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Mind the employment gap: an impact evaluation of the Czech "multi-speed" parental benefit reform

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Abstract

Parental leave is a key policy tool for addressing work-life reconciliation issues inherent to parenthood, including maternal employment and its continuity. The 2004 Czech accession to the EU shed light on the scope of the employment gap between women with and without children at pre-school age, highest among all the OECD countries (41 pp). This is due to very long universal paid parental leave: 4 years per child. In order to tackle this gap and to conform to the EU trend, a major reform was designed in 2008, and this paper investigates its effects on mothers' participation and employment. I use the Labour Force Survey to assess the effect of this reform on maternal employment and activity levels, thanks to a difference-in-differences identification strategy. The reform provided an extensive change in financial incentives in favour of shorter leaves, and I show that effects on return-to-work timing are large and significant. However, if mothers do respond to the incentive by advancing the timing of the return to work by one year, the eligibility restrictions as well as the public childcare shortage narrow - de facto - the scope of the effect, which merely compensates for the massive opposite trend induced in the 1990s.

JEL Code: J16, J18, P30

Key words: Policy Evaluation, Female Labour Force Participation, Parental Leave, European Social Integration

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1. Introduction

Over the last decades, female labour supply has garnered explicit political interest at the European Union level, and the 2004 Czech accession to the EU shed light on the scope of the employment gap between women with and without children at pre-school age, highest among all the OECD countries (41 percentage points). This is explained by very long universal paid parental leave: 4 years per child. In order to tackle this gap and to conform to the EU trend, a major reform was designed in 2008, and this paper investigates its effects on mothers' participation and employment rates. The reform's objective was to shorten the duration of paid leave by introducing better paid shorter tracks, and I show that the share of mothers in employment shortly after three years of parental leave (PL) increased significantly in both instantaneous and medium terms.

In the economic literature, the theoretical and empirical links between family policy and work-life conciliation have been largely discussed in Western European countries. Although fertility is heavy with implications on maternal labour supply at the individual level (Angrist and Evans, 1998), family policies have intervened in most developed countries, to lower the cost of children and to encompass motherhood-related transitions out of and back to employment. At the macro level, fertility and employment no longer compete (Ahn and Mira, 2002) and adapted family policy schemes are acknowledged as efficient tools in promoting both fertility and female employment. Blau and Kahn (2013) even argue that from an international perspective, the presence or absence of work-family conciliation policies is a decisive determinant of female labour market participation levels, and the lack of family policies in the USA explains why it has been recently "falling behind" other OECD countries. In a study of 16 European countries over the 1970s and the 1980s, Ruhm (1998) exploited changes in legislation to estimate the effect of parental leaves on labour market outcomes in difference-in-differences and triple difference settings. He shows that PL schemes positively affect female employment, as even short job protection (less than 3 months) increases women's job continuity. The results indicate that although leaves have a positive effect, durations longer than 6 months might hinder the progression of wages and decrease the relative wage with respect to men. Thevenon and Solaz (2013) specify that the positive relation between PL and female employment holds for countries with a PL duration up to 2 years. After this threshold, both employment rates and hours worked are negatively affected.

Case studies across OECD countries are abundant[2], and have confirmed the central role played by parental leave and childcare. In the continuity of Ruhm's approach, scholars have focused on family policy reforms and have turned to quasi-experimental designs as the purest approximation of randomisations, and this allowed them to apply rigorous impact evaluation techniques (instrumental variables, difference-in-differences, regression discontinuity design, matching and propensity scores). In countries with relatively small scale PL schemes and short leave durations, like the USA or Canada, there is strong evidence that parental leaves correlate positively to female employment[3]. Similarly, Olivier et al. (2013) analyse the introduction of a shorter and better paid full-time leave in France in 2004. They conclude that such short leave has no negative effect on further employment and wages – as opposed to negative impact on wages of the longer leave – and reduces the probability of becoming inactive for low-educated mothers.

Indeed, the results are quite different if we consider the literature which relates to relatively long schemes, like the ones we observe in the CEE region. In France, Piketty (2005) analyses the impact of a 2-year PL eligibility extension on mothers' fertility and employment rates, while Moschion (2010) analyses the impact on the interaction between fertility and labour supply. They both conclude that longer leaves negatively impact eligible mothers' post PL labour market outcomes. A very useful distinction between the incentives provided by job protection and cash transfer parts of PL schemes has been introduced in the literature on relatively long leaves. Drawing on their previous work on Austrian PL reforms (Lalive and Zweimuller, 2009), Lalive et al. (2014) assess the respective impact of each component of the PL scheme, i.e. cash transfers and job protection, thanks to a non-stationary search model and an empirical analysis on the Austrian Social Security Dataset. They demonstrate that the best way to increase maternal time at home with low cost in terms of further employment is to provide the same duration of both job protection and benefits. Schonberg and Ludsteck (2014) apply a similar analysis to a series of 5 reforms in Germany, and argue that long leaves fail to further increase maternal employment, and are costly for mothers in terms of subsequent wages. That is due to the length of the leave itself, a human capital depreciation and a lower quality of match of the job the mothers are assigned when they return to the previous employer after long leaves. In short, statistical evidence from old OECD countries shows that PL increases job continuity by

[2] A recent *Labour Economics* issue (2015) has even dedicated a special section to family policy evaluation, and comprises the cases of France, Germany, Spain, the Netherlands, Canada and Japan.
[3] See Waldfogel (1999) and Baum and Ruhm (2014) for the USA, and Baker and Miligan (2008) for Canada.

providing a guaranteed return-to-work after the end of the PL in case of leaves up to one year, while longer leaves increase the time that mothers spend with their children but come with a significant cost for further employment (Rossin-Slater, forthcoming).

As for family policies in Central and Eastern Europe (CEE), the economic literature is surprisingly scarce. Local policies were typically very comprehensive, as a legacy of interventionist social and family policy schemes under the state socialist regime. As I illustrate in this paper, the recent legislative changes in CEE are large in scale and therefore more lessons can be drawn about the incentives provided by parental leaves. In this way, the paper can add to the existing literature and contribute to our understanding of the effects of parental leave policies on labour market outcomes of mothers. Yet as far as we know, very few empirical family policy evaluations have been conducted in the post-transitional CEE (Lovasz and Szabo-Morvai, 2015; Balint and Kollo, 2008), and only two in the Czech Republic (Mullerova, 2014; Bicakova and Kaliskova, 2016). Lovasz and Szabo-Morvai (2014) use the Hungarian Labour Force Survey to estimate the effect of childcare availability on maternal post-birth activity, in a RDD-like setting. They do find a significant effect, but its low magnitude brings them to underlining alternative explanatory factors of maternal activity, such as the importance of cultural norms on the child's age of transition from maternal to collective childcare, called the "separation preferences". Balint and Kollo (2008) also use the LFS to estimate the probability of exit to employment before and after parental leave reforms, and conclude that the long leaves implemented in Hungary hinder mothers' further prospects on the labour market, mostly if they have low labour market attachment. The authors also underline the lack of evaluations of family policies in the CEE region despite their scales and costs, and the comment still holds today. In Mullerova (2014), I showed that the extension of parental benefits beyond the standard job-protected leave in 1995 led to a significant and durable fall in post-PL maternal employment rates in the Czech Republic. This finding is in line with the German and Austrian studies: it confirms that the scope of the paid leave (weeks, months, years in this case), as well as the respective lengths of job protection and cash transfer, yield heterogeneous incentives. Bicakova and Kaliskova (2016) analyse the effects of the 1995 and 2008 reforms on non-employment and on exits to unemployment and inactivity, and corroborate, on a different sample, the finding in Mullerova (2014).

