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ORIGINAL PAPER

Family policy and maternal employment in the Czech transition: a natural experiment

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Abstract Czech family policies have gone through dramatic changes since the 1989 transition to a market economy, resulting into the highest employment gap between women with and without pre-school children in OECD. This paper focuses on the 1995 Czech Parental Benefit reform which extended the payment of universal parental benefits to 4 years instead of 3 without an equivalent extension of job-protected parental leave, leaving to mothers the choice of either guaranteed return to employment or an additional 12 months of benefits. The study relies on a difference-in-differences strategy to assess the net effect of this large-scale reform on mothers' labour market participation. I find a strong negative impact on mothers' probability of return to work at the end of parental leave, with a heterogeneous size with respect to their educational attainment. I also find evidence of the persistence of this detrimental effect on mothers' employment beyond the short-term horizon targeted by the legislators.

Keywords Parental leave · Policy evaluation · Female employment

JEL classifications J16 · J18 · P30

1 Introduction

In 1989, the centrally planned Czech economy collapsed and the transition towards a market economy led to substantial changes in the welfare state as well as within the

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labour market. In particular, the labour market participation rate of mothers with young children declined steeply. In this paper,[1] I hope to partially explain this decline by examining the effect of a major family policy reform on maternal employment.

From an international perspective, the issue of female labour supply has received increasing attention from international institutions (European Commission 2013; Todd 2012). In the economic literature, the theoretical and empirical links between family policy and work-life reconciliation have been largely discussed in Western European countries. In France for example, Piketty (2005) analyses the impact of a parental leave reform 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 parental leaves negatively impact eligible mothers' further labour market outcomes. Lalive and Zweimüller (2009) and Lalive et al. (2014) estimate the impact of successive Austrian parental leave (PL) reforms on fertility as well as both short-run and long-run labour market outcomes, demonstrating that it is optimal to maintain similar durations of job protection and cash transfer. Schönberg and Ludsteck (2014) analyse a series of German PL reforms, and argue that negative effects of PL on labour market outcomes stem from an excessive duration of PL schemes, as well as from cash transfers outlasting the job protection.

These case studies are based on exploiting legal changes as quasi-experiences. They are framed by comparative studies (Ruhm 1998; Thévenon and Solaz 2013; Rossin-Slater forthcoming), which indicate that PL schemes are rather positively correlated to mothers' labour market attachment if they do not exceed a duration of 1 to 2 years, while appropriate PL duration and childcare policies can contribute to supporting both fertility and employment rates (Thévenon 2013). Therefore, statistical evidence tends to show that the guaranteed return to work provided by parental leaves increases job continuity in the case of relatively short leaves, while long leaves increase the time mothers spend with their children yet come with a significant cost for further employment.

As for family policies in Central and Eastern Europe (CEE), however, the economic literature is surprisingly scarce. Local policies in this region were typically very comprehensive, as they had been 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 concerning the incentives provided by parental leaves. This work can thus add to the existing literature and contribute to our understanding of the effects of very long parental leaves on labour market outcomes for mothers. Yet as far as I know, only one parental leave policy evaluation has been previously conducted in the post-transitional CEE (Balint and Kollo 2008), with other impact evaluations focused on the effect of childcare availability (Lovasz and Szabo-Morvai 2015; Lockshin 1999) or the effect of parental benefits on fertility (Malkova 2014). Balint and Kollo (2008) use the Hungarian Labour Force Survey, and argue that long leaves have a negative impact on maternal employment.

This paper will provide similar evidence based on the Czech 1995 reform, and it will confirm the argument by Lalive et al. (2014) that negative effects stem from the extension of cash benefits beyond the job-protected leave. The Czech PL reform was large-scale in terms of the impacted population as well as the scope of the extension. It consisted of remodelling, sharply and unexpectedly, the long universal paid PL scheme (36 months) by increasing the

[1] This study circulated between 2014 and 2016 under the title "Female Labour Supply in the Czech Transition: Effects of the Work-Life Conciliation Policies", released as EconomiX Working Paper 2014-50.

benefits duration by 12 months while leaving the job-protected period unaffected. This extension decreased incentives to return to work at the end of the job-protected leave, as the explicit goal of the reform was to maintain mothers in their role of out-of-market caregivers for a longer period. I assess whether and to what extent the goal was reached in terms of employment probability at the end of the job-protected period and beyond. The reform in question was announced and implemented on October 1st 1995 as a last-minute amendment to the State Social Support Act. This legal change thus came as a surprise, and represents an interesting case of a natural experiment. All current and future recipients of parental benefits (i.e. mothers of children less than 3 years old when the reform took effect) became eligible for the extension. Mothers were given the choice to either return to their previous employment at the end of the job-protected 36 months or give up the job protection and receive 12 extra months of benefits, under the condition that they remain inactive and take full-time care of the child. Though this reform was part of a re-familizing policy trend, it was also an attempt to ease the pressures on the emerging competitive labour market. As such, the objective of this paper is to assess the impact of this reform on mothers' labour supply and to disentangle the economic context of the reform from its genuine effects, using a difference-in-differences design applied to the Labour Force Survey.

The testable hypothesis is that the extension of the benefits operated as a disincentive to return to work by increasing the value of staying at home. The predicted effect of the reform is a lower proportion of mothers who are employed once PL entitlements are over. With the low, flat-rate benefits unaffected by earnings, their extension beyond the job-protected period yields different incentives for mothers with different labour market attachment and labour income. The analysis therefore pays special attention to the possibly heterogeneous effects along educational attainment. In the medium run, the extension of inactivity by several months as well as the induced loss of job protection might negatively affect the likelihood of transitioning back to employment later on. I therefore also investigate the persistence of the effect. The results show that a large causal effect exists: the reform significantly lowered mothers' probability of employment at the end of PL, with the estimated effect ranging from 15 to 25 pp depending on the empirical strategy. The impact is heterogeneous with respect to mothers' educational levels, although the presence of a strong negative impact on mothers with tertiary education calls for additional interpretations. Beyond the significant short-run effect, I show that mothers' employment probability was persistently and negatively affected for over 2 years after the benefit extension was exhausted.

The paper is organized as follows: Sect. 2 is dedicated to the institutional background of work-family reconciliation policies during and after the communist era (Sect. 2.1), with a particular focus on the 1995 parental benefit reform (Sect. 2.2). Section 3 presents the evaluation method, while Sect. 4 discusses the data. The results of the evaluation are reported in Sect. 5, and in Sect. 6 I conclude.

2 Background

2.1 Work-family reconciliation policies and practices before and after 1989

The centrally planned Czechoslovak economy, organized according to the USSR's model, led to a specific management of the labour force marked by strong state intervention and

virtually no unemployment. Obligatory employment did not apply to married women, but social and family benefits were conditioned on employment and female employment rates were particularly high compared with Western Europe (Bicakova et al. 2001). As early as 1955, women accounted for 42% of the Czechoslovak labour force (Haskova 2007). Massive full-time female employment coincided with a decline in fertility[2]: between 1950 and 1970, the total fertility rate dropped from 2.8 to 1.9, falling below the replacement level in 1966 (CZSO 2012). Prompted by this decline, a comprehensive pro-natalist family policy was implemented. One-year PL was established in 1964, and was then extended to 2 years in 1970 followed by 3 years in 1989, all conditioned on the presence of two dependent children in the household. Pre-transition family policy thus utilized two major tools: lengthening the leave for mothers with more than one pre-school child and also expanding the network of public day-care. Between the 1950s and the 1980s, the proportion of children attending nurseries rose from 3 to 18% while that of children attending kindergartens rose from 26 to 81% (Haskova 2007).

