	(0.014)	(0.429)	(0.021)	(0.009)	(0.292)	(0.012)
No control variables	−0.025***	−0.196	0.011	−0.010	−0.153	0.003
	(0.008)	(0.243)	(0.015)	(0.011)	(0.278)	(0.012)
Control group: non eligible mothers	−0.009	0.063	0.008			
	(0.010)	(0.304)	(0.017)			
RD-DiD specification	−0.008	0.377	−0.006			
	(0.024)	(0.650)	(0.025)			
Heterogeneous impact						
Higher educated	−0.008	0.619**	−0.005	0.003	0.150	0.024
	(0.009)	(0.305)	(0.017)	(0.012)	(0.342)	(0.014)
Lower educated	−0.025**	−1.591***	0.020	−0.035**	−0.735	−0.000
	(0.012)	(0.438)	(0.020)	(0.016)	(0.488)	(0.015)
Married	−0.026**	−0.087	0.001	−0.003	−0.219	−0.008
	(0.010)	(0.316)	(0.017)	(0.012)	(0.371)	(0.0137)
Single	−0.022	−2.300**	0.046	−0.027	−2.301***	−0.009
	(0.023)	(0.996)	(0.041)	(0.027)	(0.0815)	(0.029)

Significance levels reported at 5% level **, 1% level ***. Higher educated means high-school graduate and above

Table 8 Results for first-time mothers with a child younger than one

Mothers of one child < 1 year old	Short-run			Long-run		
	Emp. rate	Working hours	ln(W)	Emp. rate	Working hours	ln(W)
N	16,095	9683	4525	17,872	10,845	4904
2001–03 average	0.652	33.323	2.060	0.652	33.323	2.060
Policy impact	−0.020	−0.551	0.042	−0.066***	−0.199	0.031
	(0.015)	(0.450)	(0.023)	(0.018)	(0.526)	(0.020)
Robustness checks						
Local unemployment rate	−0.021	−0.556	0.042	−0.065***	−0.200	0.031
	(0.014)	(0.449)	(0.024)	(0.018)	(0.526)	(0.020)
One observation per household	−0.015	0.472	−0.029	−0.073***	−0.515	0.011
	(0.028)	(0.829)	(0.049)	(0.021)	(0.684)	(0.026)
No control variables	−0.027	−0.530	0.066**	−0.066***	−0.175	0.050**
	(0.016)	(0.456)	(0.028)	(0.019)	(0.530)	(0.024)
Control group: non eligible mothers	−0.038	−1.871	0.084			
	(0.033)	(1.123)	(0.078)			
RD-DiD specification	−0.036	0.140	0.083			
	(0.049)	(1.274)	(0.066)			
Heterogeneous impact						
Higher educated	−0.002	0.580	0.039	−0.050**	0.343	0.057**
	(0.018)	(0.547)	(0.032)	(0.021)	(0.641)	(0.026)
Lower educated	−0.040	−2.612***	0.078**	−0.096***	−1.355	0.022
	(0.026)	(0.901)	(0.039)	(0.032)	(1.040)	(0.037)
Married	−0.004	0.328	0.063	−0.076***	0.560	0.012
	(0.022)	(0.685)	(0.036)	(0.027)	(0.822)	(0.030)
Single	−0.002	−1.339	0.173**	−0.049	−1.115	0.114
	(0.048)	(2.049)	(0.073)	(0.054)	(2.360)	(0.082)

Significance levels reported at 5% level **, 1% level ***. Higher educated means high-school graduate and above

modification in the data methodology between 2001 and 2002 explains the apparent stagnation in the number of carers around that time.

7.5 Common trend assumptions

I reproduce below time-series of the dependent variables for the treatment group (mothers with one or two children younger than 3 years old) and control group (mothers with one or two children aged 6 to 10 years old). To further check the validity of the control group and ensure that the pre-reform trends were similar between mothers of pre-school children and mothers of older children, I perform an event type of study. I split my sample between observations in the treatment group