In the Czech Republic, during most of the transitional period, the payment of universal flat-rate parental benefits covered four years per mother (48 months), i.e. one more year than the three-year job-protected leave (36 months). In 2008, a major reform restructured the benefit, offering financially incentivised shorter tracks. Aimed at shortening the long paid parental leave

established in 1995 and at reducing the incumbent public expenditures, the 2008 reform of PL set up three possible durations of the cash transfer, with roughly the same total amount distributed over all the leaves. In this paper, I evaluate the effects of this multi-speed reform on maternal employment and activity levels. The reform provided an extensive change in financial incentives in favour of shorter leaves, and I show that the effects on return-to-work timing are large and significant. To this purpose, I employ the difference-in-differences identification strategy, using two approaches: a cohort effect analysis over both the period of interest and a control period, and a standard difference-in-differences setting with the counterfactual group composed of mothers with older children. Alongside the contemporaneous effect of the reform on the first treated cohorts, I consider medium run effects and show that the reform significantly hastened the transition to employment and maintained the higher employment rates over years following the treatment.

The paper is organised as follows. Section 2 is dedicated to the institutional background of conciliation policies during the transition from planned towards market economy (2.1.), with a focus on the 2008 multi-speed reform (2.2.). Section 3 presents the data and the empirical strategy. Results are reported in Section 4, and Section 5 offers a discussion of the results.

2. Institutional background

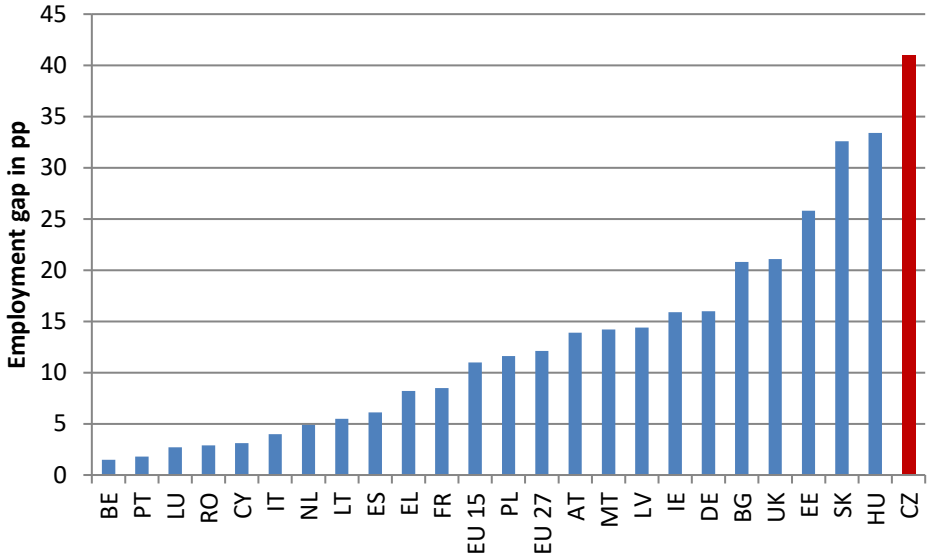
2.1. Work-life conciliation in the transition and EU accession

The fall of "Iron Curtain" and the following transition fundamentally changed the institutional context of work-family reconciliation[4]. The previously state-controlled labour market was restructured and unemployment emerged (Svejnar, 1999). At the same time, the management of public expenditure called for less interventionist family policy and more market-based solutions to the childcare issue. There were extensive cuts to and a loss of interest in public childcare: while the supply of kindergartens decreased in line with the fertility trend, the decline in nurseries was sharper. In contrast with more than 1,000 nurseries (40,000 places) in 1990, only 60 nurseries (1,800 places) remained ten years later (Kucharova et al., 2009). The evolution of public childcare illustrates what Potucek (2001, p.201) calls "ideologically induced animosity towards the institutions and policies of the welfare state". Post-transition family policy evolved in a pro-reform climate where individual responsibility was promoted as an alternative to the former state paternalism (Vecernik, 1993). Compared with the pre-transition

[4] For a summary table of family policy reforms throughout the 20th century, see table A in Appendix.

era, family policy formulated no explicit interest in either female labour supply or fertility (Sobotka et al., 2008). Female labour supply by age acquired a strongly M-shaped pattern, as female participation is high on average but increasingly low in reproductive age. The maternity-related employment gap, i.e. the difference in employment level between women with and without pre-school children, has become the highest among all the OECD countries (41%).

Chart 1 Employment gap between women without and with children under the age of 6



Source: EU Labour Force Survey, in European Commission Indicators for monitoring the Employment Guidelines (2010)

As a result, new work-family balance arrangements emerged. In this context of economic uncertainty and increased cost of children, Czech women postponed or rejected motherhood, which is reflected in an unprecedented drop in the fertility rate in the 1990s (1.13 in 1999 (CZSO, 2012)). At the same time, mothers of pre-school children gradually withdrew from the labour market, increasing the motherhood-related employment gap. In 1990, paid PL was extended to 3 years[5] for all mothers, with no other condition than the youngest child's age. Given the rather non-interventionist political climate, this generous change in the PL scheme might appear paradoxical, but it can be seen as an attempt to relieve labour market pressures and promote social peace. Therefore, in spite of the liberal discourse and contrary to international trends in increasing female employment, the PL scheme pursued a conservative

[5] Benefit paid until the child's 3rd birthday, flat rate: 900Kc per month, i.e. 25% of the average monthly gross wage.

target in terms of gender division of labour, rather than aiming to increase female labour market participation. In 1995, the payment of parental benefits was extended from 3 to 4 years to all mothers unconditionally, inducing a massive postponement of returns to work, added to the already long maternal career interruption. Four-year inactivity therefore became the norm, with over 70% of mothers staying at home over the 3rd or the 4th year (Haskova, 2011), until the 2008 reform which will be described in the next sub-section.

During the EU integration process, the public discourse about conciliation policies - heretofore practically absent - took on a new perspective, which was gender equality and equal opportunities in the labour market, in line with the EU non-discrimination law and the Council directive 2000 (Filipova and Pytlikova, 2016). Czech decision-makers have received recommendations from the European Commission which asserts that "a key challenge concerns the severe difficulties that women with children face when re-integrating into the labour market after maternity leave" (EC 2012, p.4). This recommendation concerns an insufficient supply of institutional day care services, as well as the mismatch between their opening hours and most of the parents' full-time working hours. Yet post-transitional legislation has given priority to more generous conditions of PL and thus a longer withdrawal from labour, with less emphasis on providing child care services for children under 3. While the EU announced in 2002 the Barcelona targets, aiming to provide by 2010 public childcare supply for 90% of children between 3 and 6 and for 33% for children between 0 and 3, the Czech representatives have been opposed to these objectives and assert that had the Central European region been part of the EU in 2002, these objectives would not have been targeted.[6] Indeed family related values remain conservative: although state socialism strongly promoted full time employment and it was widely implemented, Czech households did not abandon the traditional gender division of tasks and the predominant view that women's responsibilities hold primarily inside the household and in child-rearing. The 2002 ISSP survey shows that half of the adult population, for both males (53,9%) and females (47,8%), agree with the statement that a child under school age is likely to suffer if their mother works (Chaloupkova and Salamounova, 2004). It is an expression of a "strong normative support to parenthood" and to long maternal care (Sobotka, 2015).

As to the financial support during PL, in the 2000s a series of amendments[7] had loosened the conditions of access to parental benefits, towards a higher labour market participation of the

[6] Said by Petr Necas, the minister of Labour and Social Affairs in 2009, at the occasion of Czech EU presidency. See the speech at http://www.vlada.cz/cz/media-centrum/aktualne/barcelonske-cile---projev-ministra-p--necase-53292/

[7] In 2001, 2004, 2006 and 2012, loosening the authorised maximum worked hours and income for the recipient and the maximum number of hours spent in childcare for the recipient's child.

