Is Longer Maternal Care Always Beneficial? The Impact of a Four-year Paid Parental Leave

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Abstract

We study the impact of an extension of paid family leave from 3 to 4 years on child long-term outcomes. Using a difference-in-differences design and comparing the first-affected with the last-unaffected cohorts of children, we find that an additional year of maternal care at the age of 3, which primarily crowded out enrollment into public kindergartens, had an adverse effect for children of low-educated mothers on human capital investments and labor-market attachment in early adulthood. The affected children were 12 p.p. more likely not to be in education, employment, or training (NEET) at the age of 21-22. The impact on daughters was larger and driven by a lower probability of attending college and higher probability of home production. Sons of low-educated mothers, on the other hand, were less likely to be employed. The results suggest that exposure to formal childcare may be more beneficial than all-day maternal care at the age of 3, especially for children with a lower socio-economic background.

JEL codes: J13, J18, J21, J24
Keywords: family leave, maternal care, subsidized childcare, child outcomes, human capital, labor-market attachment

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1 Introduction

Early childhood inputs have an important impact on children's development, human capital accumulation, and labor-market performance later in life (García et al. 2020). While there is substantial evidence that early maternal care is the most beneficial for child outcomes (Rossin-Slater 2018), other forms of childcare also become important as children grow older. pre-school education has been consistently found to improve child outcomes, especially of children from disadvantaged families (Felfe and Lalive 2018). Extended parental care that crowds out institutional childcare can have a negative impact on child development (Canaan 2022). The impact is likely to be higher for children of low-educated parents, who provide lower-quality childcare and tend to stay at home with children for longer periods (Pronzato 2009; Cohn, Parker, and Livingston 2014). Policies that offer well-intended family leave of several years may then impede inter-generational mobility and increase educational inequality.[1] Understanding the impact of prolonged parental care on long-term child outcomes is thus crucial not only for family leave design but also for policies affecting socio-economic mobility and inequality. Research on the effect of parental care at older ages is, however, scarce, as are the potential sources of exogenous variation in the duration of prolonged parental care.

In this paper we provide the first evidence on the impact of all day maternal care beyond a child's 3rd birthday on children's human capital investments and labor-market attachment later in life. We do so by exploiting the variation in the duration of maternal care induced by an extension of paid family leave from 3 to 4 years in the Czech Republic in 1995, which resulted in 30% of eligible mothers of 3-year old children staying at home rather than enrolling their children into public kindergartens.[2] We find that the extended maternal care had a substantial negative impact on long-term child educational and labor-market outcomes, especially for children of mothers with lower levels of human capital.

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[1]By family leave we denote maternity leave, parental leave or a similar policy tool designed to encourage parents to stay at home to care for their child.

[2]Attendance at widely available and financially affordable public kindergartens was the major alternative form of childcare to maternal care for the 3-year-olds prior to the reform.

The importance of all-day maternal care for child development seems to decrease with the child's age. While studies evaluating the impact of introductions or extensions of short family leave that increased exposure to maternal care during a child's first year of life find a positive or no impact on child outcomes (see for example Carneiro, Loken, and Salvanes 2015; Dustmann and Schönberg 2012; Huebener, Kuehnle, and Spiess 2019; Rasmussen 2010; Dahl et al. 2016), the findings of research analyzing the extensions of longer family leaves do not always confirm the positive impact of maternal care. The sign and significance of the estimated effects depend on leave duration, level of financial support, children's socio-economic background and – importantly - the counterfactual form of childcare provided in the absence of the maternal care induced by the family leave (Danzer et al. 2022). The scarce existing evidence suggests that inputs other than parental become increasingly important beyond a child's 2nd birthday, but knowledge of the effect of all-day parental care at later ages is lacking (see Rossin-Slater 2018; Huebener 2016 or Table A5 in Danzer et al. 2022 for an overview). While there is evidence on the adverse effects of maternal care at the age of 2 on children's test scores and schooling choices (Dustmann and Schönberg 2012; Canaan 2022), there is no research on whether these effects continue into adulthood. The present paper extends the current state of knowledge across these two dimensions: The 1995 family leave reform in the Czech Republic allows us to analyze the impact of all-day maternal care on three-year old children and focus on the long-term educational and labor-market outcomes when these children are in their early twenties.

The 1995 reform prolonged the duration of the paid parental leave from 3 to 4 years, keeping the amount of monthly flat-rate allowance basically unchanged. Paid leave takeup effectively required that the parent did not work and that the child did not attend an institutional childcare facility.[3] The reform came into effect on October 1, 1995 but also applied retroactively to all parents who were on leave on that date. Prior to the reform, the majority of children typically enrolled into widely available and financially affordable public kindergartens at the age of 3.[4] The reform thus postponed

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[3]See section 2.1 for details.

[4]In 1995, there were basically no other forms of public or private childcare demanded or available

kindergarten enrollment of children whose mothers prolonged their parental leave by at least one year. All mothers with a youngest child of at most 3 year-old were eligible for the extended leave. The takeup was substantial and fairly universal: an additional 30% of mothers with the youngest child at the age of 3 at the time of the reform prolonged their parental leave beyond the child's third birthday, and there was almost no difference in compliance by mothers' level of education (Bičáková and Kalíšková 2019).

In order to estimate the impact of maternal care at the age of 3, we apply a difference-in-differences approach and compare the outcomes of the first cohort of children affected by the 1995 reform with the last-unaffected cohort. We focus on the long-term education and labor-market outcomes measured in early adulthood and explore the potential heterogeneity of the estimated impact across different levels of parental human capital and by child gender. We find that the extension of paid family leave from 3 to 4 years had a sizeable negative impact on educational attainment and labor-market attachment in early adulthood, especially of children with mothers with a lower level of human capital. In particular, the children affected by the reform are more likely not to be in education, employment, or training (NEET) at the age of 21-22 by about 4 p.p. This effect is driven by children with a lower socio-economic background, whose probability of being NEET rose by as much as 12 p.p. The negative impact on daughters is larger and reflects both lower human capital investments and weaker labor-force attachment. The affected daughters of low-educated mothers are less likely to attend college by as much as 20 p.p. and are more likely to do housework. Sons of low-educated mothers, on the other hand, are less likely to be employed. Our findings are robust to whether the long-term outcomes of our treatment and control groups are compared at the same age or at the same calendar time. Our placebo test and checks for potential sample selection bias also confirm that the impact we find can be attributed with a high level of confidence to the effect of the 1995 reform.

To the best of our knowledge, this paper provides the first evidence on the impact of paid family leave longer than 2 years on long-term child outcomes. While Dustmann

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in the Czech Republic. See section 2.4 for details.

and Schönberg (2012) and Canaan (2022) evaluate 3-year long leaves, they focus on short- or medium-run outcomes. Canaan (2022) estimates the effect of an extension of the duration of paid leave for up to three years on second-born children in France and shows that it harms children's verbal development at the age of 5-6. Dustmann and Schönberg (2012) find a negative impact of an extension of unpaid leave from 1.5 to 3 years in Germany on the probability that the child attends a school that streamlines children for university at the age of 14. Our paper differs from these studies in both the institutional setup (evaluating a 4-year paid leave) and in the focus on long-term outcomes. Our results are in line with the adverse effects of prolonged maternal care on child development documented in Canaan (2022) and Dustmann and Schönberg (2012). However, we find an effect of maternal care at the age of 3 on college enrollment that is about 20 times larger than that of maternal care at the age of 2 on the probability of attending an academic track at high school, the outcome that is most comparable to ours, estimated in Dustmann and Schönberg (2012).

Danzer et al. (2022) is the only study besides ours that provides evidence on the impact of a paid leave beyond child's 1st birthday on long-term educational and labor-market outcomes, but the leave extension in Austria they evaluate is only to 2 years, compared to 4 in our setting. They estimate the impact of maternal care in the second year of life on health outcomes, the probability of being in education, and a series of labor-market outcomes at the ages of 17 and 23.[5] They focus on the counterfactual type of care that the impact of the extended maternal care is compared to, dividing their sample into a group where maternal care was more likely to replace care provided in nurseries and another group where it replaced informal care mostly provided by grandparents. For the first group, which is comparable with our setting, they find zero impact of all-day maternal care at the age of 1 on long term child outcomes.[6] Given the limited availability of formal childcare for 1-year old children in Austria prior to the reform, however, this group represents only a small share of their population. While only about

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[5]While they also have wage information and health status, they do not observe the level of education and therefore cannot analyze the probability of high-school completion and college enrollment as we do.

[6]For the second group, they find a positive impact of the leave extension on all long-term child outcomes, suggesting that maternal care there was superior to the informal care provided by grandparents.

2% of communities had nurseries when the reform came into effect in Austria, prior to the Czech reform we evaluate, public kindergartens were available in all municipalities with more than 2000 inhabitants and in 75% of the smaller municipalities, providing formal childcare to the majority of 3-year old children (Kuchařová et al. 2009).

As in Danzer et al. (2022), we are able to explore the heterogeneity of the impact by the quality of maternal care, measured by mother's education but, simlarly to previous research, we have no information about the heterogeneity in the quality of formal child-care.[7] Anecdotal evidence, however, suggests that there was substantial homogeneity in the care provided by the public kindergartens in the 1990s in the Czech Republic, as they were all under the jurisdiction of the Ministry of Education and shared the same centrally-determined curriculum.

Similarly to the previous research (Dustmann and Schönberg 2012 and Canaan 2022) we do not observe the actual takeup of the leave by mothers of children whose outcomes we analyze, and therefore estimate the intention to treat (ITT) effect of the 1995 family leave extension.[8] We provide evidence on the takeup by estimating the impact of the reform on the probability of an eligible mother staying at home after the leave extension. The eligibility (universal among mothers with a youngest child aged 3) and the takeup of the 1995 leave extension (by 30% of these eligible mothers) are either comparable to the previous research or greater. We also explore potential heterogeneity in takeup but find little evidence of selectivity in terms of education or other observable characteristics of the complying mothers.

So why do we find much larger adverse effects of maternal care on long-term child outcomes than the related previous research? We attribute this mostly to the differences in the ages at which the child is exposed to the maternal care, the time when the child outcomes are measured, and the counterfactual care against which the impact of maternal care is estimated. Danzer et al. (2022) evaluates the exposure to maternal care

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[7]Danzer et al. (2022) use the presence of a nursery in the municipality as a proxy for formal childcare availability but they do not consider potential differences in the quality of the care across the nurseries.

[8]While Danzer and Lavy (2018) also produce only ITT estimates, Danzer et al. (2022) observe the actual family leave takeup and estimate the LATE using an IV approach. Their supplementary analysis of educational outcomes on PISA scores is, again, restricted only to the ITT estimates.

much earlier than we do (in the second year of life). Although Dustmann and Schönberg (2012) and Canaan (2022) focus on the third year of life, which is closer to our setup, they estimate the impact of maternal care against informal care as the dominant alternative.[9] In contrast, we estimate the impact of maternal care in a child's fourth year of life compared to the impact of formal childcare provided in widely available and financially affordable public kindergartens.

This relates our paper to a second strand of literature that studies the impact of universal childcare reforms on child outcomes, surveyed by Felfe and Lalive (2018) and Dietrichson, Lykke Kristiansen, and Viinholt (2020). In contrast to the existing family leave studies, this research also provides evidence on the impact of the type of care a child is exposed at the ages of 3-6 on long-term child outcomes, and unanimously concludes that universal childcare has a positive effect on school progression, educational attainment and labor-market outcomes of, in particular, children with a low socio-economic background (Dietrichson, Lykke Kristiansen, and Viinholt 2020). Our estimates of the impact of maternal care at the age of 3 replacing the care provided in public kindergartens turn out to be remarkably similar - both in sign and magnitude - to the effects of a large-scale expansion of subsidized formal childcare studied by Havnes and Mogstad (2011), which mostly substituted (non-maternal) informal care for 3-6 year olds, on children's educational and labor-market outcomes.

Given that we find a very similar impact, just with an opposite sign, of replacement of subsidized formal childcare with maternal care at the age of 3, we extend this line of research by confirming a positive impact of pre-school education on children not only relative to informal care but also when compared to the care provided by a child's mother. Similarly to ourselves, Havnes and Mogstad (2011) also find that children with low-educated mothers benefit the most from attending subsidized formal child care. The positive effect of attending formal childcare at the age of 3, driven mostly by children with low-educated parents, has also been confirmed by Felfe, Nollenberger, and Rodríguez-Planas (2015) and Havnes and Mogstad (2015).

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[9]Moreover, none of the two papers look at long-term outcomes and Dustmann and Schönberg (2012) evaluate the impact of unpaid leave, in contrast to other studies, including ours.

While 4-year family leave is rare,[10] all day maternal care beyond a child's 3rd birthday is a not an exceptional phenomenon, especially among families with lower socio-economic status (Cohn, Parker, and Livingston 2014), and the knowledge of its impact on children is important for addressing inequality and social mobility issues. Mothers who stay at home for 4 years, however, are self-selected and much more likely to either prolong their leave even further or permanently withdraw from the labor force (Bičáková 2016), which makes it impossible to evaluate the impact of all-day maternal care at the age of 3 on long-term child outcomes separately from exposure to maternal care at later ages. The institutional context of the 1995 Czech family leave reform seems exceptionally useful for this research question. The Czech Republic is a country that combines a very long family leave (with a very high takeup rate) and a traditionally strong overall attachment of women to the labor force (a heritage of the Communist regime). While the 1995 reform induced a high share of mothers to stay at home with their children for more than three years, the majority returned to the labor force by the time their youngest child was seven (Bičáková and Kalíšková 2019), allowing us to interpret our findings as the impact of an extension of temporary (rather than permanent) maternal care beyond a child's third birthday.

Similarly to previous research, our data do not allow us to shed more light on which early childhood inputs are lacking in the children exposed to maternal care instead of attending kindergartens at the age of 3, or which skills are most affected and what the mechanisms are that lead to the negative impact on the long-term outcomes. At the end of the paper, we consider several long-term demographic outcomes available in the data that could help us better understand what drives our findings. None of these outcomes, however, can explain our results. In particular, we find no impact of the 1995 reform on nest-leaving, marital status, cohabitation or fertility decisions of the affected children. Future research with richer data is needed to better understand the mechanisms that drive our results.

In line with previous studies we attribute the negative impact of the family leave

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[10]Note that while the Czech family leave system has undergone many changes since 1995, the 4-year paid leave still remains an option.

extension on long-term child outcomes to the prolonged maternal care at the age of 3, which, in our case, crowded out time spent in pre-school education. This is also supported by a direct comparison of our estimates with the impact of the expansion of subsidized universal childcare at the ages of 3-6 documented by Havnes and Mogstad (2011), whose estimates (of the effect of pre-school education crowding out informal care) are very similar in magnitude to ours, just oppositely signed. There may be, however, other mechanisms at play behind our findings, specifically the indirect effects of the family leave reform on maternal and other family outcomes, which also affect child development (Canaan 2022). While there is evidence that the 1995 reform had a negative impact on mothers' immediate post-leave labor-market attachment, these effects seem to have disappeared by the time children entered school (Bičáková and Kalíšková 2019). We also check the potential persistence of these negative immediate impacts by estimating the impact of the 1995 reform on labor-market outcomes of mothers of the affected children when they were 16-17, the latest age when maternal outcomes are observable for almost the whole sample. Finding zero impact confirms that the indirect effects of the family leave extension, if any, are likely to be of second-order importance.

Putting our paper into the context of the existing research, our findings contribute to three different strands of the literature: First, we extend the research evaluating the impact of family leave reforms on child outcomes (as surveyed by Rossin-Slater 2018; Huebener 2016 or Table A5 in Danzer et al. 2022) by providing evidence on the impact of a 4-year long paid family leave on long-term child outcomes. Second, we complement the studies of universal childcare reforms (as surveyed in Felfe and Lalive 2018) by estimating the impact of a postponement of formal pre-school education until the age of 4. Thanks to the specific institutional setting, we are able to compare the impact of the two dominant forms of childcare at the age of 3 prior to and after the reform: maternal care versus formal childcare in public kindergartens, and thus provide new evidence which connects the two different lines of research. Finally, the evidence on the impact of being exposed to an extra year of maternal care instead of attending a public kindergarten at the age of 3 also contributes to the research on the impact of

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early childhood inputs on long term child outcomes along the lines of e.g. García et al. (2020). While we use a fundamentally different methodology, our estimates may serve as inputs for the structural models of skill production used to analyze children's outcomes over their lifecycle in this strand of the literature.

The paper is organized as follows: Section 2 describes the institutional background and Section 3 discusses the previous evidence from the related strands of research. Section 4 covers the theoretical background of our analysis, surveys the scarce evidence on the impact of the 1995 reform on mothers and outlines the expected outcomes. Section 5 describes the data and methodology and Section 6 presents our estimation results. Section 7 focuses on our interpretation and the mechanisms and in Section 8 discusses the identification assumptions and sensitivity of our results. Section 9 concludes.