The fall of the "Iron Curtain" and the subsequent transition fundamentally changed the institutional context of work-family reconciliation. The previously state-controlled labour market was restructured and unemployment surfaced (Svejnar 1999). At the same time, the management of public expenditure called for less interventionist family policy and more market-based solutions to the issue of childcare. There were extensive cuts to—and a loss of interest in—public childcare, especially for children under the age of 3. In stark contrast to more than 1000 nurseries (40,000 places) in 1990, only 60 nurseries (1800 places) remained 10 years later (Kucharova et al. 2009). The evolution of public childcare illustrated the "ideologically induced animosity towards the institutions and policies of the welfare state" (Potucek 2001, p. 201). Compared with the pre-transition era, family policy formulated no explicit interest in either female labour supply or fertility (Sobotka et al. 2008). In the context of economic uncertainty, Czech women postponed or rejected motherhood, which is reflected in an unprecedented drop in the total fertility rate, down to 1.13 in 1999 (CZSO 2012). In parallel, mothers gradually withdrew from the labour market, increasing the motherhood-related employment gap[3] (Kaliskova and Munich 2012). In 1990, paid PL was extended to 3 years for all children, with no other condition than the relevant child's age. Given the rather non-interventionist political climate, this generous change in the PL scheme might appear paradoxical, but we can interpret it as an attempt to relieve labour market pressures and promote social peace. In 1995, the already comprehensive PL scheme was further extended through a large-scale reform, which is explained in detail in the following subsection.

2.2 Parental leave legislation in 1995

In 1995, the Act No. 117/1995 Coll. entirely remodelled the social security system, creating three pillars: Social Insurance (including maternity benefits), Social Assistance for material needs, and Social Support (including universal and means-tested benefits for families with children). As part of the Social Support pillar, the payment of parental benefits was extended from 3 to 4 years, a strikingly long duration by international standards. Before its implementation, that is between 1990 and 1995, paid parental

[2] The fertility in the 1960s also reflects the wide availability and affordability of birth control.
[3] See Fig. 5 in Appendix.

leave lasted until the child's third birthday. It was a continuous leave from the child's birth until their third birthday, or over 36 months.[4] Parental leave, synonymous with the period of protected employment, was combined with parental benefits with the same duration. The benefit amount as well as the maximum duration of leave was the same for all children, an extension to 7 years for handicapped children being the only exception. Some mothers were entitled to insurance-conditioned maternity benefits and entered parental leave only afterwards, while others were directly allocated parental benefits; yet, this distinction had no effect on the end of the entitlements which remained set on the child's third birthday. At that time, only mothers (or widowers) were entitled to maternity leave. Fathers were entitled to parental benefits but without the job security provided by PL; the proportion of fathers receiving benefits was negligible (0.1%). The parental benefits were paid at a flat rate: 1740Kc for each household in 1995, representing 22% of the average monthly gross wage[5] and 79% of the monthly full-time minimum wage. Eligibility was universal, and the only conditions for the recipient were to provide personal childcare (as opposed to using public childcare facilities) and to not work more than 2 h per day or earn more than 1800Kc per month.

On October 1st 1995, the benefit payment was extended from 3 to 4 years for all current and future recipients. The amount was kept at a roughly similar level, 1848Kc per month, i.e. 19.7% of the average monthly gross wage and 74% of the monthly minimum wage. The benefits were fixed at 1.1 times the minimal subsistence income, which would be reviewed periodically. The additional year of benefits was subjected to the same eligibility criteria, namely full-time personal childcare and prohibitive limitations on income (less than the benefits themselves) and working hours (less than 2 h per day). Job-seeking was not incompatible with the benefits, as their payment simply ceased the month in which the recipient entered employment and violated the previous conditions.

The specific feature of this reform was that the benefit extension was not accompanied by any extension of the job-protected PL. Under the jurisdiction of the labour code and independent of the social legislation, the job protection was unaffected by the reform and was only maintained until the child's third birthday. Yet, the benefit duration was increased to 48 months, i.e. until the child's fourth birthday. As a consequence, after the reform, mothers nearing the end of the 3 years had to choose either to return to employment or to receive 12 more months of benefits unaccompanied by job protection. For mothers who would not return to their job, the alternative to the 12-month benefit extension was the 6-month unemployment benefit, conditioned on previous employment and cut by half after the first 3 months of the benefit. This option was therefore uniquely available to mothers who had been employed pre PL and had their contract terminated by either themselves or the employer.[6] Mothers who did not have a

[4] Without counting pre-birth absence from employment, covered by the insurance-based maternity leave. Mothers who are ineligible for maternity leave fall directly under the universal parental leave and benefit status.

[5] 7907Kc, average monthly gross wage of employees in the civil sector of the national economy in 1995, provided by the Czech Statistical Office

[6] The most common reasons for not returning to previous employment being the lack of suitable childcare, disagreements over return conditions such as unavailability of part-time contracts (Kucharova and Svobodova 2006), firm restructuring and redundancy dismissals, with a certain social acceptance of these practices by the returnee mothers (Hašková 2015). Related laws and practices somewhat improved in the recent decades under pressure from the EU (Haskova and Uhde 2011).

job to return to as a result of being previously unemployed or inactive (e.g. students or homemakers) did not have this option and would receive only minimal social support. The parental benefit extension was consequently more generous in terms of accessibility and length, and encouraged mothers to postpone their job-seeking and return-to-work at the risk of worse labour market prospects at the end of the extension. It should be noted that the benefit extension (Section 30) was not initially intended as part of the Act and was not discussed by the legislature; it was added later on by the executive, at the initiative of the Christian Democrat Union. Thus, on top of reducing unemployment and promoting social peace, the postponement of mothers' return to employment was the policymakers' conservative, "familialist"[7] response to the pre-1989 preference for state-led childcare.

On October 1st 1995, the parental benefit reform was implemented as follows. From that date onward, the benefit was extended until the child's fourth birthday for all current and future recipients. Treatment eligibility was based on the threshold of the child's third birthday (in other words, on their date of birth 3 years earlier). As a consequence, the mothers of children whose third birthday occurred before October 1st 1995 had used up their benefits and leave entitlements before the reform and did not qualify for the benefit extension. Mothers of older children were still receiving benefits when the reform was being implemented, and they were covered by the extension. The first cohort of mothers to be exposed to the reform is the most interesting to analyse. For them, the extension came unexpectedly, thus excluding any adaptations of their fertility strategies (number of children, date of birth, etc.) to the eligibility criterion. I am therefore particularly interested in the return-to-work patterns of those mothers who experienced the end of their 3-year job-protected period shortly after the implementation of the reform.