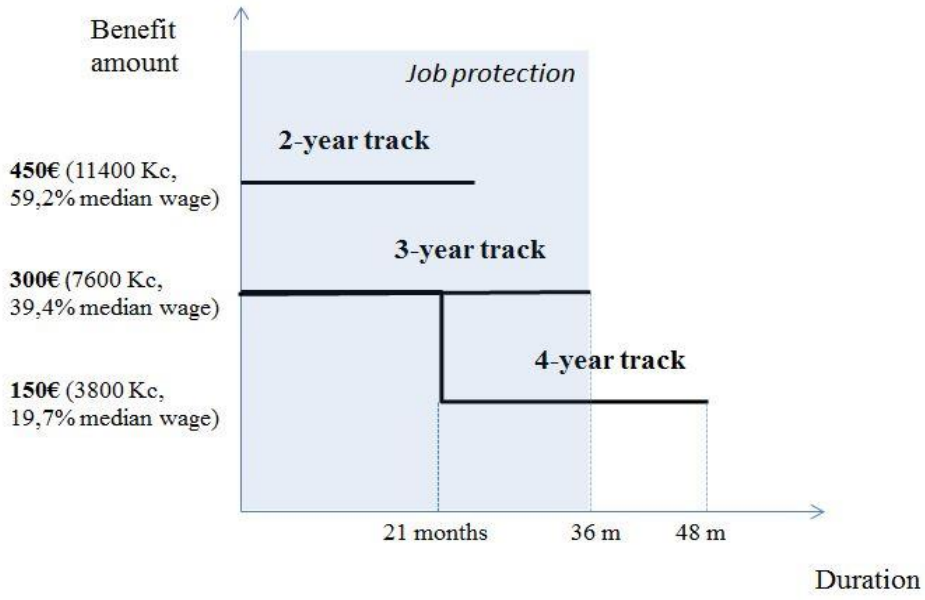
recipients, as part of the EU accession process. The multi-speed parental benefit reform is the epitome of this family policy trajectory.

2.2 The multi-speed parental benefit: rationale and mechanism

The 2008 reform brought a key change by establishing a "multi-speed" parental benefit system, which leaves to mothers the choice of receiving benefits during 2, 3 or 4 years. This reform was part of a broader reform implemented by the new right-wing conservative government: the bill on stabilisation of public finances, aiming at reducing public expenditures. The argument used is that the Czech PL scheme is too long and generous in international comparison, and must be modernised in order to fit the European Union standards and to become financially sustainable for the public budget (*Prace a socialni politika*, 2007). The financial incentive in favour of shorter leave lies in the fact that, from now on, roughly the same total amount of benefits is being distributed independently of the duration of the payment; shorter duration goes with higher monthly amounts, while longer duration goes with lower monthly amounts.

More specifically, the reform introduces, in its regular setting, three different tracks with eligibility conditions based on mothers' employment histories and previous wages. Each track (2-year, 3-year and 4-year) is associated with a monthly cash transfer so that, over the whole period, roughly the same total amount is distributed to all recipients.

Chart 2 The "multi-speed" parental benefit reform design



The choice of tracks is conditioned on previous employment: only women who had been employed before the leave (thus excluding students, the unemployed, the inactive, or those on leave with a previous child) can opt for one of the two shorter tracks. For mothers with previous employment, universal parental benefits are preceded by a 28-weeks insurance-based maternity leave. Therefore, mothers eligible for maternity leave are then eligible for shorter PL tracks; on the other hand, mothers who are ineligible for maternity leave and who enter parental leave directly after giving birth are automatically assigned the longest track. After the maternity leave eligibility criteria (i.e. pre-birth employment criteria), which identifies mothers who can/cannot choose one of the shorter tracks, an income criterion distinguishes between mothers who can shorten the leave by one year (i.e. eligible for the 3-year track) and who can shorten the leave by one *or* two years (i.e. eligible for both the 3-year and the 2-year track). Only mothers who had a pre-birth employment with sufficiently high wages[8] are entitled to the 2-year track. This restriction is presented as tool that allows a fast transition to employment for mothers with strong labour market attachment, while preventing low-income beneficiaries from shortening their leave as a transition to other types of welfare support. The 3-year track is less restrictive, as the eligibility relies on pre-birth employment but not on the level of income. This track has the specific feature of re-uniting the duration of cash transfer with the job protection (let us recall that from 1995 to 2007, the benefits were paid over four years while the job protection covered only three years). This track keeps the monthly amount similar to the previous regime, while shortening the duration by one year. Finally, the 4-year track resembles most closely the previous regime in terms of benefit duration and that it outlasts the job-protected period. However, maintaining the benefit duration comes with the price of a significantly lower monthly amount: it is cut in half at month 21 and all the way through until month 46. As mentioned before, this track is accessible to all mothers; it is also the default track to which a mother is assigned if she does not request otherwise.

[8] Their monthly wage, or the wage of their partner, must be higher than 11 400 Kc.

Table 1 The 2008 Reform design

Reform design: standard setting			
	Track eligibility		
	4-year	3-year	2-year
Pre-birth LM situation			
Inactive (student, previous PL)	✓		
Employed	✓	✓	
Employed, wage > 16400 Kc/month	✓	✓	✓

We can perceive here the incentive structure, most importantly between the 3-year and the 4-year tracks[9]. Eligible mothers face the following trade-off: either the former duration is maintained, with a perceptibly lower monthly transfer, or the same amount is maintained, over a shortened period of time, which happens to coincide with the job protection and with the widely accepted and promoted social norm in favour of a three-year personal maternal care (Krizkova et al., 2011, Haskova and Uhde, 2009). Compared to the pre-reform setting, the new design penalises the long track and incentivises the medium track, as the same total amount is distributed and the loss of job protection is no longer financially compensated.

The complexity of the reform goes further: the setting described above only applies to future recipients, i.e. to mothers whose youngest child is yet to be born at the reform implementation date, on January 1st 2008. But the reform impacts also all the current recipients, in an incremental way based on age thresholds. This transitional regime is designed as a way to accelerate the public expenditure cut by generalizing the new stricter rules (shorter duration if the same monthly amount as pre-reform; lower monthly amount if the same leave duration as pre-reform) as quickly as possible to all current recipients. This differential treatment essentially mimics the standard treatment and eligibility criteria, while adding age thresholds for slightly modified track options: if a child is older than 3, for instance, then the mother is in the final (4th) year of benefits and no options are available. In this case, the benefit was simply cut in half immediately upon the reform implementation, in January 2008. If a child is younger than 3 but older than 21 months (i.e. moment of cut of benefits in half in the standard setting, see the Chart 2 above), there is logically still no option available for shorter tracks, as the 21-

[9] As to the 2-year track, its predictable importance in the short run is marginal, given the low incentive (the job protection runs for 12 extra months), the extremely low supply of public childcare for 2 year olds (kindergartens typically accept children from age 3, and even for them there is a notable shortage of available places) and least but not last the previously mentioned social norms.

month crossroad is passed. All the mothers are assigned the 4-year track, with the cut in half in benefits being postponed until the child's 3rd birthday: therefore, this cohort still enjoys a slightly more generous 4-year track than in the standard treatment. Mothers of children from yet a younger cohort, aged 5 to 21 months at the reform date, are the first to face the standard option between the 4-year and the 3-year track. According to their choice, they will either receive a lower rate starting from age 21 months (4-year track) or keep the previous amount and exit the leave at the child's 3rd birthday (3-year track). This cohort is notable for it is the first to be treated in the general spirit of the reform, i.e. with an actual option. Only the 2-year track is absent for this cohort: it is available only to mothers with children who are younger than 5 months on January 2008. Indeed, this option must be selected before the end of the 28-week maternity leave, as the mother will then receive a higher monthly amount, from the end of maternity (approx. 5 months) until the child's 2nd birthday. This youngest cohort is therefore eligible for all the three tracks designed in the general setting (if, simultaneously, pre-birth employment and income conditions are met, of course). This transitional regime is summed up in the following table.

Table 2 The 2008 Reform design

Reform design: transitional setting			
	Track eligibility		
	4-year	3-year	2-year
Child's age at 1/1/2008			
Older than 36 months	✓ Treatment: monthly benefit cut in half immediately		
21 to 36 months	✓ Treatment: monthly benefit cut in half at month 36 instead of month 21 (postponement)		
5 to 21 months	✓	✓	
Younger than 5 months	✓	✓	✓

The standard design of the reform, applied to women whose youngest child is born after January 2008, produces labour market outcomes which are only observable from 2010 onwards for the shortest track, from 2011 onwards for the medium track, and from 2012 onwards for the longest track. The progressive application to all current recipients at the same time hinders simple identification strategies (no clear control cohort precedes mothers who give birth under

the new regime), but also offers opportunities to observe more immediate effects of the reform on mothers who are treated under the transitional regime, as we will see in the following section.