2 Institutional Background and Reform Compliance

2.1 Family Leave Policies

Family leave policies in the Czech Republic include job protection, maternity benefits, and parental allowance. Parents are eligible for job-protected leave until their child's 3rd birthday.[11] The job protection period was introduced in 1990 and has been maintained at 3 years duration since then.

Mothers who were employed for at least 270 days in the 2 years prior to a child's birth are entitled to receive maternity benefits for 28 weeks (starting 6 to 8 weeks prior to birth). Maternity benefits pay 70% of a woman's salary from the last 12 months prior to the commencement of maternity leave. There have been no substantial changes to maternity benefits since 1990.

A parent caring for a child (the youngest in the family) is also eligible for parental allowance, a non-means-tested flat rate benefit. The parental allowance starts either immediately after maternity benefits end or immediately after childbirth if the mother

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[11]Employees with a permanent contract are eligible for job protection. An employee with a fixed-term contract is eligible for job protection up to the date of contract expiration.

is not eligible for maternity benefits.[12]

The eligibility criteria for parental allowance at the time of the analysis required that the parent's earnings were below a certain threshold (in 1995, the threshold was CZK 1800 per month—less than one fifth of the average female wage then) and that the child did not attend a childcare facility. Given these conditions and a very limited availability of part-time jobs in the Czech Republic, collecting parental allowance in the 1990s implied that the parent was at home caring for a child.

2.2 The 1995 Parental Leave Reform

Until 1995, the receipt of parental allowance coincided with the 3 years of job protection. In October 1995, the receipt of monthly parental allowance was prolonged until the child's 4th birthday, exceeding the unchanged duration of job protection by 1 year.[13] All parents with children under 3 as of October 1, 1995 were eligible for the prolonged parental allowance. There was also a slight increase in the monthly allowance payment from CZK 1,740 to CZK 1,848 in 1995,[14] as part of the gradual valorization of the monthly allowance over the 1990s and early 2000s, but this change was negligible relative to the amount that the parents gained by staying at home with a child for one more year and collecting an additional 12 monthly parental-allowance benefits (see Figure 1). The total allowance available to a mother of a newborn thus increased by CZK 26,064 (about 700 EUR) and for a mother of a child who just turned 3 by CZK 22,176 (about 600 EUR). In terms of the net income effect on a mother of a 3-year old child, who would have returned to employment after the 3-year leave prior to the reform and who earned an average wage: the extension of the leave until the child's 4th birthday caused a net income loss of CZK 88,704 (about 2,400 EUR), corresponding to an 80% drop in annual income in the fourth year after childbirth.

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[12]There was no paternity leave until 2018, when a 7-day paternity leave was introduced. Parental allowance can, in principle, be received by a father but this is still very rare (0.78% of parental allowance recipients were fathers in 2001; earlier data are not available).

[13]The extension of parental allowance came about unexpectedly. It was added to the Act on State Social Support during the legislative process. The bill was passed by Parliament on May 26, 1995.

[14]The monthly allowance - approximately 50 euros - corresponded to about one fifth of an average wage in the economy.
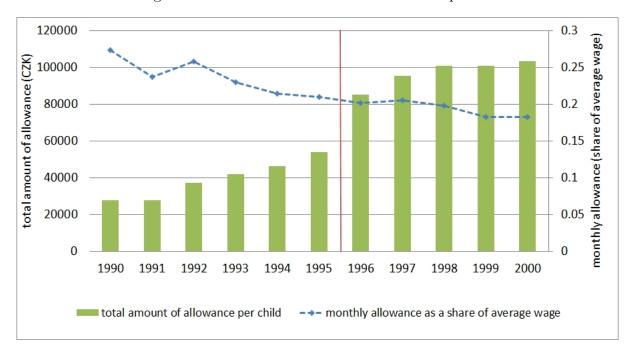
Figure 1: Parental allowance in the Czech Republic

Note: The left vertical axis shows the total amount of parental allowance per one child. All values are nominal amounts in current CZK. The right axis shows the monthly parental allowance amount as a share of average wage in the economy in a given year.

Mothers' compliance with the 1995 reform is evidenced in Figure 2, which depicts the evolution of the share of inactive mothers by the age of their youngest child before and after the reform.[15] The steep rise in inactivity of mothers of 3-year olds suggests that almost 40% of mothers whose youngest child was 3 decided to stay at home until the child's 4th birthday in response to the reform. As kindergartens start providing childcare in September, prior to the reform some mothers of children who turned 3 by the end of the calendar year used to enroll their children into kindergartens and return to work before the child's 3rd birthday. As the duration of the parental allowance was extended to 4 years, the child's enrollment and mother's return to work was shifted by 1 year, so that the inactivity of mothers of 2-year old children (who were soon to turn 3) also rose. There was also a slight rise in inactivity of mothers of 4 and 5-year old children, suggesting that some mothers prolonged their leave even beyond the 4 years of paid parental leave. The substantial increase in inactivity of mothers with a youngest

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[15]Parental allowance receipt (and the conditions restricting mother from working and the child from attending formal childcare) are always linked only to the youngest child.
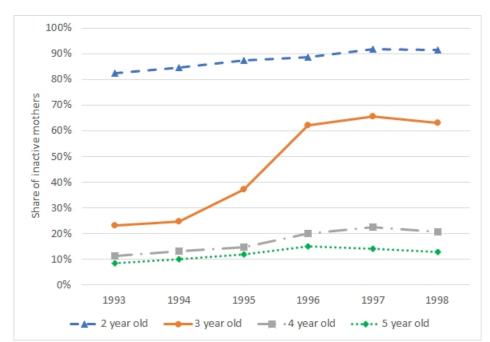
Figure 2: Share of inactive mothers by the age of their youngest child, 1993-1998

Note: The figure illustrates share of mothers that are economically inactive (out of labor force) by the age of their youngest child in the 1990s.
Source: Czech Labor Force Survey data (1993-1998), own calculations weighted by population weights.

child aged 3, documented in Figure 2, has also been confirmed in our earlier work on the impact of the 1995 reform on labor-market outcomes of mothers with children aged 3-5 (Bičáková and Kalíšková 2019). Section 6.1 presents the results from the estimation of the impact of the 1995 reform on inactivity of mothers with a youngest child aged 3, which documents the extent of the actual treatment of having a mother at home for an additional year until the child's 4th birthday, and serves as the first stage of our ITT analysis of the reform's effect on long-term child outcomes.

Finally, Figure 3 addresses the question of whether all mothers who complied with the 1995 reform and extended their leave beyond the child's third birthday stayed at home for a similar period of time or if the increase in the share of inactive mothers of 3-year olds masks heterogeneity in the prolongation of leave. The share of inactive mothers by the youngest child's age, measured in quarters of years (the most detailed age information we are able to derive from the data) in 1994 and 1996 move almost in parallel between the ages of 3 and 4, suggesting that all mothers who extended their leave in response to the 1995 reform stayed at home for about three quarters of their
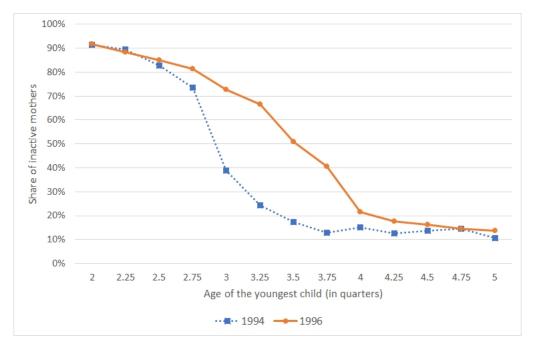
Figure 3: Share of inactive mothers by the age of their youngest child in quarters

Note: The figure illustrates the share of mothers that are economically inactive (out of the labor force) by the age of their youngest child (reported in quarters of a year) in the 1994 (before the reform) and 1996 (after the reform).
Source: Czech Labor Force Survey data (1994, 1996), own calculations weighted by population weights.

child's fourth year or more.

2.3 The Childcare System and Pre-school Enrollment

Public childcare in the Czech Republic consists of nurseries for children aged 1-2 and kindergartens for children aged 3-5. Compulsory school attendance starts on September 1 after the child's 6th birthday.

The number of nurseries, which operated as medical facilities under the jurisdiction of the Ministry of Health, was substantially reduced at the beginning of the 1990s.[16] The closures of nurseries were accompanied by a broader change in family policy in the 1990s, which promoted conservative values and encouraged women to stay at home after childbirth to raise their children (Saxonberg and Sirovátka 2006). The 1995 reform was part of this policy change.

In contrast with nurseries, the number of kindergartens was affected much less by

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[16]While 15% of children aged 1-2 attended nurseries in 1990, in 1992 it was only 5%, and by the end of the 1990s it was just 1%.

the conservative policies of the early 1990s. They were widely available and financially affordable for the vast majority of parents.[17] The slight reduction in the supply of kindergartens in the 1990s was more than offset by a large drop in fertility, resulting in a steady increase of the overall kindergarten coverage for children aged between 3 and 5 in the 1990s (see the grey bars in Figure 4).[18]

As eligibility for parental allowance required that the child was not enrolled in a childcare facility, the duration of paid parental leave and its takeup had a direct impact on the youngest child's nursery and kindergarten attendance.

Figure 4 shows the evolution of the total number of children attending public kindergartens by age as of September 1 of a given year. Prior to the 1995 reform, the majority of 3, 4 and 5 year old children attended kindergarten. While the kindergartens are designed mainly for 3-5 year olds, children between the ages of 2 and 6 can attend.[19] The low share of 2-year old children is driven by the fact that children of mothers who took up the 3-year family leave were not eligible to attend a kindergarten.

After the 1995 reform, children of mothers who took the extended family leave for the 4 years could not start attending pre-school education until the age of 4. The steep drop in kindergarten enrollment, among the 3-year-old children by over 25% and 2-year-old children by almost 50%, between 1994 and 1996 in Figure 4 shows that the 1995 reform substantially affected not only mothers' inactivity but also their children's kindergarten attendance.[20]

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[17]As public kindergartens are largely subsidised by the government, the cost of this form of childcare is very low. The kindergarten fees constituted only 2.7% of the net income of an average family in 2006 (earlier data are not available, Kuchařová et al. 2009). The geographical availability of kindergartens was also high according to Kuchařová et al. (2009), who showed that in 2002 (the earliest available statistics) kindergartens were available in all municipalities with more than 2000 inhabitants and in 75% of the smaller municipalities.

[18]The 1990s were marked by a steep decline in fertility rates in most transition economies (Sobotka 2003), the Czech Republic being no exception. Sobotka (2003) argues that in central Europe, the decline in fertility was mainly caused by postponement of parenthood (the average age of a Czech women at first birth increased from 22.6 in 1993 to 24.6 years in 1999).

[19]As kindergartens start in September, children who will turn 3 by the end of the calendar year, are allowed to enrol at the beginning of the academic year when they are still 2, provided there are enough places. Children who are not sufficiently physically or mentally mature can postpone their school starting age (conditions for such postponement are defined by law), so there can be 6 + year olds attending kindergarten.

[20]Although the decrease observed in kindergarten enrollment also partially reflects the gradual decline in fertility, the population of 2 and 3 year-old children decreased by only 8% between 1994 and 1996.
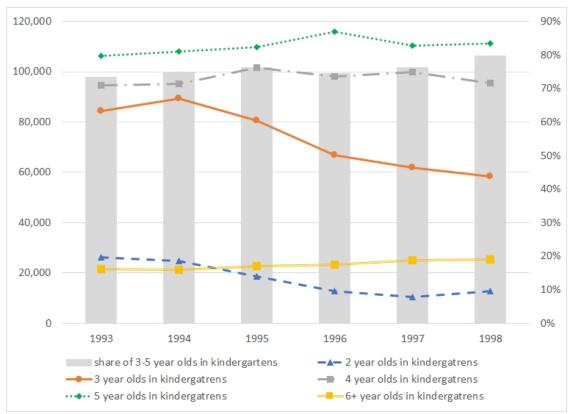
Figure 4: Kindergarten attendance by age

Note: The figure shows the total number of children enrolled in public kindergartens by their age (age is reported as of September 1 in each calendar year) on the left vertical axis. The right vertical axis illustrates the ratio of children aged 3-5 enrolled in public kindergartens to all children aged 3-5 in the population (shown in grey bars).
Source: Ministry of Education, Youth, and Sports and Czech Statistical Office, own calculation.

Interestingly, the total number of children aged 4 and 5 attending public kindergartens remained fairly stable over the period, suggesting that the children who stayed at home with their mothers at the ages of 2 and 3 after the 1995 reform also attended kindergarten, but only at a later age.[21] The share of 6-year old children and older in kindergarten also did not change much, implying that the postponed kindergarten enrollment after the 1995 reform did not result in later enrollment into school.

The evidence presented in Figure 4 suggests that the 1995 reform shifted enrollment into public kindergartens from the age of 3 to 4 and shortened the overall time spent in pre-school education, rather than postponing the school starting age or excluding some children from pre-school education altogether.

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[21] For example, from the first-affected cohort of children (born in October 1992 to September 1993), only 55% of children attended kindergarten when they were 3, but this quickly increased to 82% and 91% when they were four and five, respectively.

Over the 1990s, the aggregate trend in the share of children enrolled in kindergartens among the 3-5 year olds from 73% to 80% remained fairly stable, with the slight increase mostly reflecting the drop in the denominator due to the decline in long-term fertility. The steadily high shares confirm that pre-school education in public kindergartens was the dominantly used type of childcare for that age group.

2.4   Alternative forms of Childcare

Prior to 1995, the primary form of childcare at the age of 3, when the job protection expired and mother's paid parental leave ended, were public kindergartens, as shown in Figure 4. Alternative forms of childcare for children aged 3-5 were still very rare even at the end of 1990s. Kuchařová et al. (2009) surveyed a representative sample of Czech municipalities in 2002 and found that there were only 3 private kindergartens, 8 childcare agencies, and 4 self-help organizations focused on childcare in the 497 municipalities.[22] Another survey of parents from 2005 confirms the lack of availability and use of private childcare—only 2% of parents with children aged 1-10 mentioned that they sometimes used a childminder to help them with childcare and most of them used this form of childcare only sporadically, not on a daily basis (Ettlerová et al. 2006). It was more common to use the help of grandparents or other relatives - 20% of parents with children aged 1-10 mentioned that grandparents helped them with childcare on a regular basis during weekdays, but it is unlikely that they would provide daily full-time care (Ettlerová et al. 2006).[23]

The only two sources of data we have on the use of different types of childcare for 3-year old children are the share of 3-year olds attending public kindergarten and the share of mothers of 3-year olds who are at home and can thus be expected to be caring for their children. While in 1994 67.5% of 3-year-old children were enrolled in public kindergartens and 41% of mothers of 3-year olds were inactive, this reversed to 54.9%

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[22]Earlier data are not available, but it is very likely that these organizations were even more scarce in the 1990s when the market was still emerging.

[23]Given the high share (almost 70 %) of children aged 3 enrolled in kindergartens before the 1995 reform (see Figure 4), the care from grandparents was probably mostly used for picking children up from the kindergartens and for afternoon activities.

versus 68.8% respectively in 1996, implying a more than 12 p.p drop in kindergarten attendance and 28 p.p. rise in inactivity of mothers of 3-year olds.[24] These numbers confirm that 3-year old children in the Czech Republic in the 1990s either attended public kindergartens or their mothers were at home, implying that they were exposed to maternal, rather than any other form of childcare.

The limited information on the use of alternative forms of childcare in the 1990s suggests that the majority of children whose mothers decided to stay at home for 4 years instead of 3 after the 1995 reform would otherwise have attended public kindergarten at the age of 3. Therefore, care provided in public kindergartens represents the main counterfactual to the extended maternal care, the impact of which we estimate. While the quality of the care provided in the public kindergartens may vary across different institutions, the counterfactual is still likely to be much more homogeneous than in previous research from settings with various alternative forms of childcare, both public and private.[25] As public kindergartens constitute the main pillar of the pre-school educational system under the jurisdiction of the Ministry of Education, they are bound to follow the same curricula and unified educational approach, and can thus be expected to provide a rather similar type of care across the different institutions.

3   Previous Evidence

There are two separate lines of research that explore how child outcomes are affected by the two major forms of pre-school childcare: maternal care and formal childcare. The first line of research consists predominantly of studies that evaluate the impact of changes in family leave policies. The second line of research exploits universal childcare

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[24]The data on kindergarten attendance comes from the Ministry of Education, Youth, and Sports and the Czech Statistical Office; the share of inactive mothers are our own calculations from LFS data. The numbers exceed 100% for two reasons. Firstly, kindergarten attendance by age is measured in September of the given year, whereas mothers' inactivity is calculated based on an average of the quarterly data. Secondly, mothers of 3-year olds may be at home with subsequent children, younger than 3, while the 3-year olds attend kindergarten.

