Parental Allowance Increase and Labour Supply: Evidence from a Czech Reform*

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Abstract

We study the effect of a CZK 80,000 (36%) increase in parental allowance, a universal basic income-type benefit, on the labor supply of parents in the Czech Republic. Drawing a parental allowance does not preclude labor market activity, which allows us to study the income effect. After the reform, mothers substantially prolonged the average period they drew an allowance. The labor market participation of mothers of young children decreased by 6 percentage points (15%). The estimated effect corresponds to a non-labor income labor supply elasticity at the extensive margin of about -0.5. The effect is particularly strong among mothers with their first child (10 p.p. or 28%) and among university-educated mothers (16 p.p. or 36%). We observe a virtually identical reduction in hours worked. We found no effect on the labor supply of fathers.

Keywords: Parental allowance, Maternal labor supply, Income effect of social policy, Czech Republic

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1 Introduction

The ability of cash transfers to allow prompt responses to unexpected economic shocks with little administrative cost has made them popular and widely used by policymakers (Loeser et al., 2021).[1] Standard economic models predict that providing households with financial transfers generally evokes the income effect and leads to a decrease in labor force participation. A potential labor supply reduction may substantially reverse the intended anti-poverty effect of cash transfers. However, empirical evaluation of cash-transfer programs is often lacking, leaving policymakers uncertain about the consequences of such policies.[2] A prominent example of cash transfers that trigger the income effect is a parental allowance, a universal basic income-type benefit for families with children. Recently, the heated debate in the US about the expected effects of a possible replacement of the existing child tax credit with a parental allowance on the labor supply of parents demonstrates the importance of this topic (Corinth et al., 2022).

In this paper, we exploit an increase in parental allowance, a universal basic income-type benefit, in the Czech Republic, to study the labor supply effect of parents with children under the age of 4. Regardless of their previous income, each recipient of parental allowance is entitled to a total fixed amount and can choose the amount of monthly installments, which in turn determines the length of time they will draw the parental allowance. The reform increased the total amount available to parents by CZK 80,000 (€ 3,250), 36% greater than the previous amount. Any parent with a child below the age of 4 who was drawing a parental allowance on and after January 1st, 2020, was eligible for the increase. Labor market activity and child-care enrolment for children older than 2 do not effectively preclude drawing a parental allowance.[3] The design of the reform thus allows us to study

[1] For example, monetary compensation schemes played a crucial role in mitigating the adverse effects of income shock on disposable income during the Covid-19 crisis in many countries (Christl et al., 2022).

[2] Despite growing literature on the effects of cash-transfer programs (among recent papers see e.g., Hancock et al., 2019; Lieber and Lockwood, 2019; Garay et al., 2020; Matikka and Paukkeri, 2022; Sanches and Carvalho, 2022), the effects of many such policies remain unknown.

[3] Formally, under Czech parental allowance rules, the child cannot be enrolled in institutional child-care for more than 92 hours per month while s/he is younger than 24 months. Since the limit is quite generous and the public sector lacks means of control, this is hardly a binding restriction for mothers. In addition, most Czech mothers stay home with their children longer than two years. There is no limit on institutional child care for children older than 24 months.

the income effect of cash transfers on a policy-relevant population of parents with young children.

We rely on the difference-in-differences approach and show that the extra CZK 80,000 significantly reduced maternal labor market activity. In our preferred specification, the labor force participation of mothers with children 2 and 3 years old decreased by 6 percentage points (15%). Similarly, hours worked per week fell by 2.2 hours (16%). In alternative specifications, using a wider definition of the treatment group as mothers with a youngest child between the ages of 1 and 3, and a more narrow definition of mothers with a youngest child aged 3, the drop in maternal labor supply remains convincingly stable between 15% and 17%. We found zero effect on fathers.