Table 9 Results for first-time mothers with a child between 1 and 3 years old

Mothers of one child 1–3 years old	Short-run			Long-run		
	Emp. rate	Working hours	ln(W)	Emp. rate	Working hours	ln(W)
N	19,842	12,487	6199	26,964	16,997	7671
2001–03 average	0.701	33.253	2.072	0.701	33.253	2.072
Policy impact	−0.019	−0.015	−0.013	−0.009	−0.032	0.027
	(0.013)	(0.399)	(0.020)	(0.015)	(0.405)	(0.015)
Robustness checks						
Local unemployment rate	−0.002	−0.045	−0.013	−0.011	−0.065	0.026
	(0.022)	(0.399)	(0.020)	(0.015)	(0.404)	(0.015)
One observation per household	−0.002	0.802	−0.016	−0.008	−0.251	0.014
	(0.022)	(0.645)	(0.032)	(0.014)	(0.435)	(0.018)
No control variables	−0.038***	0.069	0.005	−0.016	0.015	0.031
	(0.014)	(0.396)	(0.024)	(0.016)	(0.413)	(0.018)
Control group: non eligible mothers	−0.027	−0.944	−0.008			
	(0.016)	(0.483)	(0.025)			
RD-DiD specification	−0.021	−0.381	0.029			
	(0.035)	(0.912)	(0.036)			
Heterogeneous impact						
Higher educated	−0.016	0.896	−0.016	0.002	0.267	0.052**
	(0.017)	(0.509)	(0.028)	(0.017)	(0.524)	(0.021)
Lower educated	−0.014	−1.376**	−0.007	−0.033	−0.303	0.008
	(0.022)	(0.699)	(0.032)	(0.026)	(0.688)	(0.021)
Married	−0.029	0.838	−0.006	−0.033	0.887	0.027
	(0.019)	(0.578)	(0.031)	(0.022)	(0.628)	(0.022)
Single	−0.028	−2.622	0.051	−0.016	−2.950***	0.043
	(0.039)	(1.442)	(0.062)	(0.037)	(1.005)	(0.038)

Significance levels reported at 5% level **, 1% level ***. Higher educated means high-school graduate and above

and control group. I regress the dependent variable on yearly dummies, a dummy if in the treatment group and yearly indicators interacted with the treatment dummy. I do not perform the analysis for years before 2000 as the labour market outcomes of the treatment group may still have been affected by the APE reform changes in 1994 (Piketty (2005) was identifying effects until 1998).

Along the extensive margin of work decision (Fig. 11), the common trend assumption in the decade around the reform seems to hold.[30] Figure 12 presents the coefficients and the 95% confidence interval of the yearly indicators interacted with the treatment dummy for the extensive margin of work. The yearly indicators for the treatment group are not significantly different from zero. Post 2004 though, the

[30] Note that in 2004, the treated group is only composed of mothers whose youngest child is younger than one, explaining the sharp drop observed in that year in Fig. 11.

Table 10 Results for mothers of two children with youngest below 1 year old

Mothers of two children < 1 years old	Short-run			Long-run		
	Emp. rate	Working hours	ln(W)	Emp. rate	Working hours	ln(W)
N	23,631	14,428	6279	24,934	15,542	6647
2001–03 average	0.488	30.708	2.237	0.488	30.708	2.237
Policy impact	0.016	0.593	−0.043	−0.000	0.576	−0.022
	(0.015)	(0.555)	(0.030)	(0.018)	(0.614)	(0.026)
Robustness checks						
Local unemployment rate	0.0214	0.576	−0.045	−0.001	0.567	−0.022
	(0.015)	(0.555)	(0.029)	(0.018)	(0.614)	(0.026)
One observation per household	0.031	0.282	−0.054	0.003	−0.267	−0.023
	(0.028)	(1.205)	(0.059)	(0.022)	(0.850)	(0.034)
No control variables	−0.006	0.649	−0.041	−0.025	0.618	−0.051
	(0.016)	(0.560)	(0.034)	(0.020)	(0.615)	(0.030)
Control group: non eligible mothers	−0.064	1.747	−0.017			
	(0.036)	(1.418)	(0.085)			
RD-DiD specification	−0.018	2.235	0.040			
	(0.055)	(2.056)	(0.074)			
Heterogeneous impact						
Higher educated	0.036	1.542**	−0.062	0.013	0.674	−0.019
	(0.020)	(0.639)	(0.035)	(0.024)	(0.713)	(0.031)
Lower educated	−0.021	−1.547	0.053	−0.041	0.839	0.047
	(0.023)	(1.169)	(0.050)	(0.029)	(1.235)	(0.045)
Married	−0.003	0.641	−0.054	0.007	0.782	−0.030
	(0.018)	(0.660)	(0.036)	(0.024)	(0.767)	(0.035)
Single	0.069	−1.582	−0.074	−0.050	−2.849	−0.114
	(0.054)	(3.378)	(0.092)	(0.066)	(3.262)	(0.106)