[25]Unfortunately, no data on the kindergartens' quality is available from the 1990s. Danzer et al. (2022) is the only study that estimates the impact of family leave separately for different alternative types of childcare. They proxy the use of public care with a local presence of a nursery, without controlling for the quality of the care provided there.

reforms to evaluate the impact of pre-school programs on child outcomes.

3.1 Studies Evaluating Family Leave Reforms

Most of the literature on the impact of maternal care on child outcomes exploits the variation in the duration of maternal care induced by changes in family leave policies. These studies, surveyed in Rossin-Slater (2018), Huebener (2016) or Table A5 of Danzer et al. (2022), find a positive impact on a set of short- and long-term child outcomes after the introduction of a short paid or unpaid family leave (see e.g. Albagli and Rau 2019; Carneiro, Loken, and Salvanes 2015 or Rossin-Slater 2011) but the evidence on the effect of the extension of an existing leave to over 1 year is mixed, with positive or zero impact (citeNPBaker2015; Dahl et al. 2016; Rasmussen 2010; Danzer and Lavy 2018; Danzer et al. 2022) and negative impact of a leave longer than 2 years (Dustmann and Schönberg 2012 and Canaan 2022). The impact of the family leave policies on child outcomes varies by the children's family background and mothers' education but the literature, again, does not reach a consensus.[26] Some studies also show a differential effect by gender of the child (Danzer and Lavy 2018). Danzer et al. (2022) emphasize the importance of the counterfactual type of care that the impact of maternal care is estimated against. They find a positive effect of maternal care induced by family leave extension when it substitutes other informal care, but not when it replaces formal childcare.

Zooming in on the impact on long-term educational and labor-market outcomes of children that we analyze in this study, the previous findings also appear to be conflicting: On the one hand, Carneiro, Loken, and Salvanes (2015) find a decline in high school drop-out rates and increase in wages at the age of 30 as a result of an introduction of a 4 month paid leave and a simultaneous extension of an unpaid leave from 3 to 12 months. On the other hand, Rasmussen (2010) finds no impact on long-term educational outcomes of an extension of family leave from 14 to 20 weeks, and Dahl et al. (2016) document no impact on children's schooling of an extension of paid maternity leave to

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[26]While Danzer and Lavy (2018), Rossin-Slater (2011), and Liu and Skans (2010) find a positive effect of family leave on children of high-educated mothers, Carneiro, Loken, and Salvanes (2015) and Albagli and Rau (2019) show that it is the children from disadvantaged families who benefit most.

35 weeks. Danzer et al. (2022) find a positive effect of a leave-extension from 1 to 2 years on educational and labor-market outcomes when maternal care replaces other informal care. Finally, Dustmann and Schönberg (2012) find no effect of shorter leave expansions on child outcomes, but a negative impact of an extension of unpaid leave from 18 to 36 months.

There is evidence of heterogeneity by parental education, socio-economic background and gender of the child but, again, the findings are mixed: Liu and Skans (2010) study an extension of paid parental leave from 12 to 15 months on school outcomes at the age of 16 and find improvements in test scores and grades, but only among children of high-educated mothers. Danzer and Lavy (2018), on the other hand, study an extension of paid family leave from 12 to 24 months on PISA test scores at the age of 15 and find no overall impact, which masks a positive effect on boys of high-educated mothers and a negative effect on boys of low-educated mothers.

Skill formation is a complex production process, where early human capital investments affect returns to later investments, and where timing of the inputs via various forms of childcare also matters (Cunha and Heckman 2007). Different types of skills (cognitive, non-cognitive or social) develop at different stages of early childhood, so the same factors play a substantially different role in skill formation depending on a child's age (Cunha and Heckman 2008). The importance of social interactions with peers and adults other than parents (as opposed to exposure to all-day maternal care) also varies across different stages of child development.[27] An extra year of maternal care will have a very different effect on a newborn than on a two-year old child. The diverse impact of maternal care at various stages of child development is probably also driving at least part of the heterogeneity in the findings of previous studies about the impact of family leaves with different lengths, with a positive impact of (introduction of) shorter leaves, zero effect of shorter extensions of existing leaves, and a negative impact of leaves of several years. Our setup falls into the last category, which is populated by just very few

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[27]Dustmann and Schönberg (2012) suggest (based on their findings of adverse effects of a 3-year long unpaid leave) that children older than 18 months benefit from the stimuli that caregivers other than their mother provide. Bono et al. (2016) shows that the positive impact of maternal time inputs on cognitive development of children at ages 3-7 declines with the child's age.

studies that evaluate the impact of an unpaid leave of a duration of 3 years at maximum (Dustmann and Schönberg 2012) and paid leave of a duration of 3 years (Canaan 2022). To the best of our knowledge, there is no evidence on the impact of a 4-year paid family leave on child outcomes. Dustmann and Schönberg (2012), which is the study most similar to ours from this strand of literature, document a negative impact of an extension of unpaid leave to 3 years on the probability that a child attends a high-track school (that streamlines children for university) of 0.6 percentage points. While this is the closest finding from this literature that our results can be compared to, the impact of a 3-year unpaid leave is likely to differ substantially from that of a 4-year paid leave.

3.2 Studies Evaluating Early Childcare Reforms

The evidence on the impact of formal childcare on children's development is surveyed in Felfe and Lalive (2018), who discuss separately studies focusing on younger (0-2) and older (3-6) children. The effect of non-parental care on younger children depends on the relative quality of the universal childcare and maternal care: While studies that evaluate the impact of lower-quality formal care find negative effects on children's development, especially for children from a higher socio-economic background (e.g. Fort, Ichino, and Zanella 2020 or Herbst 2013), studies of high-quality universal childcare find a positive impact on child outcomes, especially for children from disadvantaged families (e.g. Drange and Havnes 2019 or Noboa-Hidalgo and Urzúa 2012). The findings on the impact of early formal childcare are therefore inconclusive and, to some extent, in line with the results of the studies of family leave reforms discussed above.

The empirical evidence on the impact of universal childcare on pre-school children (3-6) is much more unanimous, revealing either a zero or mostly positive effect on children's development (Felfe and Lalive 2018). Dietrichson, Lykke Kristiansen, and Viinholt (2020) survey 26 studies on the impact of universal pre-school programs, mostly for older children (3-6), on long-term child outcomes and conclude that universal childcare has a positive average effect on school progression, educational attainment and labor-market outcomes, in particular for children with a low socio-economic background. Focusing

specifically on the impact of universal childcare on children at the age of 3 on children's educational outcomes and labor-market performance, which is the evidence most relevant for our analysis, the literature documents a positive overall effect: Felfe, Nollenberger, and Rodríguez-Planas (2015) find that an introduction of universal high-quality childcare at the age of 3 (which replaced full-time maternal care) had a sizable positive effect on children's school outcomes, especially for girls and for children from disadvantaged families. Havnes and Mogstad (2011) find that the introduction of subsidized universal childcare for children aged 3-6 (which mostly replaced other informal care) had a positive effect on human capital investments as well as labor-market outcomes of children in their early thirties. In particular, attendance at formal childcare increased the probability of attending college by 7 p.p. and reduced the probability of being on welfare by 5 p.p. among affected children. The increase in educational attainment was most pronounced among children with a low socio-economic background, whereas improvements in labor-market outcomes were primarily experienced by girls. Havnes and Mogstad (2015) further explore the non-linearity of the impact of the preschool childcare on earnings and show that the positive effect is driven by individuals from the lower and middle parts of the earnings distribution, who are often children of less-educated parents, whereas individuals from the upper part of the earnings distribution, who are typically children of high-educated parents, experience a drop in earnings as a result of attendance at universal childcare.

There is a separate strand of literature that estimates structural models of skill formation and human capital production functions over the life-cycle (starting with Cunha and Heckman 2007 and Cunha and Heckman 2008), which are sometimes used to evaluate the impact of various early childhood interventions on skill formation, subsequent human capital investments and long-term outcomes. Among the most recent and the most relevant studies, García et al. (2020) assess the long-term benefits of an early childhood program for children between 0 and 5, targeted at disadvantaged families, which was evaluated by random trial, and which followed the participants to adulthood. They find a substantial positive impact on health, children's future labor income, education,

and (absence of) criminal behavior, with the effects being larger for boys, and a positive impact on mothers' labor incomes. While this research focuses on specifically targeted smaller-scale childcare programs and uses a different methodology to ours, the structural models in some of these studies, which we refer to in due course, help us provide insights into the mechanisms behind the effects that we estimate.[28]

3.3   Benchmarks for our Analysis

The common challenge faced by the two strands of literature is the uncertainty concerning the counterfactual, i.e. the alternative form of childcare the children would have been exposed to instead of maternal care (in the family leave studies) or formal childcare (in the universal childcare studies). When a family leave and universal childcare are simultaneously available (and maternal and formal childcare represent the two major types of care), the findings of the two lines of research about the impact of the type of childcare on child outcomes should be a mirror reflection of each other.[29]

This is the case in our setup. As explained in Section 2.4, the dominant alternative to the extra year of full-time maternal care induced by the 1995 reform was childcare provided in public kindergartens. In the absence of the reform, the 3-year old children of mothers who extended their leave after 1995 would most likely have been exposed to this alternative type of care. The reform has thus, in effect, postponed by at least 1 year the enrollment of a substantial share of 3-year old children into pre-school education. This allows us to relate our findings to both strands of the literature. While we use the same methodological approach as the studies that exploit the family leave reforms to assess the impact of maternal care on child outcomes, our findings lie outside the range of the leave duration previously considered by this literature (a 2-year paid and 3-year unpaid leave at maximum, compared to the impact of a 4-year paid leave we study here). Given our institutional setting, the impact of the family leave extension from 3 to 4 years on child long-term outcomes should be identical to that of the postponement

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[28]The full review of this literature is, regretfully, beyond the scope of this paper.

[29]The estimates of the impact at a given age of a child should be of different sign but the same magnitude, as the two types of analysis basically study different sides of the same coin.

of pre-school educational attendance from the age of 3 to 4 in the universal childcare literature (considered e.g. by Felfe, Nollenberger, and Rodríguez-Planas 2015 or Havnes and Mogstad 2011). We thus use the findings from this second line of research as a benchmark for our estimates.

3.4 Indirect Effects

While the research surveyed offers a range of findings about the impact of family leave or childcare reforms on child outcomes, little is known about the mechanisms behind the estimated effects. Family leave and universal childcare policies are likely to affect children not only through the impact on the duration of maternal or formal care but also via other channels. The estimated effect of a specific reform is likely to be a result of a combination of the direct effect of the change in the duration of a child's exposure to maternal or formal childcare and the indirect effects that arise through the impact of the reform, for example, on mothers, families, household income etc.

There is extensive evidence on the impact of family leave reforms on mothers' labor-market outcomes. While family leaves with shorter duration (up to one year) were found to have a positive impact on mothers' labor force participation and employment, longer family leaves had no, or an adverse, impact on mothers' post-leave labor-market performance (see Olivetti and Petrongolo 2017 or Rossin-Slater 2018 for a detailed survey). By increasing the risk of post-leave inactivity and unemployment, as documented in Bičáková and Kalíšková (2019), paid family leave extensions beyond the job-protection guarantee may further prolong the duration of maternal care. Extensions of job-protected family leave were also found to increase mothers' probability of becoming entrepreneurs (Gottlieb, Townsend, and Xu 2016) and self-employed mothers were shown to devote more time to childcare, in particular educational care (Campaña, Giménez-Nadal, and Molina 2020).

Given the documented impact on mothers, a family leave extension is likely to affect children not only through the change in the duration of maternal care but also through other channels. The absence of a mother's earnings while she stays at home will reduce

the overall household income unless compensated by generous family leave benefits or an increase in father's earnings. Dahl and Lochner (2012) and Clark-Kauffman, Duncan, and Morris (2003), among others, show the positive effect of an increase in household income on a child's achievement, especially among children from disadvantaged families and those that experience the increase at an early stage of life. The decline in household income due to the mother's prolonged absence from the labor market may therefore also have a negative impact on children. The experience of a mother's job loss caused by long family leave may also affect a child's future labor-market outcomes, as the previous evidence on the impact of parental job loss on children's future unemployment suggests (Müller, Riphahn, and Schwientek 2017; Grübl, Lackner, and Winter-Ebmer 2020; Lindemann and Gangl 2020).

The leave-related job interruption and poor post-leave labor-market outcomes will also affect a mother's subsequent career decisions and aspirations (Rossin-Slater 2018), which may also have an impact on intra-household bargaining, gender role division in the family, or marital stability.[30] Besides the obvious impact on children's development and well-being, these changes are also likely to influence the formation of children's social norms, gender role attitudes,[31] and thus their own career aspirations, which will impact their long-term educational and labor-market outcomes (Bertrand 2011; Steinhauer 2018; Kleven et al. 2019).

Similarly to the indirect effects of family leave policies, the universal childcare reforms not only affect the duration of exposure to formal childcare rather than other forms of care, but are also likely to have a variety of indirect effects on child outcomes. In particular, the use of universal childcare may increase mothers' labor force participation and household income (Andresen and Havnes 2019; Brewer et al. 2018; Lovász and Szabó-Morvai 2019) and affect intra-household bargaining, role division, marital stability,

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[30]Cygan-Rehm, Kühnle, and Riphahn (2018) show that changes in family leave that improve mothers' financial situations reduce the risk of single motherhood. Dahl et al. (2016), on the other hand, find no impact of an extension of paid maternity leave to 35 weeks on fertility, marriage or divorce. Liu and Skans (2010) also find no effects of an extension of paid family leave from 12 to 15 months on parental fertility or divorce rates.

[31]There is evidence that gender-role attitudes of children are formed during childhood based on the gender roles of their parents (Cunningham 2016; Kleven, Landais, and Søgaard 2019).

and/or children's gender attitudes and social norms (Baker, Gruber, and Milligan 2008; Hardoy and Schøne 2008) - and therefore also children's educational attainment and future labor-market performance - in a similar but opposite way than the use of family leave discussed above.

4   Hypotheses and Expected Outcomes

This section provides the theoretical background for our research question and formulates hypotheses about the expected outcomes. We first consider the impact of the 1995 reform on the duration of maternal care. We document the scarce evidence and refer to our previous work on the impact of the 1995 reform on mothers' post-birth labor-market outcomes (Bičáková and Kalíšková 2019). We then consider the consequences the 1995 reform was likely to have for other outcomes of mothers and families. Based on this and the findings from previous literature surveyed in Section 3, we then formulate hypotheses about the expected impact of the 1995 reform on children's investments in human capital and their labor-market performance in early adulthood, which we estimate in this study.

4.1   The Impact on the Duration of Maternal Care

The impact of the 1995 reform on leave takeup and mothers' post-leave labor-market outcomes was studied in Bičáková and Kalíšková (2019).[32] We presented a theoretical model of a mother's choice of the duration of time spent at home after childbirth as well as its impact on her post-birth labor-market outcomes (Section 2.2 in Bičáková and Kalíšková 2019). Based on this model, we formulated the expectations about the impact of the 1995 reform on the probability of a mother being in one of the three labor-market states (employment, unemployment, and inactivity) at the time the child is 3, 4 and 5 years old, which is then estimated in the empirical analysis. The theoretical predictions and the estimated impacts are reproduced in Table 1.

As women with a youngest child aged 3 were financially incentivized by the reform

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[32]The impact on employment has also been considered in Mullerova (2017), whose estimates are similar to the corresponding subset of the results available in Bičáková and Kalíšková (2019).

Table 1: Overall impact of the 1995 reform

| Age of the youngest child: | Impact of the reform on the mother's: | | | |
| | inactivity | | unemployment | |
	theoretical predictions	estimates	theoretical predictions	estimates
aged 3	+	0.377***	−	-0.108***
aged 4	+/−	0.023*	+/−	0.060***
aged 5	+/−	-0.050***	+/−	0.044***

Note: This table summarizes the expected sign of the impact of the 1995 reform on the probability of being inactive and unemployed among mothers of 3, 4 and 5 year old children together with the actual estimates of these effects from Bičáková and Kalíšková (2019).

to stay at home for one more year, we expected it to increase the inactivity in this group.[33] Our prediction of an increase in leave takeup among mothers of a 3 year old was confirmed by a sharp rise in inactivity in this group (by almost 40 p.p.), which was also accompanied by an expected decrease in unemployment (by about 11 p.p.), suggesting that some of the women who decided to stay at home, would, in the absence of the reform, have become unemployed after the end of the 3-year leave.[34]

Mothers' responses to the 1995 reform were much greater than found in previous literature investigating the impact of paid leave extensions that were not covered by job protection (Lalive et al. 2014; Schönberg and Ludsteck 2014). We attributed the sizeable impact to a combination of the effect of a strong campaign supporting the conservative norm of a mother as the primary care giver until at least a child's 4th birthday, which increased mothers' compliance with the reform, and weak job protection with limited enforceability in the Czech Republic at that time.