3 Data and summary statistics

3.1 Data

I use the Czech Labour Force Survey (LFS), collected by the Czech Statistical Office on a quarterly basis as of 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 five consecutive quarters. The data are collected on a declarative basis, and provide a large battery of variables relative to each person's status in the labour market in the current quarter. The surveys are not conducted with the aim of analysing work-family reconciliation, as they are focused primarily on employment, but they are rich enough to be exploited from this angle, and no other data of comparable extent exist for the post-transition context of the 1990s. Among the major drawbacks of the data, the panel rotation does not allow us to trace one's economic history and to have knowledge of its evolution in the years preceding and

[7] As Saxonberg (2013) suggested, "familialist" policy should rather be referred to as "maternalist", since the stress is put on the mothers' role as caregivers.

following childbearing. Another limitation is that the LFS does not record information about income. It would have been useful to take into account wages and welfare transfers, and it would have been interesting to assess the effects of the reform on further earnings.

The main outcome variable is the labour market status, namely the transition back to *employment*. It should be noted that the reform's impact could also be evaluated on transitions to activity instead of employment, with a slightly different and complementary interpretation given that this outcome would comprise mothers who transitioned from parental leave to unemployment. However, due to the data structure, this analysis will mainly focus on employment. The labour market status is self-reported, and the value "employed" is the most straightforward and followed by several complementary questions. The aggregate value "active", however, is composed of mothers both with and without employment, and descriptive analysis of the multiple employment status questions reveals that mothers who are not employed while caring for a young child tend to declare in contradictory ways whether they are on parental leave, unemployed or inactive homemakers. Therefore, "being active" is a somewhat noisy and ambiguous piece of information for the population of mothers on leave, and the answer to the straightforward question "Are you currently employed" has been given priority for this analysis. Equivalent results for activity are reported in Tables 9, 10 and 11 in the Appendix. The difference between the two outcomes lies mostly in the impact on activity being larger than the one on employment.

As for the sample construction, mothers are identified in an indirect way in the LFS. The upper bound for the sample's age is set to 39, as I am interested in mothers of preschoolers and seek to minimize the risk of confusion between mothers and other adult females such as grandmothers in the household. The age of the child is given, while their date of birth is not. To cope with this deficit, I identify mothers at the end of the job-protected period by exploiting the panel structure of the data, via the child's transition from age 2 to age 3 from one quarter to the next. Through this method which exploits the panel structure of the data, I construct a "transition" variable which signals that the youngest child in the household has turned from 2 to 3 years of age[8]—meaning the mother has just exhausted her PL entitlements. This sample construction is quite restrictive in terms of sample size. Within the considered period, we lose mothers who enter the rotating panel after the age transition and those who quit the survey before the age transition; at both bounds of the considered period, we lose mothers whose child's birthday occurs before or after the quarters used for the estimations. Despite the restrictions, the large size of the dataset allows us to constitute a sample of 1464 mothers, which is representative of 141,000 individuals on a national scale.

As for the choice of quarters, the analysis focuses on three quarters before and three quarters after the reform, with a symmetrical sample around a date when no reform occurred. This choice is driven by my identification strategy. I focus on the first treated mothers shortly after the reform implementation, and similarly on the last non-treated mothers in the year preceding the reform's implementation. Together, this reform cohort yields the first difference. I then compare these employment rates to those of a control group, which is defined in two ways. The first approach relies on mothers with similar

[8] In addition to the transition to the age of 3, I identify in the same way mothers whose youngest child recently turned 4, 5 and 6, for complementary analysis and robustness checks.

characteristics around a date when no reform occurred (hence three quarters before and after a non-reform date), and the second approach focuses solely on 1995/1996 and appoints mothers with slightly older children as the non-eligible control group. The sample's detailed description is provided in Sect. 4. The choice of quarters is also adapted to the fact that the data do not include the exact dates of birth, so I rely on the transition variable instead. For the very first quarter after the reform (last quarter 1995), if the variable indicates that the child has reached the age of 3 since the previous record (third quarter 1995), I cannot identify the date of birth precisely enough to determine whether the transition occurred before or after October 1st 1995. Depending on the interview week of a given household, the transition in age from 2 to 3 may have occurred before October 1st 1995 (non-treated) or after October 1st 1995 (treated). I cannot stipulate clearly that all the mothers in this wave of the survey are eligible for the extra 12 months of benefits, which is why the fourth quarter of 1995 is excluded from the estimation. I will therefore compare mothers who experienced the transition between January 1st 1995 and September 30th 1995 (non-treated) with those who experienced the transition between January 1st 1996 and September 30th 1996 (treated); in other words, my before/after sample comprises the quarters 1, 2 and 3 in 1995 and the quarters 1, 2 and 3 in 1996.

3.2 Summary statistics

Before focusing on mothers, I compare their basic summary statistics to the overall female population within the same age group (15–39) for the pre-reform year 1995. Mothers count for 63.7% of that age group; their average age is higher (29.6 compared to 26.2 years), and they are much more likely to be married (83% compared to 54%). They are slightly more educated than the overall population (37.5% compared to 34.7% for high school graduation, and 6.5% compared to 5.8% for superior education), which may be partly linked to their higher average age. As for labour market participation, the proportion of unemployed mothers is similar to that of the overall female population (4%), as mothers are somewhat more likely to be employed (58% compared to 54%) and less likely to be students (15% compared to 20.4%). These are the characteristics of all mothers, independent of the children's ages.

As to the estimation sample, the first step is to extract treated and non-treated mothers for the first difference, i.e. mothers whose youngest child turned 3 within three quarters before (non-treated) or after (treated) October 1st 1995. For the difference-in-differences estimation, I then broaden the sample by comparing them with cohorts around two dates when no reform occurred (first approach) and with an alternative control group around the actual reform date, composed of mothers with slightly older children (second approach). The treated and control samples feature comparable individual characteristics in terms of independent variables; the differences and their statistical significance are provided in Table 8 (A–C) in the Appendix. In terms of employment status, the differences between treated and control in the different estimation approaches, as well as their decomposition by educational level, are summarized in Table 1 below.

For the first difference-in-differences approach, the 1993–1994 non-reform cohort has similar employment rates to that of the reform cohort prior to treatment, and the 1997–1998 non-reform cohort records much a lower mean outcome, as expected given

Table 1 Employment rates, summary statistics

	Before/after		1st approach			2nd approach	
	Treated (Jan.–Sep. 1996)	Non treated (Jan.–Sep. 1995)	Reform cohort (1995–1996)	Control cohort 1 (1997–1998)	Control cohort 2 (1993–1994)	Eligible (age 3)	Non eligible (age 4)
Economic status (%)							
ML or PL	52	30.4 (***)	41.4	50.8 (***)	49 (***)	41.4	4.2 (***)
Study	0.3 0	8	0.5	1	0.3	0.5	1
Work	17.2	39.2 (***)	28	15.6 (***)	33.1	28	69.8 (***)
Unemployed	2.4	10.1 (***)	6.2	1.1 (***)	9.6	6.2	9.8 (**)
Homemaker	27.5	19.2 (***)	23.4	31 (***)	6 (***)	23.4	14.6 (***)
Employed by level of education (%)							
None or elem.	15.6	17.1	16.4	5.4 (**)	18.2	16.4	40.5 (**)
Did not graduate HS	10.1	40.3 (***)	24	11.8 (***)	29.6	24	69.2 (***)
Graduated HS	22.6	37.9 (***)	30.4	19 (***)	36.3	30.4	71.7 (***)
Superior educ.	31.3	63.6 (***)	47.7	34.7	51.6	47.7	88 (***)
N	379	365	744	720	302	744	623

Significant differences between treated and controls are reported with the level of significance in parentheses. Source: LFS, 1993–1999

that it takes place in the new PL regime. For the second approach, the control group composed of mothers with older children records, unsurprisingly, much higher employment rates. As to the first difference, reported in the two first columns, we observed that the share of employed mothers indeed demonstrated a substantial decrease over the period of interest. Mothers became less likely to be employed at the end of their PL, and I will examine the causal relation between the 1995 reform implementation and this observed decline in the rest of the paper. In order to provide a clear, descriptive picture of the employment rates of mothers at the end of PL in years leading to and following the reform, Fig. 1 plots the share of employed at each quarter between 1993 and 1998.