3. Data and Empirical strategy

3.1. Data

The dataset used is the Czech Labour Force Survey (LFS), gathered by the Czech Statistical Office on a quarterly basis starting from December 1992. Each quarter records approximately 70,000 individuals, and collects rich information about the socio-economic profile of each member of a household. The survey is representative of the Czech population via an individual weighting system. The LFS is a rotating panel, where each household remains in the sample for 5 consecutive quarters. The data are collected on a declarative basis, and provide a large battery of variables related to one's status in the labour market in the current quarter (more specifically in the reference week). I use 6 then 12 quarters before and after the reform date for the sake of estimation (2006-2009), and I exploit the panel structure for the construction of our sample. In doing so, I shortlist mothers who were present in the survey around the moment when their child reached 36 months, i.e. before and after the child's 3rd birthday. This age is a crucial threshold in parental benefits entitlement, as it marks the end of the job-protected leave as well as the end of cash transfers for mothers who chose the three-year track. The data have not been collected with the aim of analysing work-life conciliation, as they are focused primarily on employment, but they are rich enough to be exploited from this angle, and they also convey comparable employment and activity information over the years 2000's. Among the major drawbacks of the data, the panel rotation does not allow us to observe the same individuals over more than five quarters. Regular panel data would have allowed us to exploit mothers' economic status history and have proper knowledge of its evolution during and after the PL at an individual level. Secondly, the LFS records no information about income. Yet it would have been convenient to take into account wages and benefits, and, most of all, it would have been interesting to assess the effects of incitation to longer withdrawal from the labour market on mothers' earnings.

As for the sample construction, mothers are identified in an indirect way in the LFS. I select women who are between 20 and 39 years of age. The upper bound is high enough for identifying mothers whose child is younger than four, and it allows us to minimise the risk of confusion between mothers and grandmothers in the household. The age of the child is given, unlike the

date of birth. Therefore, I isolate mothers at the end of the PL duration (36 months) by the child's transition from age 2 to age 3 between two consecutive quarters. I identify the quarter in which the child becomes aged 3 compared to the previous record where he is aged 2, and keep only mothers for whom we observe these two successive records in the data. That is how I construct a "transition" variable, which signals that the child passed from 2 to 3 years of age, meaning the mother exhausted her PL entitlements[10]. Therefore, the construction of the sample is restrictive and drops many individuals. Inside the considered period, we lose mothers who enter the survey after the transition or who left the survey before the transition; at both bounds of the considered period we lose those who experience the transition before or after the quarters used for the estimation. This restrictive method allows me to gain precision on the reform's effects in a clean quasi-experimental setting shortly after the reform implementation, with no possible anticipation of fertility or pre-PL employment choices by the treated. An alternative approach consists in considering a much larger sample over several years before and after the reform, increasing the sample size and compensating for lower precision by a broader reach of heterogeneity analysis. This is the choice made by Bicakova and Kaliskova (2016).

Despite the restrictions, the large size of the dataset allows me to constitute a sample of 1217 mothers, representative of 150 000 individuals on a national scale. Tables B1 to B3 in Appendix represent the summary statistics of the sample as well as broader contextual statistics of Czech women around the reform dates.

3.2. Empirical strategy

I focus on employment levels as indicators of having or not exited the parental benefit scheme. Indeed, receiving the benefits is conditioned on the mother's personal care of the child: kindergarten is only allowed to a very limited extent (five days per month). If, from 2004 onwards, there is *de jure* no limit on income and worked hours while on benefits, employment is *de facto* incompatible with benefits through the fact that employment is almost exclusively full-time. Full time working hours are incompatible with the limited authorised public childcare hours, and childcare facilities systematically give priority to full time children for organizational reasons, in the context of shortage of available places. Therefore, the employment rate is used as a primary outcome variable for estimating the length of PL and its

[10] It is at that quarter that I observe the employment status outcome.

change after the 2008 treatment. The method used is the difference-in-differences estimation, comparing the evolution of the employment rate within the eligible cohort before and after the intervention date with that of a different, non-eligible cohort. This identification strategy relies on a comparison of two cohorts in the same population of mothers with the exception that one cohort was exposed to the reform while the other was not[11]. This approach is inspired by studies on Germany and Austria (Schönberg and Ludsteck, 2014; Lalive and Zweimüller, 2009). I estimate a linear probability model, corrected for heteroscedasticity, and the estimated equation can be written as:

$$y_i = \alpha_0 + \alpha_1 Treat_i + \alpha_2 After_i + \alpha_3 Treat_i * After_i + \alpha_4 X' + u_i$$

Where *yi* is the outcome variable of the individual *i*, i.e. to be employed or not (alternatively, to be active or not), *Treat* is the dummy for the cohort before and after the reform date (eligible cohort). It is equal to 1 if mother *i* belongs to the treated cohort and it accounts for average permanent differences between treated and control. *After* is the before/after time dummy, it is equal to 1 if mother *i* is observed "after" the programme, and it accounts for the time trend common to control and treatment groups. Finally, α_3, preceding the interaction term *Treat*After*, is our parameter of interest. The covariates include individual characteristics such as age, education, marital status and number of children.

The choice of the main treated group (and, as importantly, of the main control group) stems from the peculiar incremental design of the treatment. As described above in the section 2.2, the reform introduced a set of PL tracks for future recipients, as well as an adapted differentiated treatment for all current recipients. The very first cohort to be exposed to the critical choice between 3 and 4 years is to be found among the latter, i.e. among mothers who are already on PL by January 1st 2008. Namely, it concerns the cohort of mothers whose youngest child is younger than 21 months at that date (otherwise no option would be available, only an ensuing reduction of the monthly benefit), and older than 5 months. Focusing on this specific cohort guarantees unambiguous eligibility to the programme. Let us recall that the child's age on 01/01/2008 is the decisive criteria for their identification in the data, yet it is *not* on 01/01/2008 that the outcome is observed. Indeed, the choice between 3 and 4 years of PL becomes effective and observable in the data after the "crossroad", i.e. once the child turns 3. At this precise

[11] The control cohort was not exposed to the reform, or to any other exogenous shock that might have affected the outcome variable, such as another legislative change, a sudden change in childcare supply, or other politico-economic factors.

moment, the job-protected PL is over, and the mother either just exhausted the 3-year benefits, or will remain inactive while on the last year of the 4-year benefits. The child's 3rd birthday is therefore the moment where we effectively observe the employment status of the treated group. Given the cohort's characteristics (months 5 to 21 on 1/1/2008), the 3rd birthday occurs in 2009/2010, more specifically in the two last quarters of 2009 and the two first quarters of 2010. Choosing the first genuinely treated cohort also guarantees that the identifying assumptions of a quasi-experiment be respected: neither fertility nor pre-birth behaviours can be adapted in anticipation of the eligibility criteria, as the youngest child was born before the reform was implemented.

As to the control group, the reform's design is such that the outcome of this treated cohort cannot be directly compared to the adjacent observations in discontinuity approaches, since slightly younger and older cohorts only receive a different kind of treatment instead of not being treated. Consequently, for this cohort effect analysis, a proper control group (i.e. which has not been subject to any treatment relative to the 2008 reform) is to be found among mothers with a similar advancement in PL entitlement (youngest child's 3rd birthday), but at a different point in time. Therefore, I consider mothers who exited both parental leave and parental benefits in 2007 (i.e. before the reform implementation, meaning that they are strictly unaffected by the reform), which means that their youngest child turned 3 in 2006. It is worth noting that as this cohort's children turned 3 in 2006: their labour market outcomes at that time not only avoid the 2008 reform effect, but also a possible effect of the 2007 reform which temporarily raised the benefit amount.