Our predictions about the direction of the impact of the reform on mothers' post-leave (beyond a child's 4th birthday) labor-market outcomes were ambiguous, allowing for both positive and negative signs. We empirically determined that the reform marginally

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[33]Specifically, we expected an increase in inactivity of women whose present value of staying at home with a 3-year old child exceeded that of working and no impact on the rest.

[34]The impact of the reform on mothers estimated in Bičáková and Kalíšková (2019) is even larger than presented below in Section 6.1. While here we estimate the impact on the very first cohort of children affected by the reform (born after Oct 1992), and show the takeup by their mothers in our 'first-stage regression', we consider mothers of the second cohort of the affected children (born after Oct 1993) in our earlier work, whose takeup was even greater but whose children are less comparable with the last-unaffected cohort.

increased the share of mothers who stayed at home even beyond their child's 4th birthday, but reduced the inactivity among mothers of 5 year olds. The post-leave unemployment rose for both of these groups of mothers by 6 and 4 p.p.

The impact was surprisingly similar for women with different levels of human capital. The leave takeup was somewhat more pronounced among the low-educated mothers but the increase in post-leave unemployment was quite similar in the two groups. Interestingly, some high-educated mothers prolonged their leave even beyond the statutory maximum but not to more than 5 years, whereas some low-educated mothers, who would have taken a very long leave prior to the reform, now returned to the labor market before the child's 6th birthday. We attribute this to the positive income effect of the 1995 reform, which induced the high-educated mothers to stay at home longer but allowed the presumably credit-constrained low-educated mothers to cover the fixed costs of entering the labor market and return to the labor force earlier.[35]

Overall, the 1995 reform prolonged the duration of maternal care of an additional almost 40% of 3-year old children by at least one year. The full-time presence of mothers at home also rose by about 8 p.p. for the 4-year-olds, but about 6 p.p. of this increase consisted of mothers who were unemployed rather than inactive. While the full-time presence of mothers at home did not change for the 5-year-olds, this was a result of a decrease in mothers' inactivity, especially among the low-educated, which was fully offset by a rise in their unemployment.

4.2 The Impact on Mother and Family Outcomes

On top of the direct effect of exposure to maternal care rather than attendance at the public kindergarten, the 1995 reform could also have affected child outcomes indirectly, through its impact on other mother and family outcomes (as discussed in Section 3.4).

While the impact of the extension of paid family leave from 3 to 4 years on household income, marital stability, gender role attitudes, or social values and aspirations have

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[35]The potential mechanisms and interpretation of the results are discussed in detail in Bičáková and Kalíšková (2019).

not yet been studied, there is evidence that the 1995 reform considerably increased the total duration of mothers' career-breaks after childbirth and the risk of post-leave unemployment that the mothers faced when returning to the labor market (Bičáková and Kalíšková 2019). Human capital deterioration during the long absence from the labor market and the loss of previous jobs are likely to have a substantial negative impact on mothers' future labor-market careers. While the impact of the 1995 reform on mother's employment, unemployment and inactivity fades away by the time the child turns 7 (Bičáková and Kalíšková 2019), in line with the traditionally high labor force participation of women in the Czech Republic,[36] the negative impact on mothers' post-leave earnings, job quality or career progress may also be expected to have continued beyond the child's sixth birthday.[37]

The increased duration of mothers' presence at home after childbirth and the negative consequences for their labor-market performance could have affected their future career aspirations and strengthened the traditional model of the household. While more substantial consequences can be expected in families with lower socio-economic status, the effect on high-educated mothers must also have been considerable. The prolongation of their career-breaks to over 4 years after childbirth must have had a detrimental effect on the returns to their earlier human capital investments.

Finally, the absence of mothers' earnings when at home with a child must have reduced the overall household income (the monthly allowance amounted to only about one fifth of the average female wage as shown in Figure 2.1).[38]

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[36]Also evidenced by the fact that the majority of women (94% in 1994 and 90% in 2000) return to the labor market by their child's 7th birthday.

[37]While there is no research of the impact of the 1995 reform on these aspects of women's careers, Pertold-Gebicka (2020) evaluates another reform of parental leave in the Czech Republic in 2008 to show that shortening parental leave translates into more women working in high-skilled occupations six to eight years after childbirth.

[38]Unless the drop was more than compensated for by an increase in fathers' income (see for example Ginja, Karimi, and Xia 2022 for evidence of fathers making up the income loss caused by reduction of mothers' labor supply).

4.3 Expected Impact of the 1995 Reform on Child Outcomes

The prolonged maternal care induced by the 1995 reform and documented in Section 4.1 has basically crowded out the formal care provided in public kindergartens (see Section 2.4). When estimating the effect of the 1995 extension of paid family leave from 3 to 4 years on child outcomes, we simultaneously evaluate the impact of an exposure to at least one more year of full-time maternal care on children at the age of 3 and the impact of the corresponding postponement of enrollment into pre-school education in public kindergartens (see Section 2.4 and the discussion in Section 3.3).

If maternal care at the age of 3 was superior to that provided in public kindergartens, we would expect a positive impact of the mother's presence on a child's early development, with subsequent positive effects on educational attainment and labor-market outcomes. If exposure to pre-school education, social interaction and new stimuli provided in a formal childcare facility were more important for child development at the age of 3, the impact would, in contrast, be negative. If high-skilled mothers provide better care than formal childcare but low-skilled mothers do not, we would expect a positive impact of the reform on the children of high-educated mothers and a negative impact on the children of low-educated mothers.[39]

In order to form specific expectations about the impact of an extension of full-time maternal care beyond a child's 3rd birthday on child outcomes, we refer to the evidence documented in the previous literature (surveyed in Section 3). As the family leave literature (Section 3.1) estimates only the effect of a paid leave shorter than 2 years and of an unpaid leave at most 3-years long, documenting no or a slightly negative impact of paid leave extensions on long-term child outcomes (see Dahl et al. 2016; Rasmussen 2010 or Dustmann and Schönberg 2012), we rely primarily on the evidence from research of the impact of universal childcare on children between the ages of 3 and 4.

The universal childcare literature (Section 3.2) documents a sizable positive impact of formal childcare at the age of 3 and beyond on human capital investments and labor-

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[39]For a theoretical framework of child's development based on various forms of childcare see, for example, Dietrichson, Lykke Kristiansen, and Viinholt (2020).

market outcomes, especially for children from disadvantaged families (Dietrichson, Lykke Kristiansen, and Viinholt 2020). As the increase in the duration of maternal care induced by the 1995 reform primarily substituted the enrollment of three-year old children to public kindergartens, widely used by Czech mothers after the end of their family leave on a child's 3rd birthday prior to 1995 (see Section 2.4), we expect a non-negligible negative effect of the reform on child outcomes due to the absence of exposure to universal formal childcare. The effect should be particularly pronounced for children of low-educated mothers, whose care may enhance skills at a slower pace than the public kindergartens. As the previous literature documents that girls benefit more from universal pre-school programs (Felfe, Nollenberger, and Rodríguez-Planas 2015 and Havnes and Mogstad 2011), we expect them to be more harmed by the postponement of public kindergarten enrollment than boys.[40]

In addition to the direct effect of exposure to maternal care rather than formal childcare in public kindergarten, the estimated impact of the reform may also reflect the indirect effects on children of the impact of paid family leave extension on mothers and other family outcomes (discussed in Sections 3.4 and 4.1). The negative impact of the reform on mothers' post-leave labor-market outcomes (documented in Bičáková and Kalíšková (2019)) could also have adversely affected the children's own future labor-market careers. The experience of parental job loss could have negatively affected children's long-term outcomes (Müller, Riphahn, and Schwientek 2017; Grübl, Lackner, and Winter-Ebmer 2020; Lindemann and Gangl 2020). The negative impact of the lengthy post-birth job interruptions on mothers' careers and future aspirations could have strengthened the traditional model of the household and shaped the gender role attitudes and aspirations, especially of daughters.[41] While low-educated mothers experienced a greater negative effect of the reform on their labor-market outcomes, the prolonged career breaks of high-educated women substantially reduced the returns to their earlier high human capital

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[40]Note, however, that previous research is not fully unanimous in this respect. For example, García et al. (2020) found that boys benefit from targeted early childhood programs more than girls. While public kindergartens do not have targeted programs, the impact of a child's gender on the effect of the reform remains an open question.

[41]Among others, Kleven, Landais, and Søgaard (2019) argue that the gender identity of children (girls above all) is formed during childhood, based on the gender roles of their parents.

Table 2: Theoretical impact of the 1995 reform on child outcomes

	Low-educated Mother		High-educated Mother	
	Boys	Girls	Boys	Girls
Child outcome: Education				
Direct effect of maternal care	$--$	$---$	$-$ / 0	$-$ / 0
Indirect effects	$-$	$--$	$-$ / 0	$-$ / 0
Child outcome: NEET				
Direct effect of maternal care	$++$	$+++$	$+$ / 0	$++$ / 0
Indirect effects	$+$	$++$	$+$ / 0	$++$ / 0
Child outcome: Unemployment				
Direct effect of maternal care	$++$	$+++$	$+$ / 0	$++$ / 0
Indirect effects	$+$	$+$	$+$ / 0	$+$ / 0
Child outcome: Housework				
Direct effect of maternal care	0	$+++$	0	$++$ / 0
Indirect effects	0	$++$	0	$++$ / 0

investments, which could have decreased their daughters' incentives to invest in their own human capital. While the impact of the reform on household income or family stability has not yet been analyzed, they represent two other channels that may adversely affect children's human capital investments and early labor-market outcomes. Given the asymmetry in the implications of the 1995 reform for mothers and fathers and the importance of the gender role models within the family, we expect at least some of these indirect effects to potentially hurt girls more than boys.

Our expectations are summarized in Table 2. In line with the outcomes that we analyze in our estimation (as described in the next Section), we primarily explore the impact of the reform on educational attainment and on the attachment to the labor market, with a special focus on the specific forms of non-employment. We predict a reduction in human capital investments and an increase in the share of children who are neither in employment nor in education in their early twenties. We expect the effect to be stronger for children of low-educated mothers and also more pronounced for girls than for boys, especially in the case of a higher probability of being at home engaged in housework.

5 Methodology

5.1 Data

For the main analysis of the impact of the 1995 reform on educational and labor-market outcomes of the affected children in their early twenties, we use the Labor Force Survey (LFS) for the Czech Republic for 2010-2016. As the data does not contain information about the duration of maternal care the child was exposed to or the number of years the child attended preschool, we can only estimate the ITT effect of the 1995 reform. In order to explore the size of the treatment, we use the LFS data for 1994-1997 to estimate the impact of the 1995 reform on the probability that a mother is at home with a 3-year old child (see Section 6.1). When discussing the mechanisms behind the estimated impact of the reform, we also use the LFS data for 2005-2011 to explore the potential impact of the reform on mother's outcomes and family composition at an intermediate stage, when the affected children are aged 16-17. Descriptive statistics of all our estimation samples and sub-groups that we used in our differences-in-differences regressions can be found in Appendix Tables A.1 to A.4. They are referred to and discussed in the text where relevant.

We focus on child outcomes at the age of 21-22 for two reasons: First, we can already observe the long-term measures, including high-school completion rates, the share of individuals in tertiary education, and the labor-market outcomes of recent high-school graduates who have not enrolled into college, as well as of those with a lower level of education. Second, the share of children who still live with their parents at this age is sufficiently high, which is crucial for our heterogeneity analysis of the impact of the reform by mothers' education, as information about parental background in the data is only available for them. In the affected cohorts, about 75% of individuals reside in the same household as their mother (see Appendix Table A.2 for the exact share in the cohorts we use in the analysis). While this is a relatively high share, there may be endogenous selection of children into nest-leaving that may bias the estimates from the heterogeneity analysis. We provide evidence of zero impact of the reform on the

probability of residing with one's mother when aged 21-22 in Section 8.

The identification of the last cohort unaffected by the reform and the first cohort affected is based on the exact age of the child. As the LFS data provides age information only in completed years, we use a rotational panel structure of the data and derive age in quarters of a year based on changes observed between the two consecutive quarters.[42]

5.2 Estimation Strategy

We estimate the ITT impact of the 1995 reform using a difference-in-differences method, as is common in the literature (see e.g. Dustmann and Schönberg 2012; Baker and Milligan 2015; Carneiro, Loken, and Salvanes 2015; Dahl et al. 2016; Danzer and Lavy 2018). We compare outcomes of two cohorts of children: the last cohort unaffected and the first cohort affected by the reform. The 1992 cohort of children (children born before October 1992) was the last to be unaffected, as mothers of children who turned 3 prior to October 1995 had already concluded their 3-year paid parental leave prior to the reform. The 1993 cohort (children born in October 1992 or later) were the first children whose mothers were eligible for the paid parental leave until the child's 4th birthday. We compare the outcomes of the unaffected and affected cohorts at the same age (in quarters) when they are aged 21-22.[43]

Following the previous studies of the effects of family leave policies on child outcomes (Liu and Skans 2010; Rasmussen 2010; Dustmann and Schönberg 2012; Carneiro, Loken, and Salvanes 2015; Danzer and Lavy 2018), we control for any confounding factors, including the quarter of birth, age (maturity) or business cycle effects, using two consecutive cohorts from a neighboring year when no policy change took place as the counterfactual. In particular, we use cohorts of children born before/after October 1990

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[42]Each individual is observed for five consecutive quarters in the data, so a change in age should be observed for everybody in the sample. Due to attrition, misreporting, and measurement issues with the interview timing, there is a subset of individuals (less than 9% of the sample) for whom we cannot derive the quarter of birth and therefore cannot be used in our estimation.

[43]Alternatively, the outcomes of the two cohorts can be compared at the same calendar time. We estimate this alternative specification as part of the robustness analysis. See Section 8 for a detailed discussion of the two approaches, their respective identification assumptions, and the comparison of the results.

Table 3: Overview of the identification strategy

| | Treat 1 | | Treat 2 | |
	0	1	0	1
After $= 0$	Q3 1990	Q3 1992	Q2-Q3 1990	Q2-Q3 1992
After $= 1$	Q4 1990	Q4 1992	Q4 1990-Q1 1991	Q4 1992-Q1 1993
	Treat 3		Treat 4	
	0	1	0	1
After $= 0$	Q1-Q3 1990	Q1-Q3 1992	Q4 1989-Q3 1990	Q4 1991-Q3 1992
After $= 1$	Q4 1990-Q2 1991	Q4 1992-Q2 1993	Q4 1990-Q3 1991	Q4 1992-Q3 1993

Note: The table describes the definition of the treatment and control groups based on the quarter and year of birth.

as the control group[44] and assume that the quarter-of-birth and business cycle effects are the same for the treated cohorts (born around October 1992) and the control cohorts (born around October 1990). We discuss the underlying identification assumptions in Section 8.

We define four different treatment groups based on the size of the window around the cut-off date of birth – Treat 1 are children born within one quarter before/after the October threshold, while Treat 4 are those born within four quarters around the threshold (see Table 3 for the exact definitions of our treatment and control cohorts in the four specifications using Treat 1 to Treat 4 treatment groups). Summary statistics of the treatment and control cohorts can be found in Appendix Table A.2.

In our main specification, we compare the outcomes of the first-affected and the last-unaffected cohorts (born around October 1992) observed at the same age (21-22),[45] controlling for potential quarter-of-birth and business cycle effects with two control cohorts (born around October 1990) observed in the same calendar-year quarters as the corresponding treatment cohorts. The estimated equation can be written as:

$$y_i = \alpha_0 + \alpha_1 Treat_i + \alpha_2 After_i + \alpha_3 Treat_i * After_i + X_i \beta + u_i \tag{1}$$

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[44]These cohorts include children of mothers who were all eligible for a three-year paid parental leave and were not directly affected either by the 1995 reform or by any other earlier family policy change.

[45]We use the 2-year age window to ensure a sufficiently large sample size but control for age differences within this window with quarter of birth and calendar year-quarter fixed effects.

where y_i is the outcome variable. $Treat_i$ is the dummy for the treated cohorts: children born before/after October 1992 have $Treat_i = 1$, while children born before/after October 1990 have $Treat_i = 0$. $After_i$ is the dummy for being born after the October threshold - children born after October 1990/1992 have $After_i = 1$ and children born before the October thresholds have $After_i = 1$. The coefficient of the interaction term (α_3) captures the effect of the 1995 reform. We use several outcome variables:

- completed high school[46]

- tertiary (being a student in or having completed tertiary education)[47]

- NEET (not in education, employment, or training)

- employed

- unemployed

- housework (inactive with housework as a main activity).

We also include several control variables (X_i) – dummy variable for gender, fixed effects for quarter of birth, fixed effects for regions of residence, and fixed effects for all calendar quarter-year combinations.[48] In line with previous research (Section 3) and our theoretical expectations (Section 4), we also explore the heterogeneity of the impact of the 1995 reform on child outcomes by mothers' education and by gender of the child.