The average effect masks a sizeable heterogeneity. The decline in labor market supply was more pronounced among mothers who drew the parental allowance for their first child. These mothers reduced their labor force participation by 10 percentage points (26%), and their weekly hours worked fell by 4 (32%). Mothers with a second and next child decreased their labor supply by 4 percentage points, and the effect is only marginally statistically significant. We argue that mothers with one child are likely to have another in near future, and the extra CZK 80,000 allowed them to cover the period before the second child is born. The estimated effect corresponds to a non-labor income labor supply elasticity at the extensive margin of about -0.8.

The effect of the reform also varies with the mothers' educational attainment. University-educated mothers decreased their labor market participation rate by 16 percentage points (33%). Hours worked also fell by almost a third. In contrast, we do not find a statistically significant effect of the reform among mothers without university education. Before the reform, university-educated mothers generally took shorter periods of parental leave and drew the parental allowances at a faster rate; therefore, we argue that they had the opportunity to prolong parental allowance more than other mothers. This, in turn, led to a larger drop in maternal labor market activity among the higher-educated mothers.

The Czech government's intention to increase the parental allowance budget was publicly known at least several months before it came into effect. To receive the extra funding, many

recipients reduced their monthly installments in late 2019 and postponed the termination of their allowance until after January 1st, 2022, which qualified them for the extra CZK 80,000. The postponed termination of the allowance contributes to the estimated effect.

Our results are consistent with a labor supply model in which mothers benefit from an unexpected non-labor income shock and buy more time out of paid work to stay home with their children. Administrative data reveal a substantial increase in the level of monthly allowance payments, but also a change in the duration of drawing of parental allowance. After the reform (at least in the short term), recipients substantially prolonged the average drawing of allowance.

Our research is most closely related to the literature studying the effects of child benefits provided independently of labor market activity. The recent US debate illustrates the importance of the labor supply effects of large policies such as cash transfers. Corinth et al. (2022) show that by ignoring the labor supply effect of child allowances, the anti-poverty effect of cash transfers is largely overstated as compared to other measures such as child tax credits. Further, evidence from Germany suggests that providing an unconditional child allowance implies a strong response of the intensive margin of maternal labor supply, but no reaction from fathers (Hener, 2016; Tamm, 2010). Current evidence from Denmark shows a substantial effect of a cap on unconditional child benefits on labor supply (Jensen and Blundell, 2021). Overall, cash transfers are rarely studied, and their effects are likely sensitive to the specific institutional set-up and identification strategies. Unlike most previous studies, we employ a difference-in-differences strategy with the treatment group defined by the age of the youngest child in the household. Our results show that mothers with young children up to the age of 4 reduce their employment, which is qualitatively similar to previous studies. It should be noted that parents in our sample began to draw parental allowance before January 1st, 2020. We do not study long-term effects of the reform, for example, on parents who began to draw the allowance after the reform became effective.

Our research also adds to the literature that aims to disentangle the sources of the impact of motherhood on employment. In many countries with generous family policies (including the Czech Republic), the employment rate of childless women is more than 20 percentage

points higher than that of women with at least one child below the age of 6 (see a summary by Bičáková and Kalíšková (2022)). These countries offer long leave, generous parental benefits, and low provision of child care.

Several published papers study the consequences of long parental leave on maternal labor supply in the Czech context. Bičáková and Kalíšková (2019) and Mullerova (2017) analyze the causal impacts of the extended duration of paid parental leave in 1995 from the third to the fourth birthday of a child and show a substantial impact of the possibility of longer leave and take-up of allowance on labor market inactivity. The high elasticity of labor supply with respect to the design of parental allowances has been confirmed by several studies that analyze the reversal of the trend when the possibility of shorter paid leave with higher replacement rates is introduced (Bičáková and Kalíšková, 2019; Pertold-Gebicka, 2020). Introducing greater flexibility and the possibility of shorter leaves led to lower inactivity and higher employment in highly skilled occupations. Overall, existing literature suggests that the labor market attachment of Czech mothers with children under 5 is largely driven by the design of parental leave and allowance, which is also supported by our results.