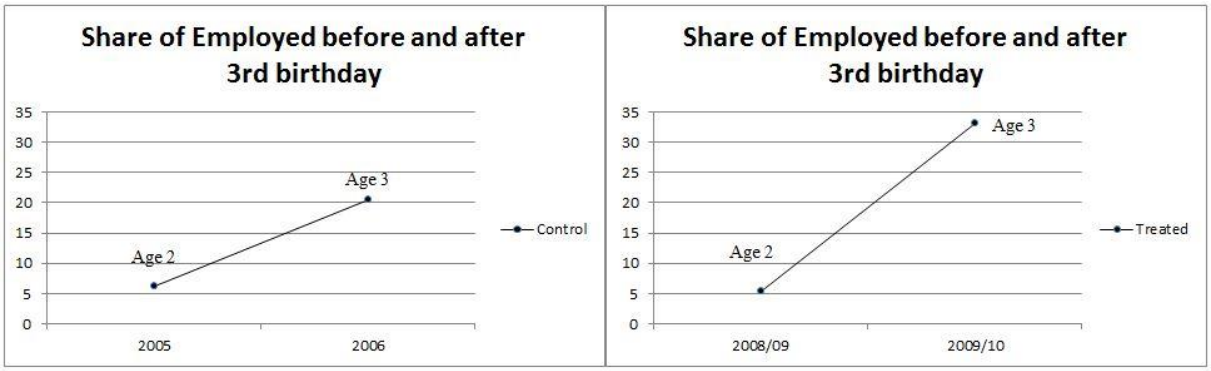
However, this comparison can only be a first step towards the impact evaluation. Indeed, if it does compare the treated to the non-treated, it might also comprise other factors such as a general trend, business cycle effect or other maturation bias. That is why I proceed to a second approach, where the focus lies entirely on the reform date. I consider the last cohort of non-treated and the first cohort of treated among the eligible population, and compare the evolution to a non-eligible population over the same period of time. This makes the approach a more standard difference-in-differences setting. I consider, on the one side, the same population in terms of entitlement advancement, shortly before and after the reform implementation. Then, I similarly point out a population that was not subjected to any treatment, and compare their respective evolution in employment rates over the same period 2008-2010. Therefore, if there is a trend induced by the low business cycle, the control group will capture it. To this respect, two control groups are relevant. The first group comprises mothers of slightly older children, which allows us to remain focused on maternal labour market outcomes and to control for

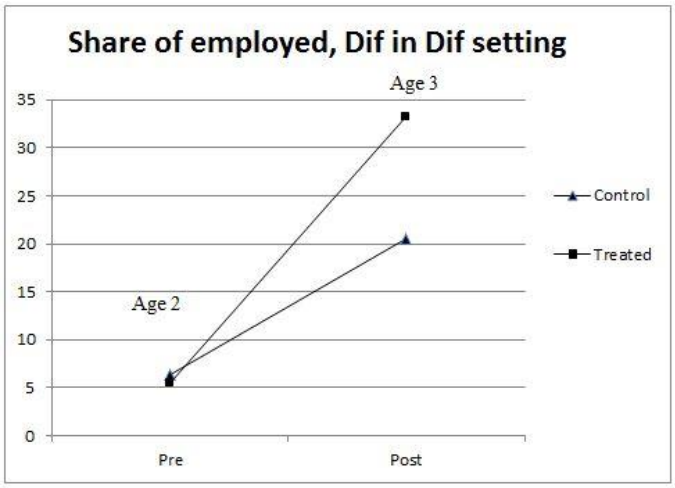
idiosyncrasies imputable to mothers of pre-school children (similar concerns, inadequate childcare supply, etc.). The second group extends the analysis to all women without pre-school children, under a looser hypothesis that women in general are likely to face comparable situations in the labour market. A summary of all the identification strategies used in this study is represented in Table D in Appendix.

4. Results

In the first approach, the baseline treated group is composed of mothers who ended their three-year PL entitlement shortly after the reform implementation. The outcome variable (employment level) is observed before and after the child's 3rd birthday, knowing that the birthday occurs in the new PL regime and is therefore the outcome "after treatment". We compare the evolution in the employment rate before and after the end of PL to that of a control group. The control group is composed of mothers with the same individual characteristics, but around a date where no reform occurred. Therefore, the return-to-work rate of the control cohort is used as a counterfactual: it is assumed to be the return-to-work pattern that we would have observed for the treated population had no reform occurred. The strategy can be summed up graphically in the following way:

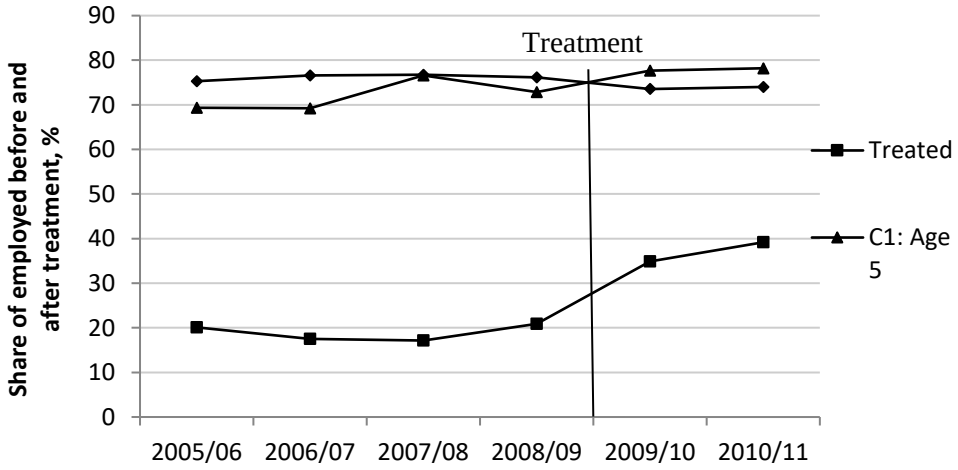
Charts 3 The cohort effect approach





In this cohort effect analysis, the common trend assumption is verified almost mechanically, as mothers with a 2-year old are not employed in more than 6% of the cases, and the employment rate quickly converges to zero for children younger than 2 - whether it be before or shortly after the reform. As to the 2nd approach, i.e. the more traditional difference-in-differences setting, the following chart 4 represents descriptively the employment rates of the treated and the control, before and after the reform. In the years preceding the reform, mothers right after the end of the 3-year PL entitlement work in quite constant proportions, 17 to 20 percent. Employment rates of the control groups are considerably higher, but do not record any marked pre-reform trade either.

Chart 4 The standard dif-in-dif approach



After these descriptive representations, the results of the actual estimations are reported in the following tables 3 to 7. The table 3 represents the results of the 1st approach, on employment and activity levels for the entire sample, then on two sub-samples of higher and lower educated mothers.

Table 3 Impact of the 2008 reform on post-PL employment and participation

	(1)	(2)	(3)	(4)
	To be employed	To be active	To be active	To be active
			High Educated	Low Educated
After	0.129***	0.155***	0.212***	0.0740*
	(0.0272)	(0.0321)	(0.0478)	(0.0395)
Treat (cohort)	-0.0311	-0.0474*	-0.0630*	0.00143
	(0.0206)	(0.0257)	(0.0372)	(0.0332)
Treat*After	**0.144*** **	**0.223*** **	**0.206*** **	**0.209*** **
	(0.0404)	(0.0456)	(0.0633)	(0.0655)
Education				
Graduated HS		*Reference value*		
None or Element.	-0.0662*	-0.111***		
	(0.0341)	(0.0388)		
Didn't graduate HS	-0.0728***	-0.0984***		
	(0.0226)	(0.0259)		
Superior Educ	0.0793**	0.0735**		
	(0.0335)	(0.0359)		
Constant	0.0556*	0.120***	0.121**	0.0545
	(0.0328)	(0.0378)	(0.0530)	(0.0399)
Observations	1,217	1,217	741	476

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: LFS 2005-2010

"After" is the before/after time dummy, and it is equal to 1 if a mother is observed "after" the child turns three. It accounts for the time trend common to control and treatment groups. It is large and significant, given that mothers are more frequently active when the child is older compared to when he's younger. "Treat" it is the treatment dummy, and it is equal to 1 if a mother belongs to the treated cohort. It accounts for average permanent differences between the cohort observed around the reform date (treated) and the cohort observed around the non-reform date (control). It is predictably mostly close to zero and non-significant, as we compare mothers with extremely similar individual characteristics. Our parameter of interest is the interaction between the time dummy and the treatment dummy: it gives us the increase, in percentage points, in the share of active mothers that is to be attributed to the reform. The positive effect is large and significant: the treatment increased the probability of being employed/active by the child's third birthday by 14.4/22.3 pp. Therefore, a large proportion of mothers respond to the incentive to shorten the inactivity duration. This result is expected, as the reform is designed as a financial sanction for mothers who take the 4th year of benefits (by cutting the benefit amount by half), given that already before this reform, the 4th year option comes at the cost of losing the three-year job protection.