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[46]We do not consider completion of lower secondary education as an outcome because over 99% of our treatment and control groups have completed lower secondary school (see Appendix Table A.2).

[47]Most individuals in our treatment groups have not (yet) finished tertiary education (only about 6-7% do, see Appendix Table A.2), so we use a variable which captures either completion of tertiary education or current tertiary studies (about 45% of treated individuals study in tertiary education) to proxy the reform's impact on enrollment into college. The total share of individuals who completed tertiary education or are currently studying is similar for our control cohorts, but they are more likely to have completed their tertiary studies (Appendix Table A.2).

[48]Note that given our setup, including both calendar year-quarter fixed effects and age year-quarter fixed effects is not possible due to perfect multicollinearity. Depending on the specification, the calendar year-quarter fixed effects capture both the unobserved differences across the year-quarters of calendar time as well as of age.

6 Results

6.1 Impact on Mothers

We do not observe family leave takeup by mothers of children whose long-term outcomes we analyze. Without the information on maternal care exposure and kindergarten attendance at the age of 3, we can only estimate the ITT effect of the 1995 reform on children. We are, however, able to document the family leave takeup of a representative sample of mothers of the last-unaffected and first-affected cohort of children from earlier surveys of the LFS data in 1994-1997, when these children were 3. Analysis of the impact of the 1995 reform on mothers' leave-taking can be considered to be the first-stage of our estimation of the reform's ITT effect on long-term child outcomes.

In order to provide information about the size and potential heterogeneity of treatment for our ITT results, we estimate the impact of the 1995 reform on the inactivity of mothers when their youngest child is 3. In particular, we classify mothers of the children belonging to the treatment and control groups before and after the reform, as described in Table 3, according to when their child was born. We then compare mothers of the last cohort of 3-year olds unaffected by the reform (those born prior to October 1, 1992) with mothers of the first cohort of 3-year olds affected by the reform (those born prior to October 1, 1992), observed at the same age of the child, and control for confounding factors due to calendar-year and quarter-of-birth effects with analogous groups of mothers of older children (born around October 1 of 1990). We again vary the window on both sides of the cut-off from one (Treat 1) to four quarters (Treat 4) of the year.

Using the same empirical specification and DID strategy as described in Section 5.2 for the estimation of the impact on child outcomes, we estimate equation 1 on mothers, with the probability of being inactive as the explained variable and the same set of right-hand-side variables, only replacing the child characteristics with those of mothers.[49] Table 9 presents the estimates of the impact of the 1995 reform on inactivity of mothers when their youngest child is 3 for the four different windows (Treat 1 to Treat 4) and

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[49]In particular, the regressions control for mothers' age, education, marital status, number of children, and presence of elderly in the household.

also by mothers' education.[50]

Table 4: Estimation results: impact on mothers

| dep. var. | Mother inactive when youngest child is 3 | | | |
	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline				
Treat*After	0.094**	0.176***	0.275***	0.293***
	(0.042)	(0.029)	(0.025)	(0.022)
R-squared	0.317	0.287	0.267	0.261
Observations	1668	3246	4439	5434
Panel B: By mothers' education				
Treat*After	0.027	0.180***	0.282***	0.289***
	(0.065)	(0.045)	(0.038)	(0.034)
Treat1*After*HighEducation	0.116	-0.009	-0.012	0.007
	(0.084)	(0.059)	(0.05)	(0.054)
R-squared	0.322	0.29	0.268	0.263
Observations	1668	3246	4439	5434

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mothers' education (Panel B). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for mothers' age, education, marital status, number of children, presence of elderly in the household, regional and quarter-year fixed effects. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (1994-1997), own calculations.

The results reveal that the compliance of mothers with the 1995 reform was substantial and did not differ across their education. Depending on the specification, an additional 10-30 percent of mothers of the 3-year olds have extended their leave beyond their child's 3rd birthday in response to the 1995 reform.[51] The fact that the size of the effect increases with the width of the window is likely to be driven by gradual adjustment of new cohorts of mothers whose children turned 3 after the 1995 reform was announced.[52]

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[50]Note that the results by mothers' education include the full sample in this case, as information is available for everybody. When analyzing the long-term child outcomes, mother's education is known only for children who still reside with their mothers.

[51]Note that the impact of the reform in Bičáková and Kalíšková (2019) is even larger, suggesting a takeup of almost 40 p.p. among the mothers of 3-year olds. While here we estimate the impact on the very first cohort of children affected by the reform (born after Oct 1992), and show the takeup by their mothers, we focus on mothers of the second cohort of the affected children (born after Oct 1993) in our earlier work, whose takeup was even greater, given the gradual adjustments of mothers to the reform, but whose children are less comparable with the last-unaffected cohort.

[52]As the reform was not announced in advance, some of the mothers who were among those first eligible could have already made arrangements for returning to the labor force before the reform came into effect.

The results from the first-stage regression documents the intensity of the treatment as well as the fact that the takeup did not vary by mothers education. Do the mothers of the youngest 3-year old children, who extended their leave beyond their child's 3rd birthday in response to the 1995 reform, differ in any other way from the non-complying mothers? In order to address this question, we compare the average characteristics of inactive mothers with a youngest 3-year old child prior to and after the reform (see Table A.1 in the Appendix). There are twice as many mothers of 3-year olds who are inactive in our data after the reform than prior to it. The differences in summary statistics for the two groups are only minor. Women who were at home with a 3-year old child after the reform are, on average, slightly less likely to be married and more likely to cohabit than inactive mothers of 3-year olds prior to the reform. They are also somewhat less educated, more likely to have ever worked (which is probably partly driven by completing school earlier with less years of schooling, vis a vis those who have children during studies), and have a less educated spouse. A higher share of mothers with attributes that proxy lower income and wealth among those who were inactive after the reform, confirms that the additional financial support granted until a child's 4th birthday allowed less well-off women to stay at home with a 3-year old child - something that they could not afford prior to the reform. The summary statistics also imply that women who prolonged their leave in response to the 1995 reform were somewhat younger, had fewer children, were less likely to have elderly members in the household, and their 3-year old youngest child was somewhat less likely to be a girl.[53] While the differences in magnitudes we have described are in line with our expectations, none of them turns out to be statistically significant.[54]

Our first-stage results on the reforms' impact on mothers' takeup suggest firstly that the treatment that we estimate in our ITT analysis was substantial, and secondly that it was most pronounced in the wider specifications (Treat 3 and Treat 4), given the grad-

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[53]Mothers who cared for the elderly were more likely to be inactive irrespective of the reform.

[54]In particular, we interacted these characteristics, one by one, with the impact of the reform in the first stage regression, in a similar way as we do with the indicator that the mother has high education, as shown above, and none of them seemed to have an impact on the takeup. Results available upon request.

ual adjustment of mothers with the youngest 3-year old child following the 1995 reform. Thirdly, those treated did not systematically differ in terms of the observed characteristics of their parents and household composition from those who were not. With no evidence of selective takeup, we consider our main results to be fairly representative and not subject to a large endogenous compliance bias.

6.2 Education Outcomes of Children

The main estimation results for education outcomes are presented in Table 5. The outcomes are described by two binary variables - one for completed high school and the second for enrollment into tertiary studies and / or their completion. Our baseline specification reveals a weak negative impact of the family leave reform on high-school completion (for Treat 2 and 3 specifications, see Panel A of Table 5) and a stronger negative impact on the tertiary studies in our wider specifications (Treat 3 and 4, see Panel A of Table 5), suggesting a decrease in the probability of college enrollment by about 5 p.p. This is consistent with a higher intensity of treatment in the two wider specifications (due to gradual adjustment of the new cohorts of eligible mothers of 3-year olds resulting in a steady rise in takeup of the extended leave during the first year after the 1995 reform) as documented in Section 6.1. The estimate is also negative, but not statistically significant, for the Treat 2 specification and for the other two specifications for high-school completion.

Estimating the impact separately for children of low- and high-educated mothers (Panel B of Table 5) reveals that the negative effect on college enrollment is driven by children with low-educated mothers, implying that the inputs to child development provided by formal childcare at the age of 3 are superior to those provided by low-educated mothers. Our results from the two wider specifications (Treat 3 and 4) suggest that the reform (inducing extra time with mother as opposed to kindergarten attendance) decreased the probability of enrolling into college by about 12 percentage points for children with low-educated mothers. The sign of the effect in the two specifications with more-narrowly defined windows around the cut-off point (1 and 2 quarters) are also

Table 5: Estimation results: education

dep. var.	Completed high school				Tertiary			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	-0.0341	-0.0389*	-0.0301*	-0.0132	0.00599	-0.0274	-0.0529*	-0.0561**
	(0.0301)	(0.0203)	(0.0165)	(0.0140)	(0.0527)	(0.0369)	(0.0298)	(0.0258)
Observations	1,448	2,853	4,254	5,616	1,448	2,853	4,254	5,616
R-squared	0.091	0.058	0.057	0.050	0.126	0.090	0.087	0.086
Panel B: By mothers' education								
low education	-0.0619	-0.0421	-0.0419	-0.0138	-0.115	-0.0728	-0.117**	-0.126***
	(0.0574)	(0.0420)	(0.0332)	(0.0282)	(0.0930)	(0.0674)	(0.0539)	(0.0471)
high education	0.0308	-0.00922	-0.00491	-0.00285	0.0236	-0.0247	-0.0226	-0.0196
	(0.0332)	(0.0226)	(0.0186)	(0.0151)	(0.0798)	(0.0557)	(0.0453)	(0.0387)
Observations	1,001	1,932	2,855	3,796	1,001	1,932	2,855	3,796
R-squared	0.106	0.063	0.057	0.052	0.269	0.215	0.213	0.200
Panel C: Girls by mothers' education								
low education	-0.00220	-0.00276	-0.0647	-0.0350	-0.301*	-0.158	-0.194**	-0.168**
	(0.0865)	(0.0528)	(0.0457)	(0.0395)	(0.162)	(0.108)	(0.0877)	(0.0767)
high education	-0.0141	0.00586	0.00947	0.0147	-0.129	-0.0298	-0.0474	-0.0405
	(0.0385)	(0.0282)	(0.0222)	(0.0172)	(0.115)	(0.0779)	(0.0620)	(0.0520)
Observations	442	871	1,289	1,747	442	871	1,289	1,747
R-squared	0.288	0.137	0.102	0.085	0.376	0.270	0.245	0.199
Panel C: Boys by mothers' education								
low education	-0.0311	-0.0375	-0.0345	-0.00422	0.0411	0.0162	-0.0445	-0.0815
	(0.0822)	(0.0610)	(0.0483)	(0.0409)	(0.119)	(0.0860)	(0.0682)	(0.0594)
high education	0.0750	-0.0101	-0.0121	-0.0116	0.171	-0.0508	-0.0124	-0.00883
	(0.0505)	(0.0352)	(0.0293)	(0.0242)	(0.121)	(0.0819)	(0.0669)	(0.0573)
Observations	559	1,061	1,566	2,049	559	1,061	1,566	2,049
R-squared	0.153	0.099	0.076	0.065	0.242	0.163	0.152	0.148

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mothers' education (Panels B and C). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

negative but the estimates are not statistically significant using these smaller samples.

Conducting our heterogeneity analysis (by mothers' education) also by gender of the child (Panel C of Table 5), we find that girls are much more affected by the reform than boys. In fact, the entire negative effect of the reform on college enrollment seems to be predominantly driven by girls with low-educated mothers. The estimates suggest that the reform decreased their probability of being in tertiary education or its completion at the age of 21-22 by as much as 17-19 percentage points. The effect is again statistically

significant for the wider specifications (Treat 3 and 4) but it is now also marginally significant for the narrowest window (Treat 1).

6.3 Labor Market Outcomes of Children

We next focus on the children's economic status in their early twenties. In particular, we look at the impact of the extra year of maternal care on the following labor-market outcomes of the affected children at the age of 21-22: the probability of being NEET (not in education, employment or training), being employed, being unemployed, and being inactive engaged in housework.[55]

While we find no significant impact of the 1995 reform on the probability of being employed, we do find a positive impact on being NEET (Panel A of Table 6). This is in line with the lower probability of attending college at 21-22 (documented previously in Section 6.2) but also reflects the impact on labor-market outcomes discussed below. The positive effect on being NEET is statistically significant for all specifications but the 3-quarters window (Treat 3), and suggests that the 1995 reform increased the probability of being NEET by 4-7 percentage points.

Looking at the heterogeneity by mother's education (Panel B of Table 6), we find that the reform increased the probability of being NEET among children with low-educated mothers by 9-21 percentage points, depending on the specification. While there is a fairly wide range in the estimated magnitudes, the effects are highly significant for all treatment and control groups. On the other hand, estimates for individuals with a high-educated mother are not significantly different from zero in any specification, suggesting that the effect is again driven by children who were exposed to maternal care by a low-educated mother instead of attending public kindergarten.

When we further split our sample by the gender of the child, we find that the positive impact of the 1995 reform on the probability of being NEET is present for both boys

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[55]All results presented in this section use the full set of control variables described in Section 5.2, but do not control for educational attainment, as this is likely to also be affected by the reform (see Section 6.2). Controlling for the highest level of education does not change the results (estimates available upon request).

Table 6: Estimation results: NEET and employment

dep. var.	NEET				Being employed			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	0.0678*	0.0560**	0.0279	0.0367**	-0.0446	-0.0344	0.0221	0.0152
	(0.0387)	(0.0257)	(0.0206)	(0.0175)	(0.0529)	(0.0369)	(0.0299)	(0.0259)
Observations	1,448	2,853	4,254	5,616	1,448	2,853	4,254	5,616
R-squared	0.078	0.060	0.058	0.051	0.125	0.093	0.095	0.089
Panel B: By mothers' education								
low education	0.209***	0.133**	0.0944**	0.116***	-0.196*	-0.0701	0.0363	0.00510
	(0.0745)	(0.0522)	(0.0418)	(0.0351)	(0.102)	(0.0732)	(0.0579)	(0.0500)
high education	0.0310	0.0240	0.00405	0.00272	0.0144	-0.0261	0.00180	0.0224
	(0.0381)	(0.0300)	(0.0241)	(0.0199)	(0.0823)	(0.0566)	(0.0464)	(0.0396)
Observations	1,001	1,932	2,855	3,796	1,001	1,932	2,855	3,796
R-squared	0.139	0.071	0.063	0.055	0.180	0.139	0.142	0.135
Panel C: Girls by mothers' education								
low education	0.211*	0.150*	0.110*	0.136**	0.0350	0.0557	0.126	0.0386
	(0.116)	(0.0791)	(0.0665)	(0.0567)	(0.165)	(0.113)	(0.0894)	(0.0776)
high education	0.0423	0.0197	-0.0121	-0.00600	0.126	-0.0610	-0.00386	0.0391
	(0.0596)	(0.0481)	(0.0376)	(0.0303)	(0.128)	(0.0847)	(0.0678)	(0.0569)
Observations	442	871	1,289	1,747	442	871	1,289	1,747
R-squared	0.312	0.144	0.114	0.092	0.292	0.182	0.159	0.136
Panel C: Boys by mothers' education								
low education	0.184*	0.114*	0.0756	0.101**	-0.355**	-0.183*	-0.0532	-0.0422
	(0.0993)	(0.0688)	(0.0545)	(0.0454)	(0.137)	(0.0996)	(0.0792)	(0.0679)
high education	0.0263	0.0338	0.0179	0.00882	-0.101	0.00780	0.00950	0.0201
	(0.0541)	(0.0425)	(0.0337)	(0.0280)	(0.122)	(0.0820)	(0.0661)	(0.0562)
Observations	559	1,061	1,566	2,049	559	1,061	1,566	2,049
R-squared	0.173	0.109	0.081	0.070	0.195	0.126	0.119	0.123

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mother's education (Panels B and C). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* p<0.10, ** p<0.05, *** p<0.01). Source: Czech Labor Force Survey data (2010-2016), own calculations.

and girls with low-educated mothers, with a larger effect on the latter (Panel C of Table 6). The results are only marginally significant in the smaller samples of the narrower specifications, but they remain highly significant for the widest window (Treat 4). There is also some evidence that the increase in NEET among boys with low-educated mothers is driven by a decrease in their probability of being employed. In particular, our results from the two narrower specifications (Treat 1 and 2) suggest that the 1995 reform decreased the probability of being employed among boys with low-educated mothers by

as much as 35 percentage points despite having no impact on their probability of studying in tertiary education, as noted earlier.

The reform had no impact on girls' employment (Panel C of Table 6), suggesting that the rise in the probability of daughters of low-educated mothers being NEET was entirely driven by the decrease in their college enrollment evidenced above. So what were the affected girls engaged in instead of tertiary education? Estimates presented in Table 7 reveal that at least some of them were more likely to be at home (inactive) engaged in housework. While the effect is only significant for the narrowest specification (Treat 1), the size of the coefficient implies that the reform increased the likelihood of being inactive engaged in housework by as much as 15 percentage points among girls with low-educated mothers. While the reform increased the likelihood of being NEET among both sons and daughters of low-educated mothers, we find no impact on their unemployment (see Table 7).