2 Institutional Details and Data

2.1 Parental Support in the Czech Republic

Czech family policy is characterized by generous provision of family benefits (primarily maternity benefits and parental allowance) and various tax reliefs and credits for parents (OECD., 2019). Direct parental support consists of maternity and parental leave and a parental allowance. Children are entitled to slots in public kindergarten after they reach the age of 3 before the end of August in a given calendar year.

Mothers are entitled to 28 weeks of maternity leave, which starts 6 to 8 weeks before the expected birth date. During the whole period of maternity leave, mothers are entitled to sickness insurance benefits. After termination of maternity leave for mothers and beginning on the child's birth date for fathers, parents are entitled to parental leave until the child reaches the age of three. During and immediately after the three-year period,

their previous employer is required to re-hire parents who apply for work, and to assign them to a job that fulfills the parameters of their prior employment contract.

Only one parent can draw parental allowance. The allowance is a fixed amount allocated per child paid in monthly payments. The recipient chooses the monthly amount, which, in turn, determines how long the allowance will continue. The monthly maximum amount is set at 70% of an assessment base (the higher of the two parents or the administrative maximum, which was, e.g., CZK 47,700 (EUR 2,000) in 2022). The choice of the amount can be changed every 3 months. The parental allowance is untested financial support not tied to how a parent uses his/her parental leave. Parents drawing a parental allowance may engage in an occupational activity without losing their entitlement. According to the Ministry of Labour and Social Affairs (MoLSA), mothers take parental leave and draw parental allowance in 98% of cases, and fathers make up the remaining 2%.

The reform we study increased the total amount of parental allowance for a single child by CZK 80,000 (36%), from CZK 220,000 (ca. EUR 8,900) to 300,000 CZK (ca. EUR 12,100). Any recipient drawing parental allowance on and after January 1st, 2020, with a child under the age of 4 was entitled to the extra amount. The reform also increased the maximum number of hours a child under the age of 2 years could spend in institutional care from 46 to 92 hours per month.

The reform was publicly discussed for several months before it came into effect on January 1st, 2020. The main features of the reform were approved by the government on the 21st of May, more than seven months before the law became effective. Mainstream media first informed the public of the reform in May and covered it extensively in November and December when the law was being discussed in the Parliament. In November 2019, the MoLSA published detailed information about the reform on its webpage[4,5]. In December 2019 and January 2020, Czech Labour Offices (an integrated part of the MoLSA) sent a letter with detailed information to all parents wwho were currently drawing the parental allowance, to inform them about the reform.[6]

[4]https://www.mpsv.cz/rodicovsky-prispevek

[5]https://ct24.ceskatelevize.cz/domaci/2980874-je-potreba-zadat-jak-stihnout-penize-vycerpat-manual-radi-jak-navysit-rodicovskou

[6]https://ct24.ceskatelevize.cz/domaci/3020231-kvuli-rodicovske-rozeslaly-urady-dopisy-po-jejich-precteni-ale-pretrvavaji-otazky

The default setting of the reform was to increase the length of time it would be drawn, and to keep the monthly payment unchanged. Depending on the age of the children and parent's initial drawing plan, the reform had different implications. First, parents who had planned to terminate their allowances after January 1st, 2020, continued to draw it automatically. Because the eligibility condition is limited by the child's age, it is possible that parents with a child close to the age of 4 would not be able to draw the whole amount. Collecting the remaining funds as a lump sum payment was not an option. To draw the full amount, parents with children close to the age of 4 were motivated to increase the amount of payment after January 1st, 2020.

Second, parents who had planned to terminate the allowance before January 1st, 2020, could temporarily decrease their monthly payment in 2019 and postpone the planned termination of the allowance after January 1st, 2020. This would entitle them to an increase in the total amount of their parental allowance. Similar to the previous scenario, depending on the child's age, the parents could draw either the whole amount of CZK 80,000 or a fraction of it. Figure 1 represents the cases.