However, the relatively large time difference between the control and the treated makes the *ceteris paribus* clause more difficult to argue. Before addressing the issue of maturation bias (a possible increase in maternal employment rates independently of the reform) and moving on to the 2nd approach, one variable stands out and is important to interpret: educational attainment. As a covariate, it indicates unambiguously that levels of activity at the end of PL are heterogeneous across different groups of mothers. However, if I split the sample in two sub-samples and estimate the reform impact, the results show that both low-skilled and high-skilled respond strongly to the reform and increase their participation after treatment. The effect is even slightly higher for the less educated, although we would expect it to be the opposite, be it only via the eligibility channel of previous employment. Mothers who are entitled to the insurance-based part of the scheme among the less educated are seemingly driving the effect. On the other hand, the higher educated half of the sample is not particularly more sensitive to the reform than the others, and it is also confirmed for mothers with superior education. These mothers could have been expected to value faster return-to-work more, yet a part of them still opts for the longest four-year track (the activity rate does increase, but still does not exceed 40%). Bicakova and Kaliskova (2016) also observe that mothers' returns-to-work are less stratified by education than the structure of the financial incentive would lead us to expect, as the higher

educated also opt for longer tracks, and the difference with the lower educated is mostly driven by the latter's higher exits to unemployment. This confirms the tendency of Czech mothers to value time at home and childcare across educational levels regardless of human capital and financial incentives (Mullerova, 2014), an important finding which deserves further exploration. When maternal education is replaced by the spouse's education, the results are fairly similar, probably due to the high positive correlation between the spouses' educational levels (a coefficient of 0.54). Although spouse's education does not come out significant as a covariate, the sub-sample analysis confirms that mothers in couple with higher and lower-educated partners experience a comparable effect of the reform on their activity levels: an increase of 28,8 and 26.6 percentage points respectively, both significant at a 1% level (for detailed results, see table E in Appendix).

It should also be noted that the number of children, included as covariate in the regressions, is not significant[12] and does not change the scope of the effect. In a sub-sample analysis, the effect seems to be lower as the number of children goes up, suggesting that the presence of other children might increase preference for home production and/or increase the difficulty to reconcile childrearing and employment, given that the labour market still provides in a large majority full-time contracts. However, these results might be affected by a strong restriction which consists in focusing only on mothers whose youngest child is at stake. Therefore, when this child is 3 years old, I exclude mothers who exited their previous PL and entered PL for another child. This selection is necessary, as another birth would be a confounding factor behind maternal inactivity. However, relaxing this restriction would add 18.5% to the baseline sample of treated, and as much as 44.8% of mothers to the sample of the persistent effect analysis, which is likely to affect the sample's characteristics. To address this issue, I describe the differences in the descriptive statistics in Appendix (Table C). The main difference is that mothers sorted out by the restriction are slightly younger, and obviously their employment probability is significantly lower: they have an extra child aged 0 to 3 that our treated groups do not have. Given that I seek to estimate the effect of the reform on returns to work, allowing for mothers with younger children in the sample would most likely, depending on their respective proportion among the treated and the control, lower the scope of the effect. For a pure demographical fertility analysis of this reform, see Stastna et al. (2016), who show that the

[12] Which is why it is not reported in the regression tables here. Detailed results available upon request.

introduction of shorter leaves led to a durable stabilisation of the second-child birth interval, while it had been in a constant increase up until then.

Finally, in order to control for a possible maturation bias and to get as close as possible to the assessment of a genuine impact of the reform on maternal employment, I combine this first cohort analysis approach with a complementary approach, which re-centers fully on the period of interest (2008-2010). In this second approach, the baseline eligible sample is composed of mothers who ended their three-year PL entitlement shortly before and shortly after the reform implementation. The former do not receive treatment, while the latter do. We observe the outcome variable "share of employed" at the moment when the treatment/absence of treatment become effective, i.e. once the youngest child turned 3 years old. At that moment precisely, the non-treated mothers exhaust job-protected parental leave and can stay one extra year on benefits. The treated mothers, on the other hand, have two new alternatives. Either they are now out of benefits if they had previously chosen to keep the former benefit monthly amount, or they stay on benefits for the extra year but the amount is cut in half. We observe the difference in employment rate between these two consecutive cohorts, and compare it to the one of a control group. Two relevant control groups were identified (cf. section empirical strategy): mothers with older children and women without children.

Table 4 Impact of the 2008 reform on post-PL participation, 2nd approach

	C : Child aged 5		C : No child 0-5	
	(1)	(2)	(3)	(4)
VARIABLES	To be employed	To be active	To be employed	To be active
After	-0.0292	-0.0149	-0.0256***	-0.00520*
	(0.0388)	(0.0371)	(0.00321)	(0.00277)
Treat	-0.342***	-0.396***	-0.639***	-0.630***
	(0.0355)	(0.0361)	(0.0229)	(0.0256)
After*Treat	**0.157*** **	**0.206*** **	**0.148*** **	**0.191*** **
	(0.0523)	(0.0535)	(0.0357)	(0.0390)
Education				
Graduated HS	*Reference value*			
None or Elem	-0.0893*	0.0278	-0.260***	-0.104***
	(0.0498)	(0.0522)	(0.00867)	(0.00771)
Didn't graduate HS	-0.0158	-0.0219	0.0224***	0.0606***
	(0.0303)	(0.0312)	(0.00381)	(0.00299)
Superior Educ	0.0572	0.0188	0.0439***	0.0373***
	(0.0408)	(0.0406)	(0.00439)	(0.00386)

Constant	0.524***	0.675***	0.817***	0.875***
	(0.0480)	(0.0482)	(0.00394)	(0.00328)
Observations	1,238	1,238	55,339	55.339
R-squared	0.121	0.130	0.259	0.281

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: LFS 2008-2010

When we compare the evolution in employment rates of treated mothers to the one of mothers whose youngest child is 5 years old (and therefore out of the PL and unconcerned by the reform), the increase imputable to the reform is 15.7 pp. Using a broader sample, Bicakova and Kaliskova (2016) compare the treated mothers with children aged 2 to 7 to the control group of mothers with children aged 8 to 13. With more than 75 000 observations, the comparable subset of their results is very similar to the one identified in this analysis, where I focus on the first cohort of mothers subjected to the reform and use the control group of mothers with a child aged 5. The second control group, i.e. mothers with no pre-school children, gives a similar result: 14.8 pp. As in the first approach, the effect on activity rates is higher than on employment rates, given that activity also comprises unemployment. However, when it comes to the results interpretation, I rely more heavily on employment. Compared to the vague definition of activity (the data show signs of confusion between the status of homemaker, unemployed and inactive), being employed or not is a strictly unambiguous question.

Last but not least, the impact persistency is examined. We observed the return to work patterns between the end of the job-protected leave and the end of the benefit entitlement, yet it would be interesting to see, for the same cohort of mothers, what their labour market situation would be one year later. Once the child turns four, the job protection has been exhausted for one year, and even the four-year track of benefits has been exhausted shortly. The reform's objective being higher maternal employment rates, it is important to study their labour market attachment beyond the PL scheme. Does the earlier return to work allow mothers to remain durably in the labour market? It appears to be the case.