7 Interpretation and Mechanisms

7.1 Comparison with Previous Findings

Our results are in line with the negative impact of family leave extension to 3 years on children's schooling outcomes documented in Dustmann and Schönberg (2012) and Canaan (2022). The previous studies, however, find effects that are much smaller in magnitude and are based on rather different settings than ours. The size of the effect of maternal care at the age of 3 on college enrollment that we find is about 20 times larger than the effect of maternal care at the age of 2 on the probability of attending an academic track at high school, the most comparable outcome to ours,[56] estimated in Dustmann and Schönberg (2012). The sizeable negative effect on long-term educational and labor-market outcomes that we document also contrasts with Danzer et al. (2022), who focus on similar outcomes to ourselves and find zero impact of a paid family

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[56] As the academic track channels high-school students towards college education, it can be regarded as a proxy for college enrollment.

Table 7: Estimation results: unemployment and housework

dep. var.	Being unemployed				Inactive - housework			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	0.00957	0.0179	0.00458	0.00659	0.0252	0.00848	0.00208	0.00654
	(0.0278)	(0.0184)	(0.0152)	(0.0129)	(0.0253)	(0.0167)	(0.0133)	(0.0113)
Observations	1,448	2,853	4,254	5,616	1,448	2,853	4,254	5,616
R-squared	0.058	0.035	0.031	0.026	0.119	0.097	0.091	0.084
Panel B: By mothers' education								
low education	0.0560	0.0467	0.0142	0.0301	0.0744**	0.0355	0.0279	0.0252*
	(0.0638)	(0.0438)	(0.0351)	(0.0298)	(0.0331)	(0.0230)	(0.0185)	(0.0147)
high education	-0.00741	-0.00306	-0.00788	-0.00980	0.00807	-0.00619	-0.00746	-0.000770
	(0.0333)	(0.0247)	(0.0208)	(0.0175)	(0.0144)	(0.00921)	(0.00651)	(0.00533)
Observations	1,001	1,932	2,855	3,796	1,001	1,932	2,855	3,796
R-squared	0.102	0.060	0.051	0.042	0.103	0.063	0.061	0.047
Panel C: Girls by mothers' education								
low education	-0.0106	0.0440	0.00741	0.0173	0.156**	0.0623	0.0513	0.0496
	(0.104)	(0.0661)	(0.0556)	(0.0464)	(0.0741)	(0.0494)	(0.0398)	(0.0322)
high education	0.0320	0.0136	0.000401	0.00230	0.00838	-0.0233	-0.0229	-0.00342
	(0.0444)	(0.0342)	(0.0301)	(0.0256)	(0.0385)	(0.0250)	(0.0164)	(0.0123)
Observations	442	871	1,289	1,747	442	871	1,289	1,747
R-squared	0.276	0.127	0.086	0.063	0.199	0.104	0.099	0.073
Panel C: Boys by mothers' education								
low education	0.0671	0.0469	0.0135	0.0422	N/A^a	0.0126	0.00725	0.00508
	(0.0850)	(0.0612)	(0.0471)	(0.0399)		(0.0118)	(0.00704)	(0.00499)
high education	-0.0316	-0.0112	-0.0119	-0.0198	N/A^a	-0.000380	-0.000966	-0.000504
	(0.0555)	(0.0372)	(0.0300)	(0.0255)		(0.00187)	(0.00132)	(0.000768)
Observations	559	1,061	1,566	2,049	559	1,061	1,566	2,049
R-squared	0.144	0.089	0.068	0.059		0.115	0.073	0.051

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mother's education (Panels B and C). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* p<0.10, ** p<0.05, *** p<0.01). Source: Czech Labor Force Survey data (2010-2016), own calculations.
[a] Due to small sample size and a very low share of boys engaged in housework, these regressions cannot be estimated.

leave extension to 2 years on children who are most comparable to our sample in terms of counterfactual type of care, i.e. the relatively small group in their data for whom prolonged maternal care most likely replaced formal childcare provided in nurseries.

The contrast between our results and the previous findings should, however, not be regarded as contradictory but rather as documenting different pieces of evidence that complement each other. The effect of all-day maternal care compared to formal childcare

cannot be the same at the age of 1 or 2 as its impact at the age of 3. This is confirmed by studies that evaluate the universal childcare reforms where maternal (or informal) care is being replaced by formal childcare. This strand of research shows that the importance of formal childcare attendance increases with a child's age, in particular for children of low-educated parents. The setup of our analysis is directly comparable to several studies from this literature and our results are strikingly similar to those in Havnes and Mogstad (2011), who analyze a large-scale expansion of subsidized child care for 3-6-year-old children on their educational and labor-market outcomes in adulthood, making this study the most comparable to ours. They find that enrollment into subsidized childcare increases the probability of attending college by almost 7 p.p. and reduces the probability of being on welfare by almost 5 p.p.; magnitudes and findings very similar to ours. While the expansion of subsidized childcare in Havnes and Mogstad (2011) mostly replaced other informal care, it was maternal care that crowded out public kindergarten attendance in our study. The fact that our results are very similar to theirs, just with an opposite sign, extends this line of research by suggesting that there is a positive impact of pre-school education on children at the age of 3 not only in comparison with informal care but also with the care provided by a child's mother.

The results of our heterogeneity analysis are also in line with findings from this strand of literature showing that the positive long-term effects of formal childcare are mostly driven by children with a low socio-economic background. We also find a larger impact on daughters, but there are slight differences in terms of the results by gender for specific outcomes. Havnes and Mogstad (2011) find that the increase in educational attainment was most pronounced among children with a low socio-economic background, whereas improvements in labor-market outcomes were primarily experienced by girls. Using the same reform, Havnes and Mogstad (2015) further explore the non-linearity of the impact of preschool childcare and show that the positive effect is driven by individuals from the lower and middle parts of the earnings distribution, which often contain less-educated parents. Finally, Felfe, Nollenberger, and Rodríguez-Planas (2015) show that the positive effects of universal childcare are driven by children from disadvantaged families and girls.

While our estimates confirm a larger risk of being NEET at the age of 21-22 for daughters of low-educated mothers (driven by a decrease in college enrollment and slight increase in home production), it is the sons of low-educated mothers who suffer a decrease in the probability of employment, which explains the rise in the their probability of being NEET.

7.2   Mechanisms

Given how close our estimates are to the universal childcare literature (Havnes and Mogstad 2011), we attribute an important part of the negative impact on human capital accumulation and labor-market outcomes of the affected cohorts to the direct effect of prolonged maternal care crowding out pre-school education at the age of 3. There may, however, be other mechanisms at play (Canaan 2022). The fact that girls with lower socio-economic background invest less in their human capital and focus more on home production when exposed to maternal care at the age of 3, rahter than attending public kindergarten, could be a result of a change in preferences and attitudes induced by the reform. In particular, an exposure to prolonged maternal care could lead to a rise in pro-family attitudes, strengthen traditional gender role models, and increase children's preference for early family formation. The heterogeneity in the effect that we find across gender is consistent with Kleven, Landais, and Søgaard (2019), who show that the traditional gender role division is transmitted from parents to daughters but not sons.[57] The impact of the gender norms on labor-market outcomes can be sizeable, as documented in Steinhauer (2018),[58] with magnitudes comparable with those we find here.

To assess the importance of the impact of gender norms, we explore whether the adverse effects of the reform on educational attainment and increased probability of home production among girls were also accompanied by changes in marital status, cohabitation

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[57]Kleven, Landais, and Søgaard (2019) find that daughters of mothers whose wages are affected by a larger child penalty are more likely to also experience a sizeable child penalty.

[58]He shows that women born into an environment with strong beliefs that children of working mothers suffer have a lower probability of working when their children are small, by as much as 5-10 p.p. (about 15-25% less than women born into a social environment with less traditional beliefs).

or fertility decisions. Estimates of the effect of the 1995 reform on these additional outcomes, presented in Table 8, suggest that this was not the case. We find no impact of the reform on the marital status of affected children, on the probability that they have their own children, or the likelihood of cohabitation at the age of 21-22.

We also consider the potential impact of the 1995 reform on mother and family outcomes, which may in turn also affect children's development. The takeup of the extended family leave resulted in loss of the mother's earnings, and the parental allowance mitigated only about one fifth of this loss. The negative impact of the 1995 reform on mothers' immediate labor-market outcomes was documented in Bičáková and Kalíšková (2019). There could also be further negative implications of the mother's prolonged absence from the labor market on household resources and family background due to her slower career progression and lower future earnings or job loss. We do not have information on household income in the data but we can explore a subset of selected mothers' outcomes in order to shed more light on the potential impact of the reform on long-term household earnings and family stability. In particular, we estimate the effect of the 1995 reform on mother's economic outcomes (probability of being employed / unemployed) and on their probability of being single and of being divorced, when the affected children are aged 16-17.[59] The results summarized in Table 9 show no evidence that the reform had an impact on mother's long-term labor-market outcomes.[60] While we cannot rule out the presence of the effects of the reform on other mother and family outcomes we do not observe in our data (including household income, mother's earnings or post-leave job quality) or the impact of the reform on children's values and aspirations, the limited evidence we have confirms our earlier conjecture that the direct effect of the reform. i.e. exposure to maternal care instead of attending a subsidized kindergarten, was likely to be the most important mechanism behind our findings.

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[59]We look at mothers' outcomes when children are 16-17 (not 21-22 as in our main specification) to check any medium-term impact of the reform on mothers, as well as to be able to observe mothers' outcomes for the majority of the children, as only those who reside with their mothers have non-missing information about mothers' outcomes. While only 75% of children live with their mother when they are 20-21, 97% do so when they are 16-17.

[60]This is in line with Bičáková and Kalíšková (2019), who document that the negative effects of the 1995 reform on mothers' immediate labor-market outcomes faded away by the time the youngest child turned 7.

Table 8: Estimation results: marital status and family outcomes

dep. var.	Living with mother				Married			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Treat*After	0.0544	-0.0177	-0.0294	-0.0159	-0.00643	0.00773	0.0134	0.00883
	(0.0468)	(0.0329)	(0.0269)	(0.0233)	(0.0203)	(0.0143)	(0.0117)	(0.0102)
Observations	1,448	2,853	4,254	5,616	1,448	2,853	4,254	5,616
R-squared	0.177	0.155	0.143	0.129	0.098	0.083	0.067	0.057

dep. var.	Having own children				Cohabiting			
Treat*After	-0.0245	-0.00163	-0.00320	0.00995	-0.0306	0.0114	0.0149	0.0111
	(0.0296)	(0.0196)	(0.0160)	(0.0136)	(0.0434)	(0.0304)	(0.0249)	(0.0214)
Observations	1,448	2,853	4,254	5,616	1,448	2,853	4,254	5,616
R-squared	0.131	0.100	0.084	0.082	0.154	0.126	0.118	0.108

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1. Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 1 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

8 Identification Assumptions and Robustness Checks

8.1 Alternative Specification

The last-unaffected and the first-affected cohorts (born before and after October 1992) can be compared at exactly the same age or at exactly the same calendar time (both measured in quarters of a year). In our main specification, we compare the last-unaffected and the first-affected cohorts (born before and after October 1992) at exactly the same age (measured in quarters of a year) when they are aged 21-22. Fixing the age ensures that our results are not driven by any age effects but also implies that the outcomes of the two cohorts are observed at different calendar times,[61] and are therefore affected by different business cycle conditions and seasonality effects. In addition, as the last-unaffected and the first-affected cohorts are born in different quarters of the year, their outcomes may also differ due to the quarter-of-birth effects.[62] In order to filter out the calendar-time and quarter of birth effects when comparing the cohort outcomes, we use the difference-in-differences approach with control cohorts of older children (born before

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[61]Fixing the age window of 21-22 for the last-unaffected and first-affected cohorts born 1 quarter before and after October 1992 means comparing their outcomes observed in Q3 2013 – Q2 2015 and Q4 2013 – Q3 2015 periods, respectively.

[62]Quarter-of-birth effects may be present in all specifications but Treat 4 when we compare individuals born one year before/after the cutoff.

Table 9: Estimation results: mother's outcomes when children are aged 16-17

dep. var.	Mother employed				Mother unemployed			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Treat*After	0.009	-0.009	-0.011	-0.019	0.011	0.005	0.004	0.003
	(0.047)	(0.029)	(0.022)	-0.019	(0.027)	(0.016)	(0.012)	(0.011)
Observations	1400	3458	5549	6916	1400	3458	5549	6916
R-squared	0.073	0.043	0.035	0.030	0.082	0.037	0.033	0.026
dep. var.	Mother single				Mother divorced			
Treat*After	-0.079*	-0.003	-0.005	-0.009	-0.060	0.021	0.021	0.016
	(0.046)	(0.028)	(0.022)	(0.019)	(0.043)	(0.026)	(0.020)	(0.018)
Observations	1326	3289	5297	6594	1400	3458	5549	6916
R-squared	0.104	0.053	0.038	0.034	0.082	0.035	0.026	0.024

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1. Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 1 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2005-2011), own calculations.

and after October 1990), observed over exactly the same calendar period and with the same quarter-of-birth compositions. Our identification strategy is thus based on the assumption that the potential confounding factors are the same for the treatment and control cohorts observed in the same year but at different age. In particular, while the outcomes are allowed to vary with economic conditions and/or quarter-of-birth effects, their impact on these outcomes must be age-invariant. To give an example, if the probability of employment of young individuals is affected by changes in economic conditions differently at age 21 than at age 23, the differences in outcomes of the last-unaffected and the first-affected cohorts due to business-cycle effects may not be correctly captured by our control group cohorts. Similarly, if, for example, the educational outcomes are differently affected by the quarter-of-birth when the individual is 21 and when 23, our control cohorts would fail to fully filter out the quarter of birth effects.

To assess the sensitivity of our results to these assumptions, we conduct a robustness check that compares the outcomes of the last-unaffected and first-affected cohorts at exactly the same calendar-time period (but at different age), controlling for potential age and quarter-of-birth effects using the control cohorts (born before and after October 1990) observed at the same age. While this alternative specification allows for the age-specific impact of changes in economic conditions and of quarter-of-birth effects, it

makes a different restrictive identification assumption that the age and the quarter-of-birth effects do not vary with calendar time. In particular, the outcomes are allowed to be affected by age and/or quarter-of-birth effects, but the impact of age and quarter of birth on these outcomes has to be time-invariant. To provide an example of a violation of these assumptions, if the probability of employment changes with age differently at times of recession than at times of economic boom, and we observe our treated and control cohorts at different stages of the business cycle, the control group will not fully filter out the age effects.

To explore the robustness of our findings and show how they vary with the different set of imposed assumptions we next compare the results from the two specifications. The estimates from the main specification were presented in Tables 5, 6 and 7 in Section 6. The corresponding results when using the alternative specification are reported in Appendix Tables A.5, A.6 and A.7 Although the robustness analysis shows no effect of the 1995 reform on the tertiary-education outcome (Appendix Table A.5), it confirms the large and significant impact on the NEET outcome. In particular, the first-affected cohorts of children with a low-educated mother are 9-16 percentage points more likely to be NEET than the last-unaffected cohorts according to our robustness results (the effect is statistically significant for the two narrowest specifications, see Panel B in Appendix Table A.6) and the effect seems to be driven solely by girls. For girls with low-educated mothers, the positive impact on NEET outcomes is highly significant in all specifications and the increase varies from 11 to 21 percentage points (Panel C in Table A.6).

Similarly to our main specification, we also see some, though weaker, evidence that the reform decreased the probability of being employed among boys with low-educated mothers (Panels B and C in Table A.6) and stronger evidence that the reform increased the likelihood of home production among girls with low-educated mothers (Panels B and C in Table A.7). In particular, the reform's impact on being inactive engaged in housework among girls with low-educated mothers is highly significant in all specifications except the narrowest one (Treat 1), and suggests an increase of 7-9 percentage points.

Interestingly, the robustness results provide some evidence that the reform decreased

the probability of being unemployed among children with high educated mothers (Panel B in Appendix Table A.7). This is the first finding that implies some positive effect of the reform on labor-market outcomes of affected children. While this finding is not supported by the results from our main specification, it is in line with the conclusion that the impact of the reform depended to a large extent on the mother's educational attainment (a proxy for the quality of maternal care that substituted kindergarten attendance for the affected cohorts). Panel C in Table A.7 reveals that this negative unemployment effect was mostly driven by boys. As there was no impact on the probability of being NEET or in employment, the decrease in unemployment suggests that the sons of high-educated mothers continued in their studies, but not at college, as there is no evidence of an increase in enrollment to tertiary education.[63]

Our findings on the impact of the 1995 reform on the probability of residing with one's mother, on partnership status and on family formation (considered in Section 7) are all robust to the alternative specification, confirming that the family leave extension had no impact on any of these outcomes (Appendix Table A.8).