2.2 Data

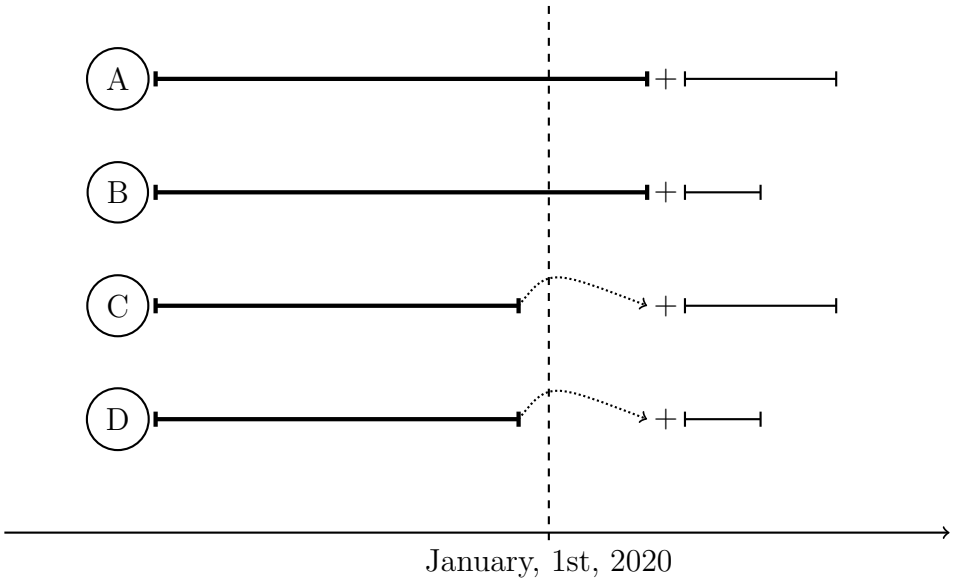
We use the Labor Force Survey (LFS) data for the Czech Republic from 2017-2020.[7,8] The data are collected quarterly on a representative sample of Czech households, and include detailed characteristics of all members. The possibility to observe all household members allows us to link parents with their children. Importantly, the LFS includes information on employment status, hours worked, the structure of households, age, the highest education level attained, and more. The LFS data do not include information about respondents' income. Respondents are selected using a two-stage sampling procedure.[9] The rotating panel consists of around 50 thousand individuals (0.6% of Czech households), from which 20% is replaced each quarter. We particularly focus on mothers of young children, who

[7]Detailed description of the Czech LFS and EU LFS can be found here: `https://www.czso.cz/csu/vykazy/vyberove_setreni_pracovnich_sil`

[8]We use Czech and Slovak EU LFS for a robustness test. For details about the data and our robustness exercise, see Appendix A.

[9]The sampling unit is the census district in the first and the dwelling in the second stage. There are over 54 thousand census districts in the Czech Republic, defined by the maximum of 140 dwellings or 400 inhabitants. One dwelling can include multiple households.

Figure 1: **Consequences of the Reform**



Notes: This figure shows the possible implications of the reform. Mother A had planned to terminate drawing her parental allowance after January 1st, 2020. She did not need to adjust her plan and the reform only increased the total amount of parental allowance she could draw by CZK 80,000. Mother B is similar to mother A, but because her child would be 4 years old soon after January 1st, she could draw only a fraction of the extra amount. Mother C had planned to terminate her allowance *just* before January 1st, 2020. When news of the reform became public, she could reduce her monthly payment to prolong the drawing period after January 1st. As a result, she increased her total parental allowance by CZK 80,000. Mother D is similar to mother C, but because her child turned 4 years old soon after January 1st, she could draw only a fraction of the extra amount.

were primary targets of the increase in parental allowance (98% of parental allowance recipients in 2020 were mothers: see Section 4 for details). The descriptive statistics of the subsample of mothers in our study are presented in Table A1.