Table 5 Impact of the 2008 reform on post-PL participation, one year after treatment

	C : Child aged 5		C : No child 0-5	
VARIABLES	(1) To be employed	(2) To be active	(3) To be employed	(4) To be active
After	0.0129	0.00608	0.00659**	-0.00196
	(0.0408)	(0.0251)	(0.00328)	(0.00282)
Treat	-0.266***	-0.207***	-0.317***	-0.185***
	(0.0427)	(0.0337)	(0.0305)	(0.0286)
After*Treat	**0.130****	**0.144*****	**0.147*****	**0.154*****
	(0.0592)	**(0.0441)**	**(0.0430)**	**(0.0364)**
Education				
Graduated HS	*Reference value*			
None or Elem	-0.309***	-0.100*	-0.268***	-0.103***
	(0.0608)	(0.0542)	(0.00903)	(0.00813)
Didn't graduate HS	-0.0895**	-0.0132	0.00886**	0.0617***
	(0.0365)	(0.0271)	(0.00400)	(0.00308)
Superior Educ	0.170***	0.0855***	0.0370***	0.0343***
	(0.0376)	(0.0272)	(0.00431)	(0.00378)
Constant	0.743***	0.926***	0.802***	0.876***
	(0.0579)	(0.0394)	(0.00407)	(0.00332)
Observations	853	853	53,577	53,577
R-squared	0.171	0.098	0.260	0.283

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: LFS 2009-2011

The positive effect of the reform on the maternal employment rate appears as strong one year after treatment as prior, i.e. after the end of PL entitlement. This result suggests that the first treated cohort not only exits into employment in larger proportion than previous cohorts, but then also stays in employment in larger proportion, and the non-treated do not catch up this trend in the medium term. This could be explained by the business cycle: mothers who stayed out of employment beyond the job protection might experience difficulties returning to the labour market in the context of higher unemployment rates. Further estimations show that the gap in employment rates between treated and control loses statistical significance by the age of 5 and 6. Childcare availabilities and primary school entry most likely stand behind this result.

Finally, in order to make sure that the results described above are not biased by any kind of general trend in maternal employments over the years 2000, independent of the reform and

uncaptured by the control groups, I perform a robustness check. It consists in applying the same regressions to the same populations, but shifted to a period when no shock occurred: namely one year before the reform. These placebo regressions' results are close to zero and non-significant, and are reported in the following tables 6 and 7.

Table 6 Placebo regression, 1st approach

VARIABLES	(1) To be employed	(2) To be active	(3) To be active High educ	(4) To be active Low educ
After	0.133***	0.157***	0.219***	0.0806**
	(0.0273)	(0.0321)	(0.0476)	(0.0392)
Treat	-0.0187	-0.0204	-0.0400	0.0246
	(0.0194)	(0.0252)	(0.0367)	(0.0329)
After*Treat	0.00763 NS	0.0131 NS	-0.0549 NS	0.0934 NS
	(0.0369)	(0.0431)	(0.0609)	(0.0582)
Education				
Graduated HS		*Reference value*		
None or Element	-0.0452	-0.0800**		
	(0.0302)	(0.0367)		
Didn't graduate HS	-0.0365*	-0.0486**		
	(0.0201)	(0.0239)		
Superior Educ	0.100***	0.0950**		
	(0.0342)	(0.0378)		
Constant	0.0340	0.0905**	0.135**	-0.00455
	(0.0314)	(0.0368)	(0.0528)	(0.0365)
Observations	1,309	1,309	756	553
R-squared	0.073	0.070	0.070	0.067

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: LFS 2005-2009

Table 7 Placebo regression, 2nd approach

	C : Child aged 5		C : No child 0-5	
VARIABLES	(1) To be employed	(2) To be active	(3) To be employed	(4) To be active
After	-0.0407	-0.00203	-0.00415	0.00159
	(0.0405)	(0.0305)	(0.00310)	(0.00270)
Treat	-0.593***	-0.651***	-0.661***	-0.669***
	(0.0336)	(0.0309)	(0.0216)	(0.0237)
After*Treat	0.0622 NS	0.0409 NS	0.0379 NS	0.0480 NS
	(0.0503)	(0.0453)	(0.0314)	(0.0348)
Education				
Graduated HS	*Reference value*			
None or Elem	-0.169***	-0.0865*	-0.255***	-0.115***
	(0.0470)	(0.0460)	(0.00813)	(0.00722)
Didn't graduate HS	-0.0781***	-0.0609**	0.0242***	0.0557***
	(0.0269)	(0.0259)	(0.00360)	(0.00290)
Superior Educ	0.113***	0.0836**	0.0428***	0.0387***
	(0.0425)	(0.0417)	(0.00436)	(0.00387)
Constant	0.759***	0.886***	0.827***	0.875***
	(0.0440)	(0.0397)	(0.00370)	(0.00313)
Observations	1,118	1,118	58,356	58,356
R-squared	0.345	0.394	0.243	0.271

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: LFS 2007-2009

5. Concluding remarks

In the post-transitional Czech Republic, two major reforms of parental leave have been implemented, both in a low phase of the business cycle. The first, in 1995, substantially increased the universal parental benefits duration by one year. The priority was then to appease pressures on the emerging labour market, coupled with a significant "re-familizing" policy trend. In 2008, the context is completely different on both the political and societal level. The multi-speed reform introduced three tracks with different payment durations, yet with the same total amount distributed per mother. In order to incentivise shorter leave with higher monthly cash transfer, the three-year track allowed mothers to keep the same monthly amount as in the pre-reform period, and mothers willing to keep the four-year duration had to accept the benefit being cut in half in the process. It aimed at reversing the trend induced by the previous reform and encouraging faster post-PL returns to work. Therefore, it re-prioritised the issue of maternal

labour supply for Czech policy-makers and conformed to the European Commission recommendations. Its positive impact on maternal employment is strong and significant, and ranges between 14 and 22 pp.

However, given the reform setting as a clear sanction for previously predominant four-year leaves, the question turns to the reasons for which the scope of the effect had not been even larger, especially for mothers with high opportunity costs of child caring. First, we can argue that the extent of the changes in mothers' work-family balance strategies is likely to be limited by the persistent lack of available childcare for children under the compulsory school age. Thévenon (2013) puts forward the conclusion that on the international level, the childcare supply plays a decisive role among institutional factors of mothers' employment. In the logic of institutional complementarity, we could therefore expect that shorter leave would prove to be an insufficient measure to increase maternal employment if carried out while unaccompanied by a larger network of child care facilities, especially for children under three. Public childcare supply has recently re-entered the Czech social policy agenda - with large support of the European Social Funds - and the new contours of the work-life balance institutional framework remain to be described and assessed. Beyond childcare provisions, however, we can also interrogate the cultural setting of gender-based division of tasks. As demonstrated in Mullerova (2014) and confirmed by Bicakova and Kaliskova (2016), the previous major parental benefit reform from 1995 also evidences a strong re-familizing effect and a low stratification by education. This relative homogeneity of responses across Czech mothers suggests that further research should interrogate and seek to explain Czechs' preferences for long leaves throughout the systemic transition and EU accession.

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Appendix

Table A Czech family policy between 1948 and 2016

FAMILY POLICY INSTRUMENT		EFFECTS ON FERTILITY AND PARTICIPATION (expected)	
Maternity Leave and Benefits		Fertility	Participation
1948	Duration moves from 12 to 18 weeks	+	- SR + MR *
1964	22 weeks	+	- SR + MR
1968	26 weeks	+	- SR + MR
1987	28 weeks and 37 weeks if 2 dependent children or single mother	+ +	- SR + MR
Additional Maternity / Parental **Leave**			
1964	Creation of 1 year Additional Maternity	+	- SR + MR
1970	2 years	+	-
1989	3 years	+	-
2001	Additional Maternity leave becomes Parental Leave, accessible to fathers in the same conditions	+	+
Additional Maternity / Parental **Benefit**			
1970	Creation of the AM benefit, 1 year if 2 dependent children or single mother	+ +	
1971	2 years (same conditions)	+ +	
1987	3 years if child born after December 1987 (same conditions)	+ +	
1990	AM benefit becomes Parental benefit, 3 years for all children (7 for handicapped)		-
1995	Extension of the parental benefit: 4 years	+	- -
1998	No more limitation of worked hours for recipients	+	+
2001	The limitation of income for recipients moves from vital minimum to 50% above it		+
2004	No more limitation of income for recipients		+
2006	Less strict limitation of hours spent in childcare facility for recipients' child		+
2007	Significant increase of the parental benefit	+ +	-
2008	Creation of a Multi-Speed Parental benefit: 2, 3 or 4 years	+ +	+
2012	No more limitation of hours spent in childcare facility for recipients' child	+	+
2012	2008 Benefit setting modification: more flexible monthly amount	+	+
Child benefit			
1959	Means-tested Family benefit becomes Child benefit	+	
1968	Universal Child benefit	+ +	
1995	Again Means-tested Child benefit		
2008	No more vital minimum indexation		
Other family policy instruments			
1957	Abortion legalization (abortion commities)	-	+ +
1970's	Various measures: Lower retirement age for mothers, Housing allocations, advantageous loans, tax deductions, subsidization of childcare and related expenses (meals, textbooks)	+ +	
1995	Modification of the legal status of nurseries, steep decline	-	-
2012	Revocation of the public nursery status by the Ministry of Health	-	-
2013	Children Group Act: tax benefits for alternative childcare facilities	+	+
2016	7-day paid paternal leave bill voted	+	+