8.2 Common Trend Assumptions

We provide further evidence on the validity of these two estimation strategies by checking their underlying common-trend assumptions. Our approach assumes that the evolution of outcome variables for the treatment and control cohorts would have been the same in the absence of the reform. We check this assumption by comparing series of cohorts who were born in the pre-treatment period and were thus not affected by the reform. Appendix Figures A.1 and A.2 show pre-reform cohort trends for the main specification, where the corresponding treatment and control cohorts are observed in the same calendar-time period (2014). The figures report averages of our main outcome variables for the treatment cohorts born before October 1, 1992, and for the control cohorts born before October 1, 1990 (cohorts born 1 quarter before the corresponding October thresh-

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[63]For the widest specification (Treat 4), we also find a negative unemployment impact of the reform on boys with low-educated mothers, which is, however, not confirmed by the narrower specifications.

old are denoted -1, those born 2 quarters before the corresponding threshold are denoted -2, etc.). Both the education and labor-market outcomes of the treatment and control cohorts evolve more or less in parallel over the pre-treatment period, with the exception of cohorts -2, for which the share of individuals in tertiary education is higher and the share of NEET individuals lower in the control cohort (relative to the treated cohort) compared to what the common trend would have implied.

Appendix Figures A.3 and A.4 illustrate the pre-treatment trends for the alternative specification by comparing the pre-reform evolution of the outcome variables for the treated and control cohorts observed at the same age (21). Most of our outcome variables are slightly more volatile in the treated than the control cohorts and the trends are thus less similar than in the main specification. However, there is one outcome variable for which the common trend seems to work better in the robustness specification – the share of individuals being inactive engaged in housework. Recall that while the robustness results are not significant for the educational outcomes, they show much stronger evidence of a reform's effect on the probability of doing housework than we found in our main specification.

There is a trade-off when choosing between the main and alternative specifications, as each imposes a different set of potentially non-innocuous identification assumptions. Neither of the two approaches can be regarded as superior and their adequacy for a particular empirical question also depends on the outcomes analyzed. As we study two types of outcomes, different assumptions may be more likely to be violated for one type than the other: While educational outcomes are probably less affected by current macroeconomic conditions (captured by the calendar year effects), they may be more sensitive to the exact age (captured by age effects). On the other hand, comparing labor-market outcomes at the same calendar time but slightly different age seems to be more innocuous than doing so at exactly the same age but at different states of economy (as reflected by calendar time). This was also confirmed by the common-trend assumptions of the two alternative specifications for the two types of outcomes discussed above. Accordingly, we conjecture that our main specification might be more suitable for

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the education outcomes, whereas the alternative specification may be more appropriate for the study of labor-market effects. While they differ in terms of the exact magnitude and the statistical significance of the estimates, they are unanimous in documenting the negative impact of the 1995 reform on long-term outcomes of children of low-educated mothers.

8.3   Other Robustness Checks

As explained in Section 5.1, our heterogeneity analysis, where we study the reform's effect by mothers' education, is conducted on a restricted sample of individuals who still live with their mother (for others, the mothers' education is unknown). While this subsample is comprised of as much as 75% of our main sample, the estimates may still suffer from sample selection bias if the decision to leave one's parents' home is affected by the reform.[64] We address this issue by estimating the impact of the 1995 reform on nest-leaving. We find zero impact of the reform on the probability that children aged 21-22 live in the same household as their mothers. Estimated coefficients are small and insignificant for all four windows (Treat 1-4) in both our main (Table 8 in Section 7) and alternative specifications (Appendix Table A.8), suggesting that the results from our heterogeneity analysis should not be affected by the selection into nest-leaving.[65]

Note that the fact that the 1995 reform had no effect on whether children in their early twenties still live with their mother is an interesting finding in itself, which complements our analysis of the family-formation outcomes discussed in Section 7 and confirms that the impact of the reform on educational and labor-market outcomes are not driven by mating, fertility or family arrangement channels.

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[64]Note that the share of those residing with their mother is even lower (around 64%, see Appendix Table A.2) for the control cohorts in our main specification, which uses their outcomes when they are older (23-24), in order to compare the treatment and control cohorts at the same calendar time. This is not the case in the alternative specification in our robustness analysis, where the treatment and control cohorts are compared in different calendar years but at the same age (21-22).

[65]We confirm this by re-running our baseline specification on the subset of individuals used in the heterogeneity analysis (those living with their mother) and obtain similar results to those for the whole sample (results available upon request).

8.4 Placebo Analysis

We estimate the impact of the 1995 reform on all children who turned 3 within the given window before or after the cutoff of October 1, comparing the last-unaffected with the first-affected cohort. While all mothers of children younger than 3 were eligible to extend their family leave to 4 years, children with younger siblings were not directly affected by their mother's decision. As the parental allowance was paid to care only for the youngest child in a family, the additional year of maternal care, as well as the restriction on the use of formal childcare at the age of 3 concerned only the youngest child.[66] Long-term outcomes of children in the last-unaffected and first-affected cohorts, who had a younger sibling at the time of the reform, should not therefore differ in response to the family leave extension on October 1.[67]

Unfortunately, the data does not allow us to precisely identify children in our treatment group who had a younger sibling at the time of the reform. We only observe siblings if both the children and their siblings still resided together with their mother at the time when we measure their long-term outcomes. In order to avoid the possible bias due to selective nest-leaving, we use all children who turned 3 before and after the reform (irrespective of the presence of a sibling) in our baseline estimation in Section 6. As an identification check, however, we can estimate the impact of the reform on a subset of children who still live with their parents and a sibling younger by less than 3 years when they are 21-22. As these children were not directly affected by the family leave extension on October 1 due to the presence of a younger sibling, we should not find any direct impact of the 1995 reform on their long-term outcomes. The results from this placebo test, presented in Table 10, confirm that there is indeed no difference in outcomes of children who turned 3 either before or after October 1, 1995 but had a younger sibling at that time.

When we exclude this subgroup of children who had a younger sibling at the time

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[66]The kindergarten attendance of children with younger siblings was not restricted in any way.

[67]There is no reason why the effect of the potential extension of maternal care for their younger sibling to 4 years should vary across these two cohorts or why children who turned 3 after October 1 should be more likely to have their kindergarten enrollment postponed than the children in the last-unaffected cohort.

Table 10: Placebo analysis

dep. var.	Tertiary				NEET			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Placebo group								
Treat*After	-0.300	-0.0437	0.0243	0.0426	0.0433	0.0244	0.0120	0.0538
	(0.267)	(0.175)	(0.122)	(0.102)	(0.136)	(0.0746)	(0.0548)	(0.0502)
Observations	141	254	367	482	141	254	367	482
R-squared	0.526	0.295	0.281	0.249	0.507	0.390	0.314	0.250
Panel B: Main sample excluding placebo group								
Treat*After	-0.0106	-0.0679	-0.0855**	-0.0790**	0.0870**	0.0594**	0.0404*	0.0434**
	(0.0703)	(0.0494)	(0.0398)	(0.0343)	(0.0420)	(0.0295)	(0.0241)	(0.0201)
Observations	860	1,678	2,488	3,314	860	1,678	2,488	3,314
R-squared	0.177	0.113	0.102	0.089	0.129	0.066	0.056	0.045

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1. Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 1 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

of the reform and were thus not differently treated by the family leave extension from our main sample, the results of our baseline specifications become even stronger, both in terms of statistical significance and magnitudes (Table 10), suggesting that our main results show only a lower bound of the true effects. In particular, the negative impact of the family leave extension on the probability of enrolling into tertiary education rises from 5 p.p. to 8 p.p. in the wider specifications (Treat 3 and 4). The size of the increase in the probability of being NEET when 21-22 years old in response to the 1995 reform also rises, from 5 p.p. to 6-8 p.p., in the two narrower specifications (Treat 1 and 3).

9 Conclusion

Too generous family leave policies may, according to a couple of recent studies, have substantial unintended negative consequences for mothers and children in the short-run. Extended maternal care was documented to have an adverse effect on children's schooling outcomes (Dustmann and Schönberg 2012 and Canaan 2022) but the evidence on the long-term impact of leave longer than several years was missing. This paper shows that the negative impact lasts and translates into poorer educational and labor-market outcomes in early adulthood.

Using the parental leave reform in the Czech Republic in 1995 that extended paid family leave until a child's 4th birthday, we show that maternal care at the age of 3, which crowds out pre-school education, reduces college enrollment and labor-market attachment at the age of 21-22. The negative impact is driven by children of low-educated parents and amounts to as much as a 12 p.p. increase in the risk for this subgroup of being neither in education nor in employment. Suggestive evidence reveals that different factors are at play behind this effect for girls and for boys. Daughters of low-educated mothers who were exposed to maternal care at the age of 3 rather than subsidized formal childcare are less likely to attend college and more likely to be inactive engaged in housework at the age of 21-22. Sons of low-educated mothers who were affected by the reform, on the other hand, are less likely to be employed.

Our findings are robust to whether the long-term outcomes of children from our treatment and control groups are compared at the same age or at the same calendar time. Our placebo test and checks for potential sample selection bias also confirm that the impact we find can be attributed to the effect of the 1995 reform. The paid family leave extension had universal eligibility and induced an additional 30 % of mothers of 3-year olds to prolong their maternal care beyond their child's 3rd birthday. As the complying mothers formed one third of all those eligible and did not differ systematically in terms of any of the observable characteristics from those who did not extend the leave, we consider our ITT estimates as fairly representative of the impact of the 1995 reform on child outcomes.

The sizeable negative impact of an extension of paid family leave on child outcomes is new to the literature evaluating the impact of family leave reforms but so is the 4-year long duration of the paid leave that we analyze. Our results are qualitatively in line with Dustmann and Schönberg (2012), the most similar study from this strand of literature, which focuses on an extension of unpaid leave from 1.5 to 3 years. The size of the effect is, however, about 20 times larger than the negative impact that they find on the probability of attending an academic high-school track that streamlines children for college.

Our estimates are much closer in magnitude to those found in the studies that evaluate the impact of universal childcare reforms on 3-year olds. The negative impact of maternal care crowding out pre-school education implied by our estimates is remarkably similar in size to the positive effect of subsidized formal childcare replacing informal care for 3-6 year olds on their college attendance and welfare dependence found in Havnes and Mogstad (2011), a study with the closest setup to us from this line of research. Their effect is also primarily driven by the children of low-educated parents. Our study complements theirs by showing that the subsidized universal childcare at the age of 3 is more beneficial not only compared to informal care but also to all-day maternal care provided by low-educated mothers.

We attribute the adverse effect of an additional year of maternal care at the age of 3 on children's long-term outcomes to an inadequate amount and quality of early childhood inputs, especially for children of low-educated mothers, when compared to the inputs these children would have received in subsidized formal childcare in the absence of the reform. In particular, the intellectual stimuli and social interactions provided as part of pre-school education in public kindergartens at that age, which are crucial for cognitive and non-cognitive skills accumulation, may not be easily substituted, especially by lower-educated mothers (Broberg et al. 1997; García et al. 2020; Loeb et al. 2007). In line with Havnes and Mogstad (2011), who show that girls are more affected than boys, we also provide suggestive evidence that the negative impact on human capital investments and labor-force attachment was, in particular, pronounced among the daughters of low-educated mothers. The fact that they focus more on home production when exposed to maternal care at the age of 3 instead of attending a public kindergarten, may also reflect a potential impact of the extended maternal care on children's pro-family values and gender role attitudes, possibly leading to earlier family formation in their early twenties, rather than studying. We test this conjecture but find no evidence of the impact of the 1995 reform on nest-leaving, marital status, cohabitation or fertility decisions. As we cannot shed more light on what factors are driving the negative impact of the prolonged maternal care on long-term child outcomes with the limited information in our data, we

leave it as an agenda for future research.

Our study is the first to show that family leave policies, typically designed to improve children's well-being, may even, if too generous and if they crowd out pre-school education, negatively impact child long-term outcomes. The fact that it is primarily the children from low socio-economic background, with low-educated parents who provide lower-quality childcare who are negatively affected has important consequences for social equity. Our findings suggest that policies that offer well-intended family leave of several years are likely to impede inter-generational mobility and increase educational, social and economic inequality.

The evidence of the adverse effect of paid family leave extension on child outcomes that harms daughters more than sons has further policy relevance. Generous family policies are often claimed to help women balance work and family and thus reduce gender inequality. While a recent study focusing on Austrian family policies and childcare subsidies over more than 50 years (Kleven et al. 2021) finds no impact on the unequal position of women and men in the labor market, our results bring an additional angle to this policy debate, suggesting that very long paid family leave may actually increase gender inequality through its greater negative impact on future outcomes of daughters than on sons.

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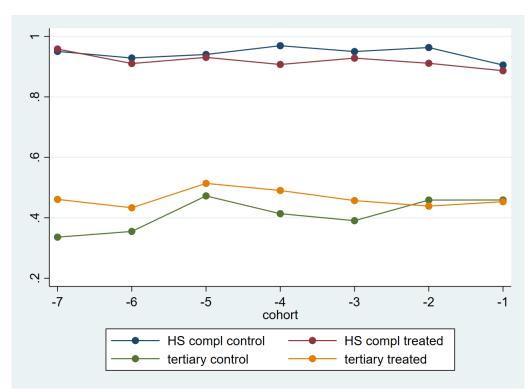
Figure A.1: Main specification - common trend assumption: education

Note: The figure shows shares of high school completion rates and of individuals who either completed tertiary education or are currently students of tertiary education. These shares are reported for the treated cohorts (born before Oct 1992, i.e. cohort -1 corresponds to individuals born in Q3 1992, cohort -2 to those born in Q2 1992, etc.) and the control cohorts (born before Oct 1990, i.e. cohort -1 corresponds to individuals born in Q3 1990, cohort -2 to those born in Q2 1990, etc.). All individuals are observed at the same year (2014). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Appendix

Table A.1: Characteristics of inactive mothers with a youngest 3-year-old child

	age	married	cohabiting	high education	spouse high education	ever worked	number of children	elderly in the HH	3-year-old is a girl	observations
Before	30.803	0.915	0.034	0.474	0.527	0.954	2.144	0.033	0.526	452
After	30.503	0.9	0.049	0.433	0.484	0.964	2.003	0.028	0.463	1081

Note: The table reports characteristics of inactive mothers with a youngest 3-year-old child born 1 year before/after the cut-off birth date, i.e. children born in Q4 1991-Q3 1992 (before) and in Q4 1992-Q3 1993 (after) (corresponding to Treat4 in Table 3).
Source: Czech Labor Force Survey data (1994-1997), own calculations.

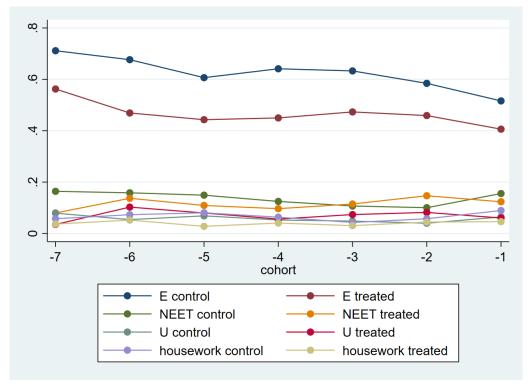Table A.2: Summary statistics for the main specification

	female	age	NEET	employed	unemployed	housework	completed l.s.e.*	completed h.s.**	completed tertiary	studying tertiary	married	living with mother	observations
Treat1=0	0.468	23.61	0.148	0.555	0.07	0.066	0.996	0.924	0.229	0.349	0.057	0.657	724
Treat1=1	0.492	21.66	0.126	0.419	0.062	0.04	0.997	0.912	0.068	0.457	0.017	0.772	724
Treat2= 0	0.483	23.61	0.135	0.561	0.062	0.06	0.997	0.935	0.238	0.36	0.06	0.653	1441
Treat2=1	0.499	21.65	0.13	0.425	0.063	0.045	0.997	0.909	0.064	0.443	0.019	0.752	1412
Treat3=0	0.487	23.61	0.134	0.575	0.064	0.059	0.998	0.929	0.24	0.342	0.059	0.63	2373
Treat3=1	0.502	21.65	0.123	0.441	0.063	0.043	0.998	0.914	0.067	0.432	0.02	0.726	2325
Treat4=0	0.492	23.62	0.131	0.57	0.061	0.058	0.998	0.933	0.242	0.351	0.056	0.64	3026
Treat4=1	0.505	21.65	0.117	0.435	0.06	0.04	0.998	0.915	0.064	0.442	0.021	0.736	3034

Note: The table reports summary statistics for the treatment and control groups used in our main specification, where the groups are observed in the same calendar time. The groups are defined by their quarter of birth in Table 3.
* lower secondary education; ** high school
Source: Czech Labor Force Survey data (2010-2016), own calculations.