Significance levels reported at 5% level **, 1% level ***. Higher educated means high-school graduate and above

coefficients are concentrated below zero. This is a reflection of the large negative impact of the policy on the subgroup of first-time mothers with a child younger than one.

Along the intensive margin of work decision (Fig. 13), the common trend assumption in the years prior to the reform seems to generally hold. Figure 14 presents the coefficients and the 95% confidence interval of the yearly indicators interacted with the treatment dummy for the intensive margin of work. The yearly indicators for the treatment group are not significantly different from zero.

Regarding hourly wages (Fig. 15), the common trend assumption in the years prior to the reform seems to broadly hold well. Figure 16 presents the coefficients and

Table 11 Results for mothers of two children with youngest 1 to 3 years old

Mothers of two children 1–3 years old	Short-run			Long-run		
	Emp. rate	Working hours	ln(W)	Emp. rate	Working hours	ln(W)
N	27,229	16,268	7365	32,949	19,671	8449
2001–03 average	0.485	31.348	2.208	0.485	31.348	2.208
Policy impact	0.022	−0.631	−0.032	0.042***	−0.504	−0.031
	(0.013)	(0.439)	(0.022)	(0.015)	(0.453)	(0.017)
Robustness checks						
Local unemployment rate	0.020	−0.660	−0.034	0.041***	−0.515	−0.031
	(0.013)	(0.440)	(0.022)	(0.015)	(0.452)	(0.017)
One observation per household	0.038	−1.541**	0.060	0.039***	−0.605	−0.039**
	(0.033)	(0.772)	(0.038)	(0.014)	(0.467)	(0.019)
No control variables	0.003	−0.452	−0.018	0.033**	−0.415	−0.033
	(0.014)	(0.429)	(0.026)	(0.016)	(0.454)	(0.021)
Control group: non eligible mothers	0.028	1.515	−0.043			
	(0.018)	(0.605)	(0.037)			
RD-DiD specification	0.029	1.143	−0.084			
	(0.42)	(1.168)	(0.047)			
Heterogeneous impact						
Higher educated	0.031	0.121	−0.034	0.048**	0.625	−0.016
	(0.018)	(0.520)	(0.028)	(0.019)	(0.545)	(0.022)
Lower educated	−0.011	−1.792**	−0.001	−0.003	−1.514	−0.024
	(0.019)	(0.876)	(0.036)	(0.023)	(0.851)	(0.029)
Married	0.009	−1.027	−0.022	0.055***	−1.15**	−0.033
	(0.016)	(0.527)	(0.028)	(0.018)	(0.557)	(0.021)
Single	−0.015	−3.160	−0.115	−0.019	−2.018	−0.024
	(0.041)	(2.107)	(0.073)	(0.045)	(1.503)	(0.052)

Significance levels reported at 5% level **, 1% level ***. Higher educated means high-school graduate and above

the 95% confidence interval of the yearly indicators interacted with the treatment dummy for the logarithm of hourly wages. The yearly indicator for the treatment group are not significantly different from zero.[31]

[31] It should be noted that Lequien (2012) found long-term effects of the 1994 APE reform on mothers of two children's wages. The control group in the years 2001-03 would be composed of mothers who were not affected by the APE (those whose youngest child was born before 1994 and who would be 8 or 9 years old in 2001) and of mothers who were affected (those whose child was born in 1994 and later). After 2003, the control group is exclusively composed of mothers affected by the APE policy. Evidence presented here using the Labour Force Surveys point to limited risks of such a bias. Also, observable characteristics such as age, marital status, education and labour supply across the control group did not differ much whether mothers would have been affected nearly a decade earlier by the APE or not.