Our main outcome variables are labor force participation and hours worked in a regular week. Labor force participation is an indicator variable that equals 1 if the respondent is either an employee, self-employed, or is actively looking for a job. In the remaining cases, e.g., parental leave, retirement, and student status, labor market participation equals zero. Changing labor market participation status does not require market interaction, and respondents can do that promptly. The other main variable measures the number of hours worked in a regular week.

To provide additional insights, we collect our own survey data. In fall 2021, we surveyed 1,098 parents (50% males, 50% females) of young children. The median age of the youngest child of the surveyed parents was 4. A third of the respondents were affected by the reform. The survey was conducted by Behavio, a private company that administrates a panel of

regular respondents, and contains questions regarding parents' knowledge of the reform, their response to the reform (adjustment of the duration of the allowance), and the actual length of parental leave.

Finally, we use monthly national administrative statistics of the number of parents drawing parental allowance by child age and the assessment base, the total amount paid, and the number of requests to change the monthly installments.

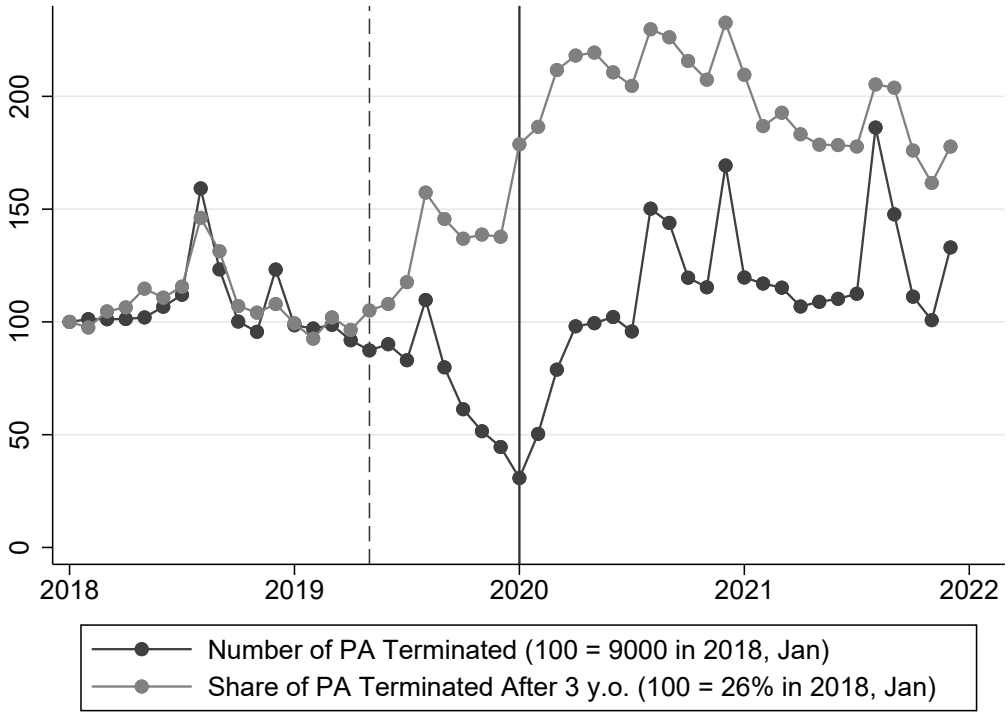
3 Parental Allowance

We document three patterns in the duration and the average amount of monthly payments of parental allowances using administrative data aggregated by months provided by the MoLSA. First, the total number of terminated allowances decreased rapidly just before the reform, which we interpret as evidence that recipients adjusted their allowance plans to postpone termination until after January 1st, 2020. Second, the share of recipients who drew a parental allowance until the child was at least 3 years old doubled after the reform. Third, the average monthly payment increased by 40% in the first months after the reform.