* In the **short run**, the extension of the leaves automatically decreases mothers' participation, but in the medium run the aspect of job security rather increases participation after the leave.

Key

Post-transitional policies: from 1989 onwards

EU harmonization (preparations and EU accession): from 2000 onwards

Table B1 Summary Statistics. Treated vs. Control

	Treated	Control 1st app. (2006)	Controls 2nd app. (2009/10)	
			Child aged 5	No child 0-5
Individual controls				
Mean age	31,8	30,2	33,2	29,4
Married, %	72,1	78,2	80,1	33,8
Educational level				
No or elementary	5,8	10,7	11	5,7
High school, no grad	28,2	31,2	35,3	26,6
Graduated high school	49,4	45,1	40,3	51,1
Superior	16,7	12,9	13,4	16,6
Dependent variable				
Employed, %	32,4	20,5	78,6	68,9
N	312	317	201	20 080

Source: LFS 2006-2011

Table B2 Summary Statistics. Contextual, Women aged 20-39

	2006	2007	2008	2009	2010	2011
Educational levels, %						
No or elementary	6,9	6,9	6,7	6,7	6,4	6,1
High school, no grad	32,4	31,4	29,5	28	27	25,8
Graduated high school	49	49,3	49,8	49,3	48,9	48,1
Superior	11,8	12,4	14	15,9	17,7	20
Married, %	49,7	49,1	47,7	46,7	45,8	45,6
Unemployment, %	7,8	5,6	4,4	6,3	7,1	6,3
Activity, %	68,2	66,6	65	65	65	65,3
Activity no 20-24, %	73,6	72,6	71	71,4	71,8	72,6
Activity no child 0-5, %	81,5	80,2	79,15	78,4	77,8	77,2

Source: LFS 2006-2011

Table B3 Summary Statistics. Contextual, general

	2006	2007	2008	2009	2010	2011
GDP growth	6.9	5,5	2.7	-4.8	2,3	2
Unemployment rate	7.1	5,3	4,4	6,7	7,3	6,7
Fertility rate	1.328	1,438	1,497	1,492	1,493	1,427

Source: CZSO, Eurostat

Table C Summary statistics. Treated pop. without and with "youngest child" restriction

	Approach 1 and 2			Medium term effect		
Individual controls	Youngest child aged 3		No restriction	Youngest child aged 4		No restriction
Mean age	31.8	*	31.5	32.3	*	31.9
Married, %	73		74.4	77.4		79.2
Educational level						
No or elementary	5.5		6.4	9.2	*	8.2
High school, no grad	28.1	*	26.1	25.8		24.8
Graduated high school	49.7		49.4	49.8		46.8
Superior	16.8		18.1	15.2	*	20.2
Mean n. of children	1.65	*	1.8	1.69		1.88
Dependent variable						
Employed, %	33.2	*	28.1	66.4	*	45
N	292		360	217		331

Source: LFS 2009-2011

Note: the asterisk represents the statistical significance of the difference between the sample used for estimations and the larger no restricted population.

Table D Summary Table of Identification strategies

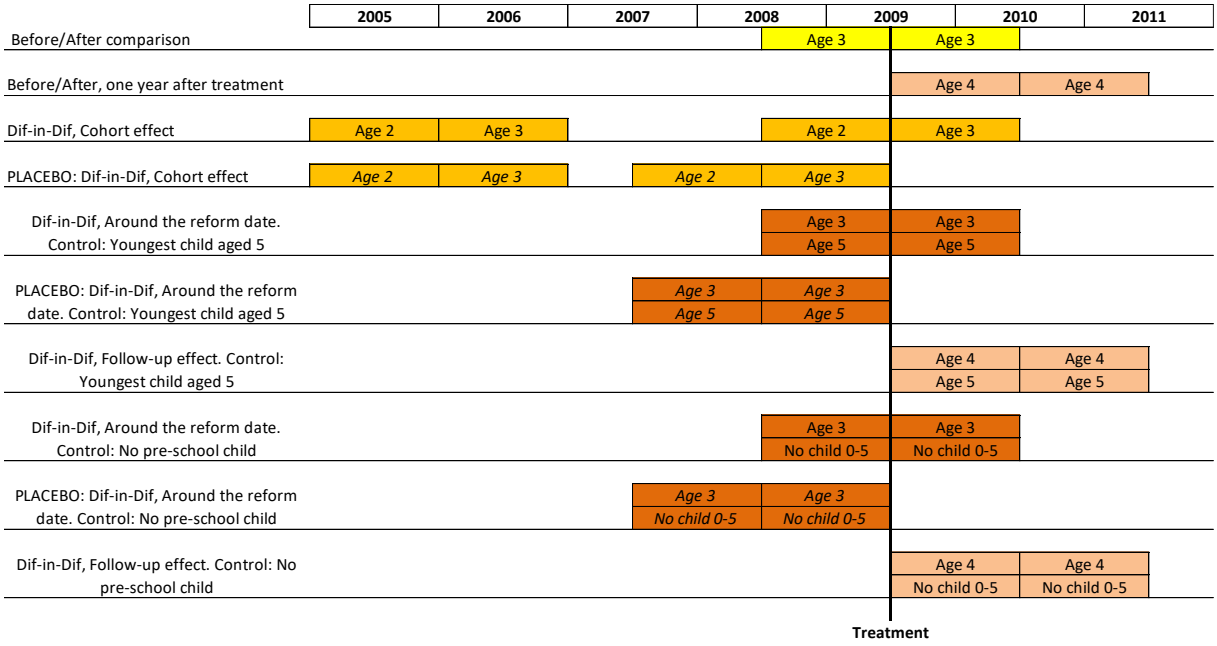

Table E Impact of the 2008 reform on post-PL participation by spouse's education

VARIABLES	(1) To be active	(2) To be active
	Spouse Higher education	Spouse Lower education
After	0.120***	0.141***
	(0.0456)	(0.0532)
Treat (cohort)	-0.0535	-0.0639
	(0.0390)	(0.0404)
Treat*After	**0.266*** **	**0.288*** **
	(0.0679)	(0.0717)
Education		
Graduated HS	*Reference value*	
None or Element.	-0.152***	0.0828
	(0.0522)	(0.113)
Didn't graduate HS	-0.129***	-0.0758
	(0.0380)	(0.0464)
Superior educ	0.0437	0.0828**
	(0.0982)	(0.0418)
Married	0.0762**	-0.00345
	(0.0383)	(0.0578)
Age: 25-29	*Reference value*	
Age: 15-24	-0.0801*	0.0171
	(0.0459)	(0.0726)
Age: 30-39	0.0105	0.0774*
	(0.0393)	(0.0401)
1 child	*Reference value*	
2 children	-0.0193	0.0270
	(0.0377)	(0.0381)
3 children	0.0332	0.0581
	(0.0630)	(0.0752)
Constant	0.144**	0.0604
	(0.0598)	(0.0684)
Observations	522	513
R-squared	0.182	0.187

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1