It should be noted that the period of interest was not subject to other legislative shocks (for instance, the fiscal system was stable until 2005). The supply of public childcare was also relatively stable over the period 1995–1996, after a steep decline in the very first transitional years.

4 Empirical strategy

After a preliminary before/after comparison, I develop the conclusive difference-in-differences estimation using two alternative choices of control group. I want to account for mothers' return-to-work patterns after the 36 months of parental leave, so as to test the hypothesis that the reform induced a disincentive to transitioning back to employment.

Previous evidence shows that parental leave schemes mostly increase post-maternity employment in the medium run (Ruhm 1998) or at least do not decrease it (Lalive and Zweimüller 2009), depending on the total duration of the scheme as well as on the way in which job protection and cash transfers are combined. The specific feature of this reform, beyond its marked length by international standards, is that it creates a disparity between the duration of job protection and the duration of benefits, in favour of the latter. Schönberg and Ludsteck (2014) and Lalive and Zweimüller (2009) analyse a series of PL reforms in Germany and Austria, and demonstrate that when benefits outlast job protection, it is likely to lower mothers' post-maternity labour market attachment. I follow a similar reasoning in the evaluation of the impact of the reform: a difference-in-differences design applied to the short-run return-to-work probability. What are the mechanisms of the reform's impact on mothers' return to work? The extension of the flat rate benefit (a little under 20% of the average wage) takes place at

Fig. 1 Share of employed mothers once child reaches age 3. Note: At each quarter, proportion of mothers in employment shortly after youngest child turned 3. Source: LFS 1993–1998

the expense of the guaranteed return to work; thus, we can expect a heterogeneous effect on mothers according to their labour market attachment and labour income. Also, at the moment of the reform implementation, the economic situation was deteriorating and unemployment was rising. The benefit extension might therefore have been used as a tool to delay return to activity for mothers with low labour market perspectives. Yet, the unemployment threat may also encourage mothers to value the job protection more and to opt for a return to guaranteed employment instead of 12 extra months of benefits followed by uncertainty. In order to test these assumptions, I estimate the causal effect of the reform on mothers' employment probability just after the end of their job-protected PL. In other words, I identify the impact of the extension of benefit payments from 36 to 48 months on mothers' employment probability after the 36th month. My outcome of interest is the employment status at the end of parental leave (as soon as the youngest child turns 3). Given the data structure, this equates to considering one's economic status in the first quarter following the relevant child's "transition" to age 3. After this third birthday threshold, parental leave entitlements had recently expired and the possible difference in returning to employment between treated and non-treated mothers can be observed.

As a visual starting point to the description of the evaluation strategy, Table 2 sums up the mechanism of the 1995 reform.

As a preliminary step, I estimate a simple before/after comparison, where mothers from the first row of Table 1 are the last to be unaffected by the reform (they quit the parental benefit system between January and September 1995, and they do not receive treatment but are close to the limit), and mothers from the second row are the first treated (the transition from age 2 to age 3 is recorded between January and September 1996, and they are entitled to 12 extra months of benefits and also close to the limit). In other words, I compare the employment probability of mothers whose child reached 36 months shortly before October 1st 1995 to that of mothers whose child reached 36 months shortly after October 1st 1995. I estimate a linear probability model, and correct for heteroskedasticity. This simple approach reveals a strong and significant drop in employment rates and constitutes an invitation to build a more sophisticated causal framework. Indeed, one wants to make sure that the observed decline in employment probability is not affected by maturation bias, and that Czech mothers do not simply lower their labour supply from 1 year to the next due to the business cycle or other economic and social factors. As the temporal stability of mothers' employment rates cannot be assumed without further arguments to support it, I use the before/after sample

Table 2 Design of the 1995 reform

	Targeted by the reform	Job protection (=PL)	Parental benefit	Child's age on October 1st 1995	Situation after October 1st 1995
Child born before October 1st 1992	No	36 months	36 months	More than 36 months (already aged 3)	PL over, benefit payment over
Child born after October 1st 1992	Yes	36 months	48 months	Less than 36 months (not yet aged 3)	PL over, 12 extra months of benefit

as a first difference and proceed to confronting it with the employment rates of possible counterfactuals, therefore applying a double comparison.

I use the difference-in-differences design to compare the evolution of the eligible cohort's outcome around the intervention date with that of a control cohort. The estimated equation can be written as

$$y_i = \alpha_0 + \alpha_1 \text{Treat}_i + \alpha_2 \text{After}_i + \alpha_3 \text{Treat}_i{}^{*} \text{After}_i + \alpha_4 X' + u_i$$

where y_i is the outcome: to be employed or not. *Treat* is the dummy for the reform cohort (eligible group), it is equal to 1 if mother *i* belongs to the reform cohort 1995–1996 and it accounts for average permanent differences between treated and control groups. *After* is the before/after time dummy, it is equal to 1 if mother *i* is observed "after" the programme for both the reform cohort and the non-reform control cohort, and it accounts for the time trend common to treated and control. Finally, α_3, preceding the interaction term *Treat*After*, is the parameter of interest.

Descriptive statistics show that mothers have fairly similar individual characteristics between the two cohorts.[9] Covariates such as marital status, age, education and number of children are considered. This double comparison captures possible trends in the outcome. I adopt two complementary approaches in the choice of the control group. The first strategy, a cohort effect analysis, is similar to Schönberg and Ludsteck (2014) and Lalive et al. (2014). It entails comparing the evolution of the employment probability of mothers with similar characteristics in terms of eligibility, both around the reform date and around a date when no reform occurred. Here, October 1st 1997 is selected as the non-reform date, and I compare the change occurring after the reform implementation with the same date 2 years later, around which no confounding exogenous shock occurred. As I use three quarters before and after the reform in the regression, maintaining this 2-year distance between the reform date and the non-reform date is necessary in order for the two cohorts not to overlap. This is a first attempt to isolate the causal impact of the reform on mothers' return-to-work patterns, motivated by very similar individual characteristics between the reform and non-reform cohorts. They are identical with respect to the age of the youngest child (who recently turned 3), and differ only with respect to being exposed or not being exposed to the policy change, imposed by the date of implementation of the reform.