Figure A.2: Main specification - common trend assumption: labor-market outcomes

Note: The figure shows shares of individuals who belong to a category of NEET, employed, unemployed, and housework. These shares are reported for the treated cohorts (born before Oct 1992, i.e. cohort -1 corresponds to individuals born in Q3 1992, cohort -2 to those born in Q2 1992, etc.) and the control cohorts (born before Oct 1990, i.e. cohort -1 corresponds to individuals born in Q3 1990, cohort -2 to those born in Q2 1990, etc.). All individuals are observed at the same year (2014). Source: Czech Labor Force Survey data (2010-2016), own calculations.
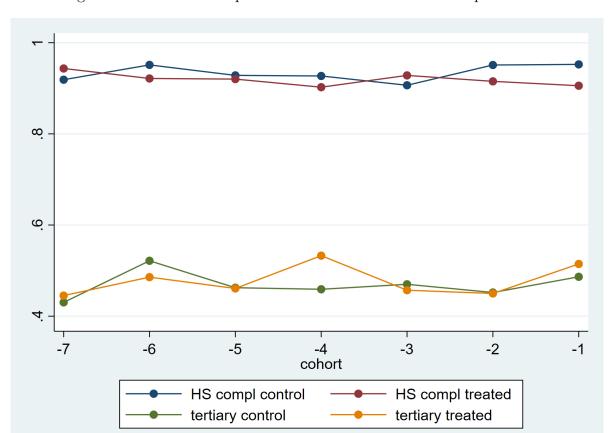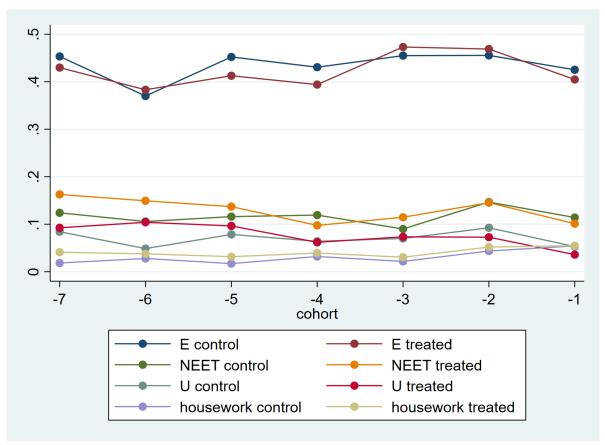
Figure A.3: Alternative specification - common trend assumption: education

Note: The figure shows shares of high school completion rates and of individuals who either completed tertiary education or are currently students of tertiary education. These shares are reported for the treated cohorts (born before Oct 1992, i.e. cohort -1 corresponds to individuals born in Q3 1992, cohort -2 to those born in Q2 1992, etc.) and the control cohorts (born before Oct 1990, i.e. cohort -1 corresponds to individuals born in Q3 1990, cohort -2 to those born in Q2 1990, etc.). All individuals are observed at the same age (21). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Figure A.4: Alternative specification - common trend assumption: labor-market outcomes

Note: The figure shows shares of individuals who belong to a category of NEET, employed, unemployed, and housework. These shares are reported for the treated cohorts (born before Oct 1992, i.e. cohort -1 corresponds to individuals born in Q3 1992, cohort -2 to those born in Q2 1992, etc.) and the control cohorts (born before Oct 1990, i.e. cohort -1 corresponds to individuals born in Q3 1990, cohort -2 to those born in Q2 1990, etc.). All individuals are observed at the same age (21). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.3: Summary statistics for the alternative specification

	female	age	NEET	employed	unemployed	housework	completed l.s.e.*	completed h.s.**	completed tertiary	studying tertiary	married	living with mother	observations
Treat1=0	0.486	22.185	0.114	0.433	0.063	0.043	0.998	0.94	0.092	0.442	0.029	0.762	815
Treat1=1	0.495	22.107	0.12	0.456	0.058	0.043	0.999	0.908	0.107	0.432	0.017	0.752	747
Treat2= 0	0.488	22.175	0.119	0.44	0.065	0.043	0.998	0.941	0.095	0.434	0.032	0.763	1809
Treat2=1	0.493	22.186	0.13	0.46	0.061	0.049	0.998	0.909	0.113	0.419	0.024	0.72	1624
Treat3=0	0.495	22.231	0.123	0.452	0.071	0.044	0.999	0.931	0.101	0.423	0.03	0.745	3239
Treat3=1	0.5	22.24	0.124	0.474	0.057	0.047	0.998	0.912	0.117	0.41	0.025	0.696	2854
Treat4=0	0.497	22.327	0.127	0.46	0.077	0.043	0.999	0.931	0.119	0.413	0.031	0.744	4343
Treat4=1	0.5	22.322	0.12	0.475	0.055	0.045	0.998	0.914	0.129	0.417	0.028	0.702	3873

Note: The table reports summary statistics for the treatment and control groups used in our alternative specification, where the groups are observed at the same age. The groups are defined by their quarter of birth in Table 3.
* lower secondary education; ** high school
Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.4: Summary statistics by living with mother

	female	age	NEET	employed	unemployed	housework	completed l.s.e.*	completed h.s.**	completed tertiary	studying tertiary	married	cohabiting	own children	observations
Panel A: Living with mother														
Before	0.497	21.62	0.077	0.39	0.06	0.005	0.999	0.937	0.065	0.515	0.003	0	0	1109
After	0.456	21.62	0.094	0.413	0.058	0.01	0.996	0.934	0.064	0.471	0.002	0	0	1003
Panel B: Not living with mother														
Before	0.577	21.73	0.204	0.513	0.047	0.152	1	0.86	0.05	0.321	0.087	0.75	0.21	343
After	0.572	21.75	0.214	0.503	0.075	0.114	0.997	0.836	0.067	0.308	0.067	0.76	0.19	360

Note: The table reports summary statistics for the treatment group 4 before and after the cut-off birth quarter (see Table 3). Panel A reports summary statistics for individuals who live with their mother in the same household, while Panel B reports them for those who do not live with their mother at the age of 21-22.
* lower secondary education; ** high school
Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.5: Alternative specification: education

dep. var.	Completed high school				Tertiary			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	0.0306	-0.0110	-0.00474	0.00582	0.00472	0.0198	0.00651	0.0224
	(0.0282)	(0.0185)	(0.0147)	(0.0124)	(0.0512)	(0.0338)	(0.0265)	(0.0223)
Observations	1,562	3,433	5,461	7,548	1,562	3,433	5,461	7,548
R-squared	0.078	0.051	0.046	0.038	0.118	0.084	0.079	0.080
Panel B: By mothers' education								
low education	0.0130	-0.0152	-0.00423	0.00273	-0.0381	0.0143	-0.0201	-0.0211
	(0.0513)	(0.0358)	(0.0290)	(0.0249)	(0.0852)	(0.0566)	(0.0453)	(0.0385)
high education	0.0605**	0.000174	0.00756	0.0155	0.0649	0.0284	0.0271	0.0441
	(0.0297)	(0.0207)	(0.0163)	(0.0134)	(0.0757)	(0.0496)	(0.0387)	(0.0328)
Observations	1,149	2,463	3,878	5,352	1,149	2,463	3,878	5,352
R-squared	0.111	0.063	0.059	0.052	0.246	0.222	0.211	0.202
Panel C: Girls by mothers' education								
low education	-0.0747	-0.0213	-0.0204	0.00220	-0.0816	0.0176	-0.0225	-0.0340
	(0.0757)	(0.0489)	(0.0395)	(0.0343)	(0.144)	(0.0954)	(0.0747)	(0.0637)
high education	0.0203	-0.00489	0.0153	0.0131	0.0167	0.0193	0.0310	0.0280
	(0.0376)	(0.0277)	(0.0204)	(0.0167)	(0.107)	(0.0682)	(0.0519)	(0.0439)
Observations	532	1,130	1,783	2,463	532	1,130	1,783	2,463
R-squared	0.203	0.117	0.088	0.069	0.313	0.247	0.228	0.191
Panel C: Boys by mothers' education								
low education	0.0754	0.00496	0.00959	0.00346	0.0517	0.00132	-0.0208	-0.00806
	(0.0745)	(0.0502)	(0.0416)	(0.0362)	(0.117)	(0.0710)	(0.0567)	(0.0476)
high education	0.132**	0.0137	0.00917	0.0203	0.0789	0.00877	0.0223	0.0577
	(0.0516)	(0.0319)	(0.0256)	(0.0212)	(0.112)	(0.0729)	(0.0579)	(0.0489)
Observations	617	1,333	2,095	2,889	617	1,333	2,095	2,889
R-squared	0.167	0.092	0.078	0.066	0.259	0.189	0.162	0.156

The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mother's education (Panels B and C)). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.6: Alternative specification: NEET and employment

dep. var.	NEET				Being employed			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	0.0362	0.0289	-0.00411	-0.00826	-0.0141	-0.0322	0.0150	0.00622
	(0.0337)	(0.0226)	(0.0177)	(0.0151)	(0.0517)	(0.0337)	(0.0264)	(0.0222)
Observations	1,562	3,433	5,461	7,548	1,562	3,433	5,461	7,548
R-squared	0.079	0.058	0.042	0.035	0.098	0.086	0.089	0.092
Panel B: By mothers' education								
low education	0.159**	0.0920**	0.0200	0.0223	-0.176*	-0.0757	0.0215	0.00610
	(0.0630)	(0.0441)	(0.0355)	(0.0302)	(0.0942)	(0.0624)	(0.0491)	(0.0415)
high education	-0.0296	-0.0133	-0.0260	-0.0295*	0.0506	-0.0397	0.00657	0.0161
	(0.0408)	(0.0277)	(0.0202)	(0.0172)	(0.0751)	(0.0487)	(0.0387)	(0.0328)
Observations	1,149	2,463	3,878	5,352	1,149	2,463	3,878	5,352
R-squared	0.104	0.066	0.056	0.042	0.167	0.150	0.140	0.137
Panel C: Girls by mothers' education								
low education	0.210**	0.171**	0.110**	0.119**	-0.155	-0.136	-0.0600	-0.0752
	(0.0905)	(0.0713)	(0.0551)	(0.0467)	(0.145)	(0.0960)	(0.0755)	(0.0633)
high education	-0.0635	-0.0178	-0.0417	-0.0311	0.0915	-0.0332	0.00740	0.00103
	(0.0626)	(0.0405)	(0.0291)	(0.0248)	(0.108)	(0.0673)	(0.0531)	(0.0452)
Observations	532	1,130	1,783	2,463	532	1,130	1,783	2,463
R-squared	0.219	0.118	0.096	0.080	0.238	0.160	0.141	0.128
Panel C: Boys by mothers' education								
low education	0.158	0.0461	-0.0396	-0.0516	-0.232*	-0.0256	0.0824	0.0650
	(0.0975)	(0.0554)	(0.0457)	(0.0394)	(0.137)	(0.0845)	(0.0661)	(0.0555)
high education	0.000882	-0.0291	-0.0255	-0.0361	0.0144	-0.0160	0.00444	0.0305
	(0.0589)	(0.0410)	(0.0297)	(0.0256)	(0.112)	(0.0725)	(0.0570)	(0.0477)
Observations	617	1,333	2,095	2,889	617	1,333	2,095	2,889
R-squared	0.152	0.102	0.083	0.051	0.206	0.144	0.128	0.124

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mother's education (Panels B and C). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.7: Alternative specification: unemployment and housework

dep. var.	Being unemployed				Inactive - housework			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Panel A: Baseline								
Treat*After	0.0274	-0.0135	-0.0129	-0.0172	-0.00982	0.0128	0.00279	0.00128
	(0.0255)	(0.0167)	(0.0131)	(0.0114)	(0.0207)	(0.0142)	(0.0111)	(0.00931)
Observations	1,562	3,433	5,461	7,548	1,562	3,433	5,461	7,548
R-squared	0.055	0.034	0.027	0.024	0.111	0.084	0.071	0.067
Panel B: By mothers' education								
low education	0.0883*	0.0214	-0.0147	-0.0180	0.0243	0.0468**	0.0328**	0.0350***
	(0.0524)	(0.0374)	(0.0295)	(0.0255)	(0.0211)	(0.0190)	(0.0148)	(0.0122)
high education	-0.0180	-0.0390*	-0.0298*	-0.0338**	-0.00319	-0.000392	-0.00331	-0.000892
	(0.0374)	(0.0234)	(0.0172)	(0.0152)	(0.0141)	(0.00759)	(0.00557)	(0.00468)
Observations	1,149	2,463	3,878	5,352	1,149	2,463	3,878	5,352
R-squared	0.084	0.054	0.049	0.039	0.075	0.059	0.045	0.038
Panel C: Girls by mothers' education								
low education	0.111	0.0926*	0.0546	0.0588	0.0411	0.0904**	0.0706**	0.0749***
	(0.0690)	(0.0561)	(0.0439)	(0.0370)	(0.0400)	(0.0407)	(0.0314)	(0.0266)
high education	-0.0563	-0.0231	-0.0210	-0.0222	-0.00393	-0.0122	-0.0150	-0.00598
	(0.0556)	(0.0324)	(0.0237)	(0.0211)	(0.0330)	(0.0192)	(0.0135)	(0.0107)
Observations	532	1,130	1,783	2,463	532	1,130	1,783	2,463
R-squared	0.192	0.098	0.075	0.056	0.154	0.092	0.068	0.055
Panel C: Boys by mothers' education								
low education	0.0565	-0.0204	-0.0642	-0.0727**	0	0.00880	0.00459	0.00330
	(0.0887)	(0.0511)	(0.0397)	(0.0348)	(0)	(0.00823)	(0.00449)	(0.00327)
high education	0.00281	-0.0623*	-0.0430*	-0.0478**	0	-2.79e-05	-0.000154	-0.000138
	(0.0536)	(0.0350)	(0.0258)	(0.0230)	(0)	(0.00143)	(0.000673)	(0.000391)
Observations	617	1,333	2,095	2,889	617	1,333	2,095	2,889
R-squared	0.140	0.101	0.080	0.057	0.	0.113	0.061	0.039

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1 (Panel A) and Treat*After interacted with two dummy variables capturing the highest level of mother's education (Panels B and C). Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 3 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Table A.8: Alternative specification: marital status and family outcomes

dep. var.	Living with mother				Married			
	Treat1	Treat2	Treat3	Treat4	Treat1	Treat2	Treat3	Treat4
Treat*After	0.0218	-0.00875	-0.0153	-0.000691	0.0165	0.00948	0.00999	0.00460
	(0.0429)	(0.0287)	(0.0228)	(0.0193)	(0.0151)	(0.0109)	(0.00868)	(0.00773)
Observations	1,562	3,433	5,461	7,548	1,562	3,433	5,461	7,548
R-squared	0.158	0.139	0.128	0.130	0.088	0.066	0.051	0.046
dep. var.	Having own children				Cohabiting			
Treat*After	-0.0284	-0.00299	-0.0108	-0.0109	-0.0482	-0.0119	-0.00983	-0.0122
	(0.0234)	(0.0160)	(0.0126)	(0.0108)	(0.0394)	(0.0262)	(0.0208)	(0.0176)
Observations	1,562	3,433	5,461	7,548	1,562	3,433	5,461	7,548
R-squared	0.115	0.081	0.065	0.068	0.143	0.125	0.119	0.111

Note: The table reports estimated coefficients of the interaction term Treat*After from equation 1. Results are presented for four different treatment and control groups (Treat1-Treat4), which differ by the size of the window around the cut-off birth date – October 1992 (see Table 1 for details). All regressions control for a full set of control variables. Standard errors are in parentheses (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Source: Czech Labor Force Survey data (2010-2016), own calculations.

Abstrakt

Tento článek zkoumá vliv prodloužení placené rodičovské dovolené ze 3 na 4 roky na dlouhodobé výsledky dětí. K odhadu vlivu využíváme metodu rozdílů v rozdílech a srovnání poslední kohorty dětí ještě nezasažených reformou s první kohotou, kterou již reforma ovlivnila. Zjišťujeme, že dodatečný rok mateřské péče, která vytěsnila především péči poskytovanou ve státem dotovaných předškolních zařízeních, má negativní vliv na investice do lidského kapitálu a zapojení na trhu práce v rané dospělosti dětí méně vzdělaných matek. Zasažené děti mají o 12 p.p. vyšší pravděpodobnost, že ve věku 21-22 let nestudují, nepracují, ani se na zaměstnání nepřipravují (NEET). Vliv reformy je výraznější u dcer, u kterých je způsobený především nižší pravděpodobností studia na vysoké škole a vyšší pravděpodobností práce v domácnosti. Oproti tomu synové méně vzdělaných matek mají nižší pravděpodobnost zaměstnanosti. Výsledky naznačují, že docházka do předškolního zařízení může být ve věku 3 let přínosnější než celodenní péče matky, obzvlášť u dětí z nižších sociálně-ekonomických vrstev.