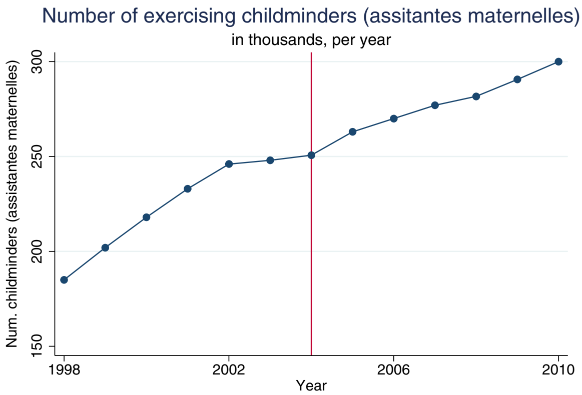

Fig. 10 Number of childminders (assistantes maternelles)

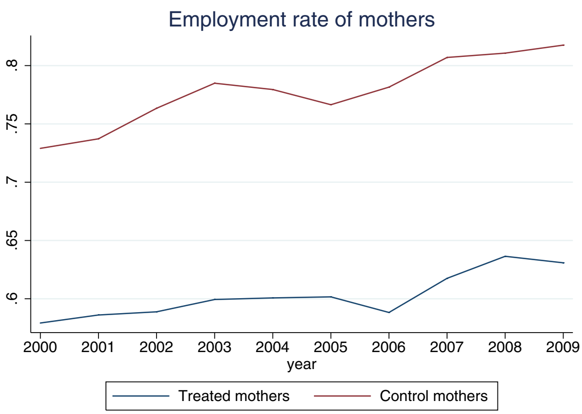

Fig. 11 Common trend for treatment and control groups, employment rate

7.6 Check on demographic characteristic trends: education, household composition and age

In the graphs below I check the share of mothers that at least graduated from high school (the higher educated group defined in the main text). The common trend on that covariate does not seem to hold perfectly, especially in the last year of observation. Note that after 2005, the educational attainment of the control group rises faster than that of the treated group. There is a likely cohort effect for mothers in the control group between those observed in the early 2000's that could have graduated from high school in the first half of the 1990's and those observed in the late 2000's that could have graduated from high school in the late 1990's. In this period, public