However, in the context of the first decade of the transition, marked by increasing imbalances in the Czech labour market, it might appear problematical to assume the common trend of the outcomes of the treated and control cohort 2 years apart. The reform aims to withdraw mothers from employment in a context of rising unemployment, and therefore raises the issue of endogeneity of its purpose with respect to the economic situation. In order to avoid capturing the deterioration of the labour market situation from 1 year to another instead of the genuine impact of the reform, I opt for an alternative choice of control group to ensure the results' robustness. In this second approach, the control group centres on the same three quarters immediately preceding and following the implementation of the reform in 1995–1996, and eligible mothers—our previously defined before/after sample—are compared to a group of non-eligible mothers who differ slightly in terms of the youngest child's age. I compare the eligible

[9] See Table 8 (B and C) with summary statistics of the sample, in the Appendix.

group, i.e. mothers whose child turned 3 shortly before or after the reform, to the control group, i.e. mothers whose child turned 4 within the same period. This complementary approach has advantages and shortcomings compared to the first one. As a disadvantage, control and treated now bear fewer analogies in terms of labour supply, as the end of PL entitlements is more distant for the non-eligible group than for the eligible group. On the other hand, the worsened economic conditions during the second half of the 1990s support this approach, as it captures possible employment trends over 1995 and 1996, and also prevents us from comparing different phases of the business cycle. For these reasons, I use both approaches and obtain a range of estimated values from which we can then draw main conclusions.

5 Results

The 1995 reform substantially changed the parental benefits framework by adding one extra year to what was already a generous and universal paid PL scheme, thus creating a disconnect between the 3-year job protection and the 4-year benefits. We can expect this to have an impact on mothers' return to work between the end of the job-protected leave and the end of benefits entitlement. Estimation results confirm this prediction and indicate a significant negative causal relation between the eligibility to the benefit extension and the probability of being employed in the months following the end of PL.

5.1 Before/after comparison

A simple before/after comparison using a linear probability model indicates that the probability of employment fell by 22 percentage points for mothers who were targeted by the reform and exposed to the possibility of 12 extra months of benefits between January 1st 1996 and September 30th 1996, as compared with mothers who were not exposed and who ended their PL between January 1st 1995 and September 30th 1995. In Table 3, I show that neither the significance nor the size of the effect varies notably when controlling for individual characteristics. Only the significant covariates are reported here; full results with all covariates and both employment and activity outcomes are reported in Table 9 in the Appendix.

The stability and the scale of the drop in employment probability suggest that the reform had indeed come with a significant change in mothers' transitions to employment, with educational attainment standing out as a meaningful covariate in stratifying the employment levels. On this basis and in order to get closer to a causal interpretation, I then let the before/after sample compose the first difference, which I confront with two successive control groups in a difference-in-differences framework. The results are reported in the following two subsections.

5.2 Difference-in-differences: comparison over time

In this cohort effect approach, I compare the observed change around October 1995 to a change around a date when no reform occurred (October 1997). The results obtained

Table 3 Impact of the 1995 reform on post-PL employment

Linear probability model		
	Dep. variable: to be employed	
	(1)	(2)
Treatment	−0.220***	−0.216***
	(0.032)	(0.032)
Education		
Graduated HS		Ref. value
None or elem.		−0.141**
		(0.0549)
Did not graduate HS		−0.0488
		(0.0358)
Superior educ.		0.165**
		(0.0663)
Covariates	N	Y
Observations	744	744

Robust standard errors in parentheses. This table reports the employment probability at the end of the PL, comparing treated (1996) and non-treated (1995) mothers, using age, education, matrimonial status and number of children as covariates. Source: LFS 1995–1996

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

with this difference-in-differences method corroborate the intuition from the preliminary results. In Table 4, the first column reports the results from the difference-in-differences strategy comparing six quarters in 1995/1996 (three before and three after the implementation of the reform) to six quarters in 1997/1998. In the second column, the individual characteristics are controlled for, and in the two remaining columns the sample is restricted closer to the reform date, with two quarters at each side of the reform in column 3, and only one quarter in column 4.

The size of the effect, while still significant at the 1% level, remains at roughly 23 pp when I restrict the number of quarters to 2 instead of 3 on each side of the intervention date. The restriction to only one quarter before and after provides similar but less clear-cut results, as the significance comes down to the 5% level and the size of the effect to 18.4 pp. It should be noted that the sample size becomes very low with this restriction, with only 285 treated and 244 control individuals. Compared with the before/after comparison, the difference-in-differences estimation features two additional variables, which capture seasonality and the underlying trend. The seasonality parameter appears to be non-significant, which is consistent with the sample structure, as we compare identically large fractions of a year (9 months before and 9 months after the treatment), with a 2-year interval. Concerning the trend, the coefficient is sizeable[10] and significant, which is expected given the choice of the non-reform cohort. Although no reform had occurred for mothers in the control group, they were in fact all affected by the reform of 2 years earlier, instead of all being unaffected. For this reason, as well as for the worsening economic conditions over the late 1990s, considering a control cohort 2 years

[10] The positive sign might be misleading, but the interpretation of the coefficient is the probability of employment in the reform cohort (1995–1996), taking the non-reform cohort as reference (1997–1998). The employment rates were higher in the first period, and for this reason the coefficient is positive.

Table 4 Impact of the 1995 reform on post-PL employment

Difference-in-differences 1st approach				
	Dep. variable: to be employed			
	(1)	(2)	(3)	(4)
Treatment	−0.236***	−0.231***	−0.226***	−0.184**
	(0.042)	(0.042)	(0.051)	(0.071)
Seasonality	0.0159	0.0149	0.0101	0.0335
	(0.027)	(0.027)	(0.032)	(0.043)
Trend	0.244***	0.236***	0.252***	0.253***
	(0.032)	(0.031)	(0.039)	(0.053)
Education				
Graduated HS		Reference value		
None or elementary		−0.138***	−0.117***	−0.136**
		(0.033)	(0.042)	(0.058)
Did not graduate HS		−0.0593**	−0.0712**	−0.0962**
		(0.023)	(0.029)	(0.040)
Superior educ.		0.155***	0.112*	0.147*
		(0.049)	(0.061)	(0.080)
Covariates	N	Y	Y	Y
Bandwidth (quarters)	6	6	4	2
Observations	1464	1464	998	529

Robust standard errors in parentheses. This table reports the estimates for the employment probability at the end of the PL, comparing reform cohort (95/96) and non-reform cohort (97/98), using age, education, matrimonial status and number of children as covariates. Columns 3 and 4 use restricted samples closer to the reform date. Source: LFS 1995–1998

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

before the reform (1993–1994) instead of 2 years *after* the reform would be a good alternative. However, the poor quality of the very first quarters of the Labour Force Survey at the beginning of the 1990s does not allow us to study a comparably sized cohort. Such an estimation can only be built if I restrict the sample to one quarter on each side of the date, instead of three.[11] These results corroborate both the significance and the scale of the impact estimated with the 1997–1998 control cohort, and are reported in Table 5.

As mentioned in the before/after comparison, one variable among the individual characteristics appears particularly relevant for further decomposition of the reform effect: the educational level. The difference-in-differences estimation results reported in Tables 3, 4 and 5 confirm that the employment probability is strongly stratified across mothers' educational attainment. The sign of the parameter is consistent with general knowledge on female labour market attachment across educational levels. Taking the group "graduated from high school" as a reference in the second column of the previous Table 5, mothers with no education or elementary education have a 13.8 pp lower probability of employment at the end of PL, while mothers with a higher level than elementary school but who did not graduate high school have a 5.9 pp lower probability. By contrast, mothers who completed tertiary education have a significantly

[11] The summary statistics for this cohort are reported along with the other cohorts in Table 8 (A) in the Appendix.