Figure 2 shows the evolution of the numbers of termination of parental allowances over time normalized to 100 in January 2018 (black line). The number of terminated allowances had been declining since May 2019, reaching a minimum of 30% of the baseline level in January 2020. The numbers returned to the baseline in April 2020. In the second half of 2020, terminations increased again to above baseline level, with double peaks at the beginning of the new academic year (the final month of allowance falling in August and September) and December 2020.

Figure 2 further documents a substantial increase in the number of long-term parental allowances (grey line). The number of allowances terminated after a child reached the age of 3 doubled from pre-reform levels. In 2018, about 25% parental allowances were terminated after the child reached 3: this increased to almost 60% in the months after the reform was instituted. Increases in long-term allowances are consistent with Figure A1a, which shows that the number of parents receiving a parental allowance in a given month increased from about 280,000 in the pre-reform period to 320,000 (a 14% increase) in the

Figure 2: **Parental Allowance Termination**



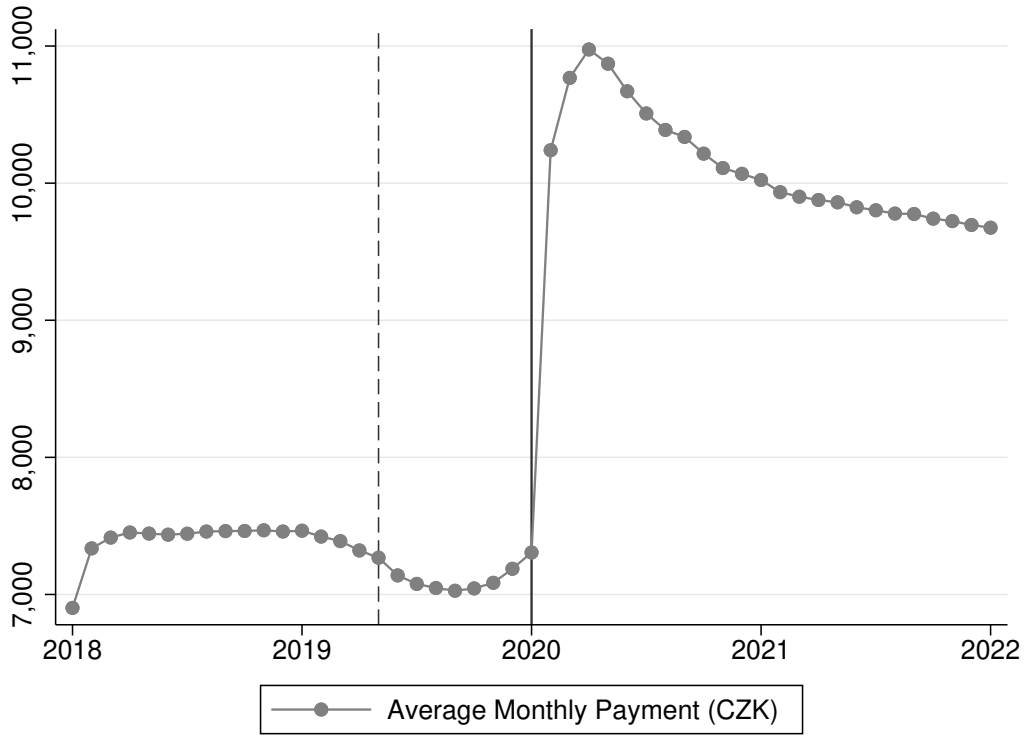
Notes: This figure shows the relative frequency of parental allowances termination and share of termination after a child reached the age of 3 over time. Both measures are normalized to 100 in January 2018. The dashed vertical line corresponds to May 2019, when the reform was approved by the government, and the solid line marks January 2020, when the reform was instituted. The number of terminated parental allowances fell before January 2020, and reached the minimum at 30% in January. The share of long parental allowances more than doubled after the reform, from about 26% to 60%.

first months after the reform. Overall, recipients postponed termination of their allowances, and, as a result, more recipients drew allowances for longer periods.