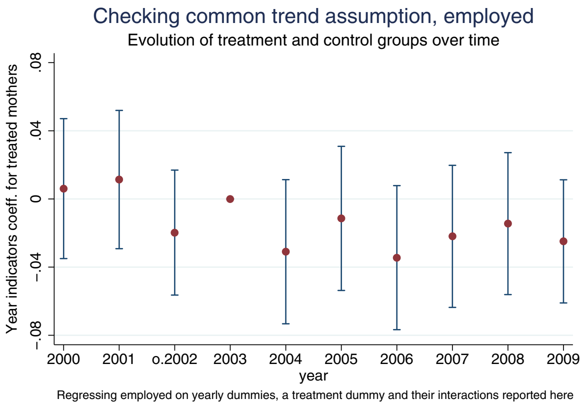

Fig. 12 Treated vs control group trend analysis, employment rate

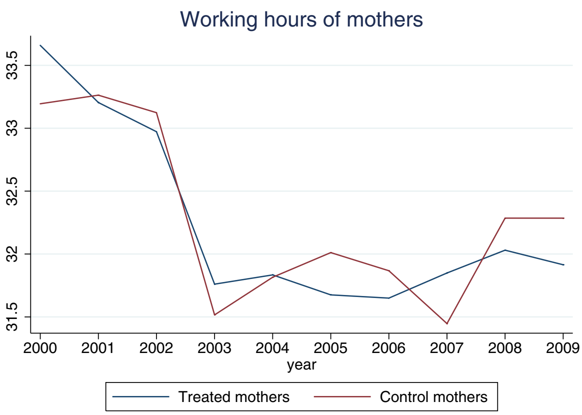

Fig. 13 Common trend for treatment and control groups, working hours

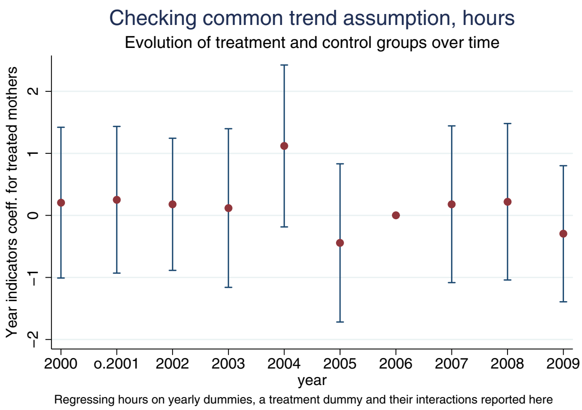

Fig. 14 Treated vs control group trend analysis, working hours

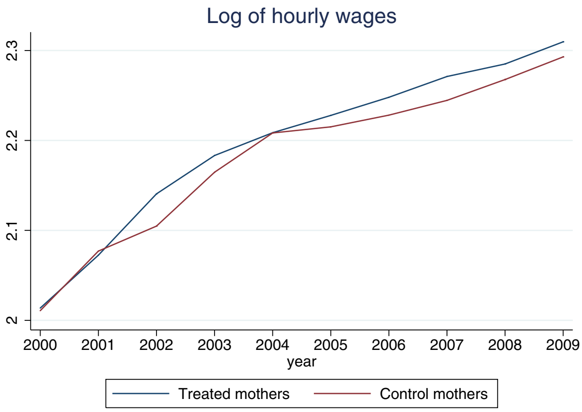

Fig. 15 Common trend for treatment and control groups, wages

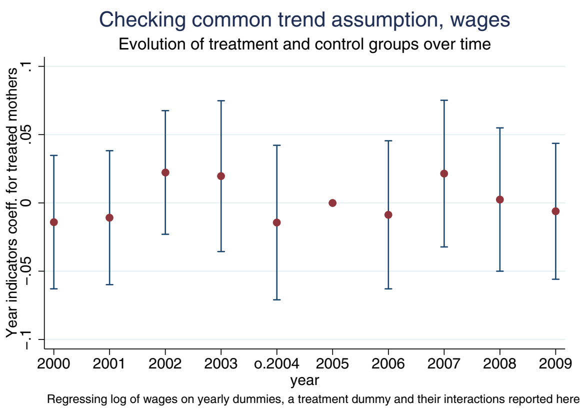

Fig. 16 Treated vs control group trend analysis, wages

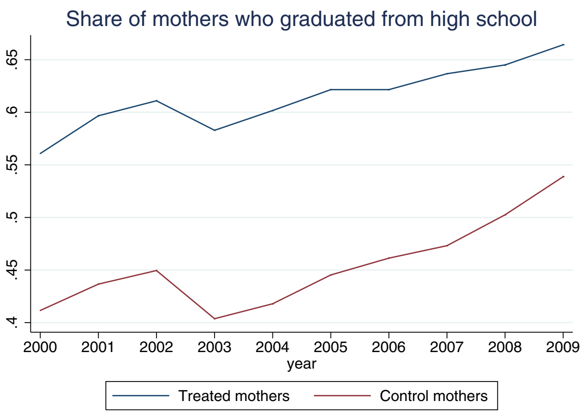

Fig. 17 Common trend for treatment and control groups, education

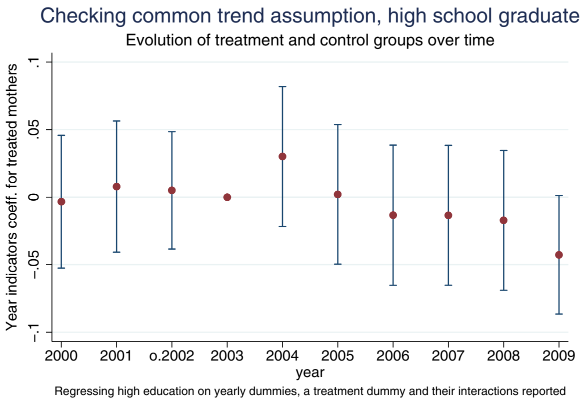

Fig. 18 Treated vs control group trend analysis, education

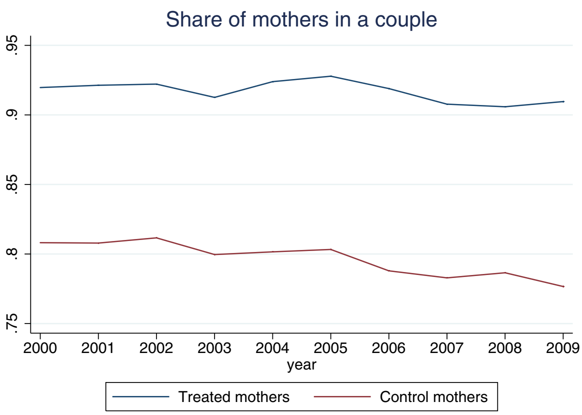

Fig. 19 Common trend for treatment and control groups, mothers in couple

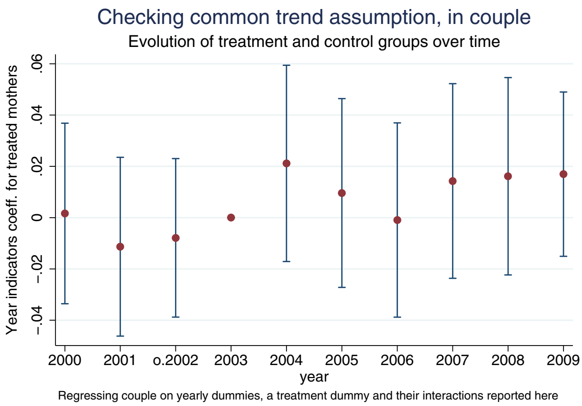

Fig. 20 Treated vs control group trend analysis, mothers in couple

policy was explicitly aiming at gradually increasing the educational attainment in the population (see Verdugo (2014)[32]) (Figs 17 and 18).

In the two figures below (Figs 19 and 20), I check that the reform had no impact on household formation and the share of mothers in a couple appears unaffected throughout the years of observation.

In the two figures below (Figs 21 and 22), I check that the reform had no impact on the age of the mothers. The average age of mothers in treatment and control groups seem to follow the same evolution.

7.7 Probabilities of working and using a childcare mode

I use the childcare surveys from the last section of the main text and run a simple difference regression (unfortunately no obvious control group exists as childcare choices mainly affect mothers of pre-school children). For each household category, I run the