Table 5 Impact of the 1995 reform on post-PL employment

Difference-in-differences 1st approach				
	Dep. variable: to be employed			
	(1)	(2)	(3)	(4)
Treatment	−0.236***	−0.231***	−0.268***	−0.251***
	(0.042)	(0.042)	(0.080)	(0.080)
Education				
Graduated HS		Reference value		
None or elementary		−0.138***		−0.174**
		(0.033)		(0.069)
Did not graduate HS		−0.0593**		−0.0737*
		(0.023)		(0.044)
Superior educ.		0.155***		0.203***
		(0.049)		(0.077)
Covariates	N	Y	N	Y
Control years	97–98	97–98	93–94	93–94
Bandwidth (quarters)	12	12	4	4
Observations	1464	1464	563	563

Robust standard errors in parentheses. This table reports the estimates for the employment probability at the end of PL, comparing reform cohort (95/96) and non-reform cohort (97/98 and 93/94), using age, education, matrimonial status and number of children as covariates. Source: LFS 1993–1998

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

higher probability of employment at the end of PL, by more than 15 pp. In this table, the coefficient captures a difference in levels in employment probability, indicating that the sample is stratified with respect to the outcome variable.

However, in order to highlight whether the reform had a heterogeneous impact on these populations, we need to proceed to estimations on subsamples. The level of education can be used as a proxy for qualification and hence for labour income. The interpretation here is that less-educated mothers might benefit comparatively more from the extension of the flat-rate parental benefit than higher-educated mothers, as the replacement rate is higher and their post-PL labour market prospects are lower. Indeed, when I estimate the impact of the reform on subsamples of mothers according to their educational attainment, the highly significant results of two groups stand out: mothers who completed elementary school but did not graduate from high school (mostly from the apprenticeship track which does not award a high-school degree) respond significantly more to the reform than mothers who graduated from high school but did not pursue higher education. Around this high-school graduation pivot, indeed we observe a strong heterogeneity of the impact of the reform. However, as soon as we consider the tails of the distribution on both sides (none or elementary education on one side and tertiary education on the other), the results become much more ambiguous. As we can see in the following Table 6, the size of the negative impact on employment is not inversely correlated with the educational level in a straightforward, linear way.

My analysis is limited by very small sample sizes towards each end of the educational level distribution, but the results do suggest that we should be cautious in interpreting the impact of the reform on very high- and very low-skilled populations. While the impact on very low-skilled mothers appears to be non-significant (their

Table 6 Heterogeneous impact of the 1995 reform by education

Difference-in-differences 1st approach				
	Dep. variable: to be employed			
	(1) None or elem.	(2) Did not graduate HS	(3) Graduated HS	(4) Superior educ.
Treatment	0.015 NS	−0.309**	−0.191**	−0.319*
	(0.104)	(0.060)	(0.071)	(0.183)
Observations	141	626	583	114

Robust standard errors in parentheses. This table reports the estimate for the employment probability at the end of PL, following the cohort framework (1st D-in-D approach), for two subsamples of mothers, using age, matrimonial status and number of children as covariates. Source: LFS 1995–1998

*$p < 0.1$; **$p < 0.01$

employment rate was already very low before the reform, on average 17.1% in 1995), I detect a surprisingly strong and somewhat significant impact on mothers with tertiary education. This result suggests that highly educated mothers do respond to the reform, despite the low financial incentive offered by the flat-rate benefit. One of the reasons might be that their decision is not driven solely by economic rationality, but also by dominant social norms which explicitly promote the mother's role as main caregiver during the first years of a child's life.[12] In this sense, the salience of the norm in favour of active motherhood across the Czech society would play against the differentiation of the reform's incentive along income and educational levels. Another complementary factor might be informal arrangements with employers that reduce the cost of the delayed return to work, or simply a lower risk of unemployment due to better prospects on the labour market compared with lower-skilled female workers. The differences in fathers' educational level (still as a proxy for income) might also play a role, although this control variable systematically appears non-significant, be it for high-skilled mothers or the overall sample.[13] We are left to note this unexpectedly strong response of high-skilled mothers to the reform, but transforming the speculations on its rationale into positive explanations is beyond the scope of this paper.

5.3 Difference-in-differences: comparison over groups

So far, the estimation method has been built on a comparison of the reform cohort with a similar group of women at a different point of time, when no reform occurred. In this subsection, a different control group will be considered in order to test the previous results and to capture unequivocally the possible business cycle effects. Given that other factors might have influenced mothers' employment rates in 1995–1996, such as the expected and broadly advertised costs of the transition in terms of unemployment or

[12] See the work and numerous public appearances of the influential Czech psychologist Zdenek Matejcek, dedicated to establishing the negative effect of institutional childcare on a child's development and well-being, as well as to promoting the role of family as caregiver.

[13] Incorporating spouses' education into the analysis does not appear to bring additional understanding to mothers' return-to-work patterns, either as covariate or as subsampling dimension. Results are available upon request.

a low availability of childcare, I centre the estimation on these two specific years. The "treated" side of the difference-in-differences estimation remains unchanged—it is composed of the before/after sample, turned into the "reform cohort" for the sake of the first approach—and it will now be used as the "eligible group" as opposed to what we will define as the "non-eligible group". The legislative change applied to mothers whose child recently turned 3, and therefore, the labour market participation of mothers whose child recently turned 4 remained unchanged by the reform. Their employment probability may for some reason differ before and after the reform, but this would be independent of the PL reform. If we assume that the employment rate of the eligible group (youngest child aged 3 in 1995–1996) would have followed the same evolution as that of mothers with children who turned 4 over the same period, the difference-in-differences genuinely controls for the business cycle and provides us with a rather precise estimate of the causal effect of the reform. Figure 2 plots the employment rates of the eligible and non-eligible mothers around the reform date, showing the second approach as a more standard difference-in-differences setting.

We observe a declining trend in employment rates for the non-eligible, and a markedly steeper decline for the eligible. Assuming that the trend would have been similar had no reform occurred, the difference in slope represents the causal impact of the reform. This complementary approach, represented in Table 7, lowers somewhat the size of the estimated effect of the reform on employment[14] (bringing it down to 15.3 pp) while validating its high significance.

5.4 Persistence of the impact

Finally, mothers whose youngest child recently turned 4 are not only a suitable alternative control group but also a lead for investigating possible medium-term effects of the reform. If we consider the sample of eligible mothers from a cohort perspective, we note that children who turned 3 in 1996 will turn 4 in 1997, then 5 in 1998 and so on. They are the first cohort exposed to treatment in 1996, and they will be identifiable in the data in the following years through the increasing age of the youngest child. As the LFS data are a rotating panel, these are not the same individuals as in the treated sample in 1996, but they are assumed to be a random sample of the same population. As a consequence, this provides us with an insight into the return-to-work patterns of the first treated cohort year after year, first at the end of the job-protected leave, then at the end of the benefit extension if they decided to enrol, and so on. What we observe is a deceleration of their transitions back to employment—less than 50% of mothers with the youngest child aged 4 were employed in 1997—which coincides with the entry into the sample of the first cohort of mothers exposed previously to the benefit extension. While the overall decline in employment rates may be business cycle-related, the change in rhythm suggests that a substantial negative effect on female employment persists beyond the 12 months intended by the legislators. The same intuition can be applied to mothers with a youngest child aged 5. Mothers whose children turned 5 in 1998 are part of the same population whose children turned 4 in 1997 and 3 in 1996 (and therefore the first to be eligible for the benefit extension). If the reform has a

[14] See Tables 10 and 11 in the Appendix for estimated effects on activity levels with the first and second D-in-D approach.