The average monthly payment increased from CZK 7,306 in December 2019 to CZK 10,240 (40%) in January 2020 and peaked in April at almost CZK 11,000 (50%). Despite a decreasing trend from April 2020, the average payment in 2022 remained at approximately CZK 2,500 (or 35%) above the pre-reform level. Since the default implementation of the reform kept the planned amount fixed and prolonged the period of allowances, to increase the amount, recipients had to proactively change their monthly payments at the Czech Labour Office. Figure A1b in Appendix A shows that 144,000 recipients adjusted their monthly payments immediately in January 2020. Over the first three months of 2020, more than 190,000 (or 60%) of recipients changed their monthly payments.[10]

[10]Over the first three months of 2021, 26,000 recipients changed the amount, while more than 160,000 (or 50%) of recipients changed their amount in response to the reform over the first three months of 2020.

Figure 3: **Average Monthly Payment**



Notes: The figure shows the average monthly payment amount of parental allowance in CZK. After the reform in January 2020, the average payment increased by 40%. The payment increases were accompanied by an increase in the number of recipients.

Further, our survey data suggest extensive awareness of the reform among parents. More than 90% of respondents who were drawing a parental allowance and who terminated the allowance after January 2020 and were thus affected by the reform correctly answered that the reform increased the total amount of parental allowance in a multiple choice type of question with 5 options.[11] University-educated respondents were more likely to provide the correct answer (97%) than less educated respondents (88%; p-value of t-test 0.003). Because the survey was conducted more than 18 months after the reform was instituted, it is not perfectly informative about the prevalence of information at the time of the reform, Similarly, we are careful in our interpretation of the observed heterogeneity: university-educated respondents may simply have been more likely to remember the reform, for example, because they were exposed to the reform and to the information about the reform.

[11] As of 1 January 2020, the rules for drawing parental allowance have changed. Without searching, do you know what has changed?: (i) An increase in the total amount from CZK 220k to CZK 300k; (ii) The father of the child can now draw the parental allowance; (iii) Grandparents can also draw parental allowance; (iv) The reform reduced the maximum duration of the drawing; (v) None of the above. The order of options was randomized at the individual level.

4 Mothers in the Labor Market

We have shown that the reform prolonged the drawing period and increased the average monthly payment. Next, we next present how the reform impacted the maternal labor market. We add heterogeneity of the effect by first-child status and by mothers' education level.

4.1 Empirical Strategy

To identify the causal effect, we rely on the difference-in-differences approach. The control and treatment groups are defined by the age of the youngest child, which is a necessary eligibility condition for parental allowance. The control group in all specifications contains mothers whose youngest child is older than 4 and who are thus certainly ineligible for a parental allowance. The treatment group consists of mothers with a child younger than 4. The actual exposure to the reform is further restricted to mothers (parents) drawing parental allowance on and after January 1st, 2020. Unfortunately, we do not observe the timing of parental allowances at the individual level. As a result, some mothers classified in the treatment group were not exposed to the increase in parental allowance.

Compared to standard difference-in-differences literature, the policy change in our setting was anticipated, and many recipients adjusted their allowance plans. As a result, the treatment group includes mothers who would otherwise have chosen a shorter allowance period. If mothers who intentionally prolonged their allowance periods are less likely to work than they would have been without the reform, the treatment effect is stronger. The possibility of prolonging the parental allowance is thus one of the channels through which the policy change reduced the labor supply of mothers of young children. The mechanism does not invalidate the internal validity of our results. However, it complicates interpretation of the estimated coefficients. We discuss estimated effects in more detail in Appendix B.