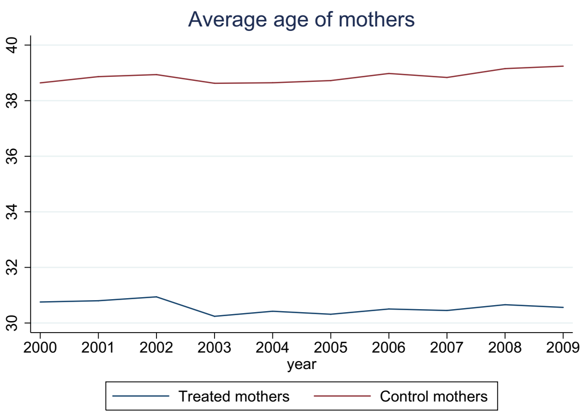

Fig. 21 Common trend for treatment and control groups, age of mothers

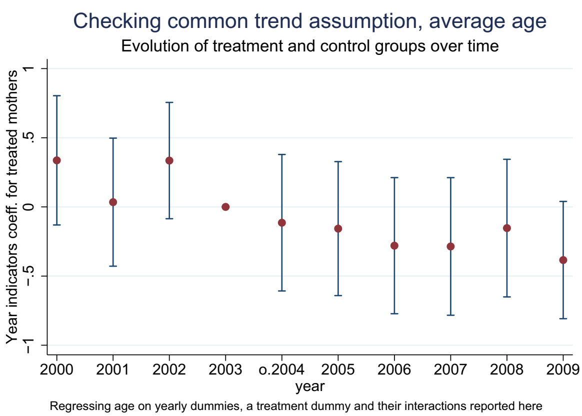

Fig. 22 Treated vs control group trend analysis, age of mothers

Table 12 Probability of employment and childcare choices post vs pre-reform

	1 child	2 children	1 child	2 children	1 child	2 children
	Pr (Work and Private carer)		Pr (Work and Kindergarten)		Pr (Work and Other)	
Youngest 0–1 year old	−0.013	−0.027	0.003	0.019	−0.036	0.0930**
	(0.047)	(0.033)	(0.033)	(0.022)	(0.052)	(0.041)
Youngest 1–3 years old	0.015	0.026	−0.0355*	0.0323**	0.0580**	0.0659**
	(0.025)	(0.022)	(0.019)	(0.015)	(0.027)	(0.026)
	Pr (No work and Private carer)		Pr (No work and Kindergarten)		Pr (No work and Other)	
Youngest 0–1 year old	0.0054	−0.0204***	−0.0265**	0.007	0.067	−0.067
	(0.0077)	(0.0071)	(0.011)	(0.007)	(0.047)	(0.044)
Youngest 1–3 years old	−0.0150**	0.0054	0.008	.0103**	−0.031	−0.1396***
	(0.0074)	(0.0044)	(0.005)	(0.004)	(0.021)	(0.026)

Significance levels reported at 10% level *, 5% level **, 1% level ***

following regression:

$$Pr(Work_i \& Childcare_{ij}) = \alpha + \gamma' Post_i + \beta' X_i + e_i,$$

where $Childcare_{ij}$, represents the main childcare mode j used by household i. *Post* is a dummy equal to 1 if the observation was after the policy change, and X_i is a vector of controls (education, age of the mother and its square, the age in months of the youngest child, as well as a dummy if married and in couple). I look at three main modes of care: private carers (childminders and nannies) that was affected by the reform, kindergarten (creches) and other. The estimates of the γ parameter above are presented in Table 12.

From this exercise, we can conclude the following:

(1) A significant fall in the probability of using kindergarten for mothers of one child and an increase for mothers of two children, independent of their work situation.
(2) A significant fall in the probability of using private carers while not working induced by the modification in eligibility rules to subsidies (now available exclusively to working mothers).
(3) For mothers with a child aged 1 to 3 years old (irrespective of the number of children), the probability of working and using other modes significantly rose. The probability of working and using private carers rose (although the impact is not statistically significant and bigger for mothers of two children).

7.8 Labour supply of fathers

In the figures below (Figs 23–26), I report the employment rate of fathers observed in the Labour Force Surveys by the year of birth of the youngest child in the household. The graphs highlight that there was no clear impact of the reform.

Similar graphs representing the fathers' intensive margin of work show no reaction to the reform and are not presented here. They are available upon request.

[32] In 1985, the French government set the objective that 80% of each 18 years old cohort would graduate from high-school by 2000. The objective was eventually reached in 2012.

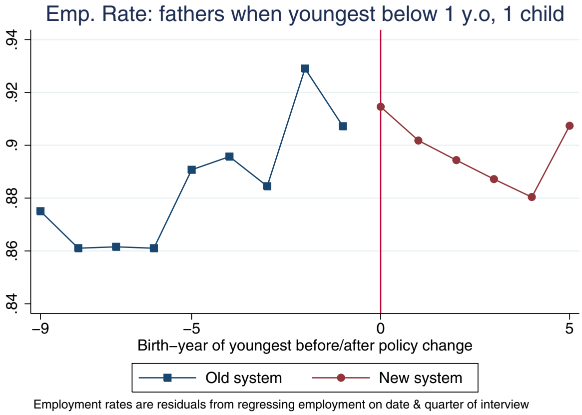

Fig. 23 Employment rates for fathers of one child (youngest under 1 year old)

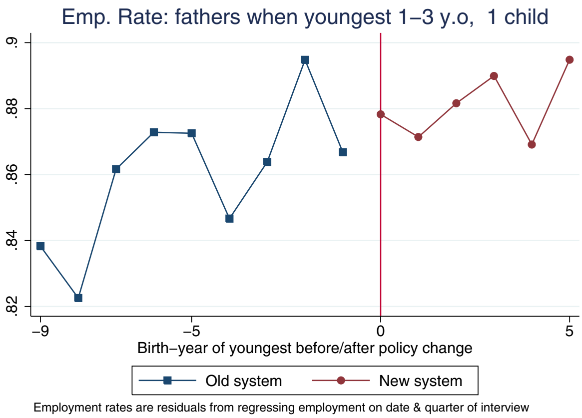

Fig. 24 Employment rates for fathers of one child (youngest 1 to 3 years old)