Fig. 2 Share of employed mothers once child reaches age 3 and 4. Note: Outcome for mothers when youngest child reaches age 3 (Y_3) and age 4 (Y_4). Y_3: mothers eligible to the reform in October 1995. Y_3: mothers non-eligible to the reform in October 1995. Source: LFS 1993–1996

persistent impact beyond the 12 months covered by the benefit extension, then we will observe a fall in the employment rate as soon as the eligible mothers appear in the respective groups—after October 1995 for mothers of children aged 3, after October 1996 for mothers of children aged 4 and after October 1997 for mothers of children aged 5. It is indeed the case. Figure 3 reports the share of employed mothers with respect to the age of the youngest child (who "just turned" 3, 4 and 5, respectively, before and after the first quarter of eligibility), and the descriptive evidence does suggest a persistent effect in the medium run.

To confirm whether there is a causal effect, I apply the difference-in-differences framework and estimate the impact of the reform, over several years, on employment probabilities of the first treated cohort. The following Fig. 4 plots the employment rates of the last non-treated mothers before the reform, along with the estimated employment rates of the first treated mothers, up to 3 years after being exposed to treatment. The scope and significance of the difference are estimated with the second approach of the previously defined difference-in-differences strategy. The return-to-work profile of this first treated group is compared with that of the last non-treated group (first difference),

Table 7 Impact of the 1995 reform on post-PL employment

Difference-in-differences 2nd approach		
	Dep. variable: to be employed	
	(1)	(2)
Treatment	−0.155*	−0.153*
	(0.049)	(0.049)
Covariates	N	Y
Observations	1367	1367

Robust standard errors in parentheses. This table reports the estimates for the employment probability at the end of the PL, comparing eligible mothers (child aged 3) and non-eligible mothers (child aged 4) from the same cohort (95/96), using age, education, matrimonial status and number of children as covariates. Source: LFS 1995–1996

*$p < 0.01$

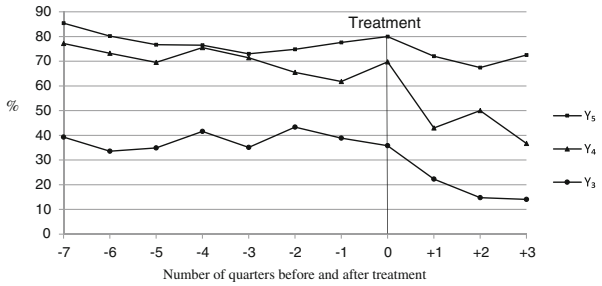

Fig. 3 Share of employed mothers by age of the youngest child in the medium run. Note: Mean outcome for the first treated cohort of mothers (treated Jan.–Sep. 1996), when the youngest child turned 3 (Y_3), then 4 (Y_4) and 5 (Y_5). Y_3: immediate effect, observed in 1996. Y_4: observed in 1997. Y_5: observed in 1998. Source: LFS, 1993–1998

which is then compared to the concomitant evolution in employment rates of mothers with slightly older children.

As the youngest child gets older, we observe a significant delay in return to work for treated mothers, which loses statistical significance once the youngest child turns 6. It should be noted that possible delays and rigidities in the labour market might account for a part of the observed evolution. We are looking at moments shortly after a threshold—the job protection had recently expired at age 3, then the benefit extension had recently expired at age 4—and it is likely that certain mothers will return to the labour market within the following months. However, we observe an unambiguous change in the rhythm of return to work of mothers of children aged 3, and a lasting delay at ages 4 and 5. The difference-in-differences estimation for each group of mothers therefore provides evidence of a significant persistence of the effect: 16.6 pp for children aged 4 and 9.5 pp for children aged 5.

These results extend the short-term validity of the negative impact on mothers' employment rate beyond the 12 months induced directly by the benefit extension. Mothers remain out of employment in larger proportions 2 years after the end of the benefit entitlement. Beyond that period, however, I find no statistically significant difference between the treated and the control groups. This finding is coherent with the work-family reconciliation background, with children aged 6 being admitted in elementary school and therefore facilitating labour market participation of the main caregiver.

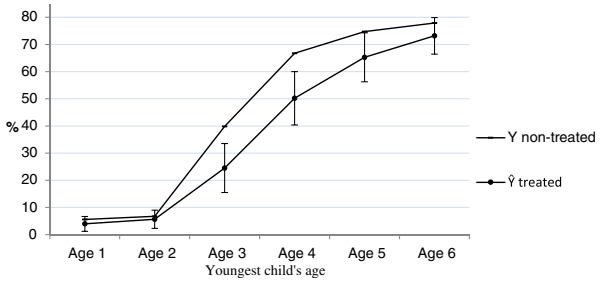

Fig. 4 Estimated return to employment of treated and non-treated in the medium run. Note: This chart reports the estimated delay in return to work of the first cohort of treated mothers ($\hat{Y}$) compared to the last cohort of non-treated (Y), using older children as ineligible controls in the D-in-D design. The difference in employment probability is significant for age 3, 4 and 5. Source: LFS, 1993–1999

The remaining question is whether the negative impact on employment probability until age 6 reflects a decrease in the activity rate or an increasing unemployment rate. However, my analysis does not allow us to assert whether one of these two channels is significantly predominant; the labour supply channel is sizeable and significant for mothers with children aged 4, yet only the unemployment channel appears significant 1 year later. In a subsequent analysis extending the results of this study by using larger samples, Bicakova and Kaliskova (2016) show that higher post-PL exits to unemployment are mostly observed among lower-educated mothers, suggesting that the different transitions are to be associated with different socio-economic groups. We can indeed question the incentive provided by the Czech social security system with respect to these transitions. Guzi (2014) tests empirically for the link between social benefits and exits from unemployment, and concludes that a welfare trap exists, and that individuals who are low educated and have not worked prior to entering unemployment are less likely to transition back to employment. However, Guzi points out that as far as mothers of young children are concerned, the effect of the replacement rate on their transitions to employment is not significant and that therefore maternal returns to work seem to respond to other factors than social benefits.

6 Conclusions

Czech post-transitional family policy shifted away from the former emphasis on female labour market participation and public provision of childcare. The new policy orientation promoted family-based responses to social needs, and the 1995 reform of parental leave represented its epitome. The Act on State Social Support introduced an unexpected 12-month extension of universal parental benefits for all current and future recipients. This extension led to the duration of parental benefits outlasting the job-protected leave, leaving mothers to choose between 12 extra months of benefits or a secure post-PL return to work. I find a substantial impact of this reform on mothers' probability of employment after the end of parental leave. Depending on the empirical strategy within the difference-in-differences framework, the estimated decline in the employment probability of the first cohort of mothers exposed to treatment ranges between 15.5 and 23 percentage points. Expectedly, the decrease in employment probability is heterogeneous with respect to educational attainment. However, the correlation is not linear and high-skilled mothers are found to respond strongly and significantly to the incentive to remain inactive for an extra year, in spite of their comparatively low financial interest in doing so.

This reform had an explicit objective of withdrawing mothers from the labour market, as a short-term response to the threat of emerging mass unemployment, and the analysis confirms that the reform achieved its intended effect. However, the reform still appears to have a negative impact on the employment rates of the first eligible cohort of mothers 2 years after the end of the extended benefit payment, i.e., until their youngest child is 5 years old. By increasing the duration of career discontinuities, this medium-term effect is likely to weigh on mothers' subsequent wages and pensions. The lack of good quality data for this period is the major obstacle for analysing this turbulent post-transition legislation and its effects on labour market outcomes. This could

be a possible explanation of the scarcity of family and social policy evaluations in the Czech Republic, along with other Central Eastern European countries. Yet, the persistence of the phenomenon until the present day, more than a decade after joining the European Union and harmonizing international survey data, leaves a gap in the literature which begs to be filled.

The European social integration which followed the transition to market economy has indeed been yet another shock to local social and family policies, as the European Commission emphasized the importance of female labour market participation and public childcare services as tools for increasing mothers' labour supply. The Czech Republic as well as its CEE neighbours have responded half-heartedly to these harmonization recommendations, and further research is needed to assess whether and how work-family balance has been addressed by post-socialist countries within the enlarged European Union.

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Compliance with ethical standards

Conflict of interest The author declares that she has no conflict of interest.

Appendix

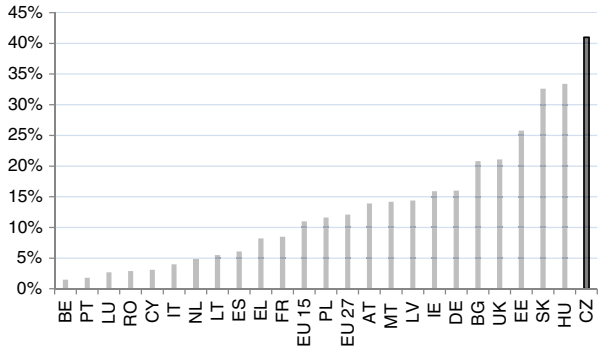

Fig. 5 Employment gap—women with children under 6 years old. Note: The maternal employment gap is defined as the difference in employment rates between women with and without children of pre-school age. Source: EU Labour Force Survey, in European Commission Indicators for monitoring the Employment Guidelines (2010)

Table 8 Summary statistics of the sample

	(A) Summary statistics (1)		(B) 1st approach			(C) 2nd approach	
Individual controls	Treated (Jan.–Sep. 1996)	Non treated (Jan.–Sep. 1995)	Reform cohort (1995–1996)	Control cohort 1 (1997–1998)	Control cohort 2 (1993–1994)	Eligible (age 3)	Non eligible (age 4)
Mean age	27.7	27.5	27.6	27.7	28.3 (***)	27.6	28.4 (***)
Age groups (%)							
5–24	25.3	29.6	27.4	27.2	21.9 (**)	27.4	21.2 (***)
25–29	40.4	36.2	38.3	41.7	45	38.3	42.7
30–39	34.3	34.2	34.3	31.1	33.1	34.3	36.1
Mean number of children	1.74	1.75	1.7	1.7	1.8	1.7	1.7
Number of children (%)							
1	44.3	39.7	42	42.8	33.8 (**)	42	40
2	40.6	46.6	43.6	45.1	52.3 (**)	43.6	48.3
3 and more	12.6	13.7	14.4	12.1	13.9	14.4	11.7
Married (%)	89.5	91.2	90.3	85.8 (***)	92.3	90.3	88
Educational level (%)							
None or elementary	8.5	9.6	9	10.3	10.9 (***)	9	6.7
High school, no graduation	44.6	39.5	42.1	43.5	41.4	42.1	42.7
High school graduated	38.5	41.2	40.2	39.4	37.4	40.2	42.5
Superior	8.4	9	8.7	6.8	10.2	8.7	8
Dependent variable							
Economic status (%)							
ML or PL 52	30.4 (***)	41.4	50.8 (***)	49 (***)	41.4	4.2 (***)	
Study 0.3	0.8	0.5	1	0.3	0.5	1	
Work	17.2	39.2 (***)	28	15.6 (***)	33.1	28	69.8 (***)
Unemployed	2.4	10.1 (***)	6.2	1.1 (***)	9.6	6.2	9.8 (***)
Homemaker	27.5	19.2 (***)	23.4	31 (***)	6 (***)	23.4	14.6 (***)
N	379	365	744	720	302	744	623

Significant differences between treated and controls are reported with the level of significance in parentheses.
Source: LFS, 1993–1999

Table 9 Impact of the 1995 reform on post-PL employment and activity

Linear probability model		
	(1) To be employed	(2) To be active
Treatment	−0.216***	−0.298***
	(0.0322)	(0.0343)
Education		
Graduated HS	*Reference value*	
None or elementary	−0.141**	−0.175***
	(0.0549)	(0.0625)
Did not graduate HS	−0.0488	−0.0947**
	(0.0358)	(0.0384)
Superior educ.	0.165**	0.109
	(0.0663)	(0.0660)
Married	−0.0120	−0.0507
	(0.0538)	(0.0593)
Age		
Age group 25–29	Reference value	
Age group 15–24	0.00246	−0.0508
	(0.0415)	(0.0462)
Age group 30–39	0.0406	0.00832
	(0.0397)	(0.0419)
Number of children		
1 child	Reference value	
2 children	0.0346	0.0250
	(0.0381)	(0.0414)
3 children	−0.0252	−0.00443
	(0.0541)	(0.0601)
Constant	0.393***	0.642***
	(0.0678)	(0.0736)
Observations	744	744
R^2	0.090	0.123

Robust standard errors in parentheses. This table reports the employment probability at the end of the PL, comparing treated (1996) and non-treated (1995) mothers, using age, education, matrimonial status and number of children as covariates. Source: LFS 1995–1996

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 10 Impact of the 1995 reform on post-PL activity difference-in-differences, first approach

	Dep. variable: to be active			
	(1)	(2)	(3)	(4)
Treatment	−0.329***	−0.321***	−0.254***	−0.229***
	(0.0464)	(0.0458)	(0.0832)	(0.0832)
Education				
Graduated HS		Reference value		
None or elementary		−0.186***		−0.223***
		(0.0384)		(0.0831)
Did not graduate HS		−0.106***		−0.129***
		(0.0259)		(0.0464)
Superior educ.		0.127**		0.177**
		(0.0498)		(0.0692)
Covariates	N	Y	N	Y
Control years	97–98	97–98	93–94	93–94
Bandwidth (quarters)	12	12	4	4
Observations	1464	1464	563	563

Robust standard errors in parentheses. This table reports the estimates for the employment probability at the end of PL, comparing reform cohort (95/96) and non-reform cohort (97/98 and 93/94), using age, education, matrimonial status and number of children as covariates. Source: LFS 1993–1998

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 11 Impact of the 1995 reform on post-PL activity

Difference-in-differences, 2nd approach		
	Dep. variable: to be active	
	(1)	(2)
Treatment	−0.178*	−0.177*
	(0.047)	(0.047)
Covariates	N	Y
Observations	1367	1367

Robust standard errors in parentheses. This table reports the estimates for the activity probability at the end of the PL, comparing eligible mothers (child aged 3) and non-eligible mothers (child aged 4) from the same cohort (95/96), using age, education, matrimonial status and number of children as covariates. Source: LFS 1995–1996

*$p < 0.01$

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