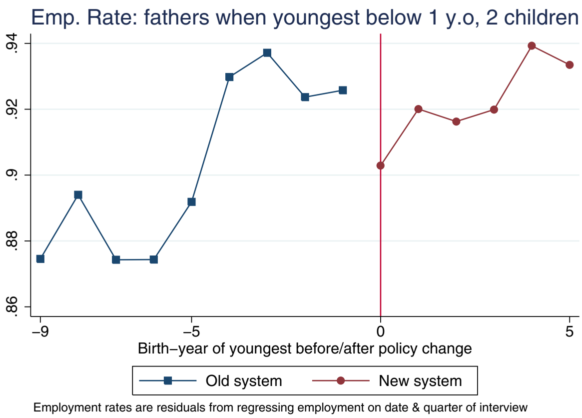

Fig. 25 Employment rates for fathers of two children (youngest under 1 year old)

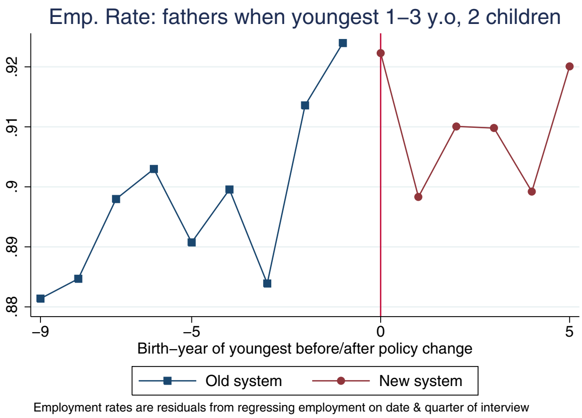

Fig. 26 Employment rates for fathers of two children (1 to 3 years old)

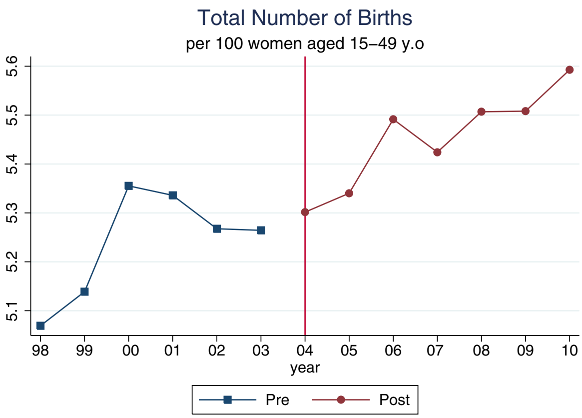

Fig. 27 Number of births per 100 women aged 15 to 49

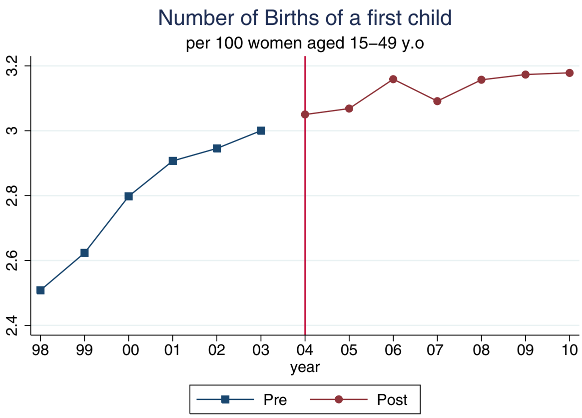

Fig. 28 Number of first births per 100 women aged 15 to 49

7.9 Fertility trends

Piketty (2005) estimates a small impact of a much longer parental policy change on fertility in France in the mid to late 1990's. The data available does not allow me to study the response of natality choices to the policy with precision. I rely on the population statistics from the INSEE (National Institute of Statistics and Economic Studies) to construct the indicators reported in this section. The number of children born per 100 women aged 15 to 49 has been constantly rising between 1998 and 2010 in France (as shown in Fig. 27). This is particularly true for the number of first births per woman (Fig. 28). The parental leave subsidies that now became available to this group of women do not appear to have had any impact on their decision to start a family. The difference with the results in Piketty (2005) could be due to the much shorter duration of the program (6 months here versus 3 years in Piketty (2005)). Also, Joseph et al. (2013) and Givord and Marbot (2015) provided similar checks that the 2004 policy change did not impact fertility in the short-run at least.

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