We are unaware of any policy reform that coincided or overlapped with the increase in parental allowance and that could have disproportionately affected the labor supply of mothers of either older or younger children. Another potential concern is the Covid-19

pandemic and anti-Covid measures. While childcare institutions were never closed by the central government, many were temporarily closed if teachers or any children were under quarantine or isolation mandates. Most closures occurred in the fall of 2020, several quarters after the effect of the January 2020 increase in parental allowance appeared. Hence, childcare institution closures cannot explain the observed reduction in labor supply. To provide additional evidence that the labor supply drop was not driven by Covid-19, we also implement a triple difference estimator using data on Slovak mothers.[12] A final concern is that mothers who are in the treatment group in early 2020 shifted to a control group as their youngest children become older than 4 years. The spillover effect from the treatment to the control group may underestimate the true effect.

4.2 Effect on Mothers

Figure 4 captures the labor force participation of mothers between the age of 18-64 years by the age of their youngest child. The vertical line at age 4 represents the child age cut-off for eligibility for a parental allowance. The grey and the black dots represent maternal labor force participation rates before and after the increase in parental allowance, respectively.[13] A visual inspection reveals a systematic downward shift among mothers with children younger than 4, i.e., mothers in the treatment group. The effect is especially sizeable among mothers whose youngest child is two (6.6 p.p) and three (10.2 p.p.).[14] Figure 5 replicates Figure 4 using average hours worked as the outcome variable. After the reform, mothers with a youngest child aged two and three worked significantly fewer hours than before the reform.

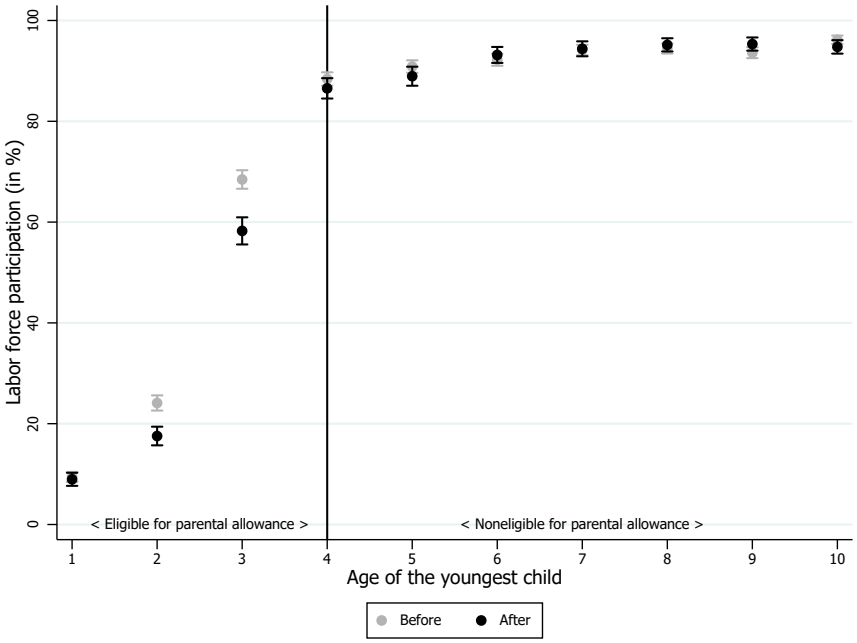
These findings suggest that the prolonged parental allowance led mothers to reduce their labor supply. This interpretation is also consistent with our complementary survey. Roughly 60% of the respondents who themselves or whose partner prolonged the parental allowance (N=145), reported that they also postponed their return to work by 7.5 months on average.

[12] Results are presented in Appendix B.

[13] The period before the reform is defined as Q1 2018 - Q3 2019, while the period after is Q1 2020 - Q4 2020.

[14] We find no effect of the increased parental allowance on fathers' labor supply. For details, see Section A1.3 in Appendix A.

Figure 4: **Labor Force Participation of Mothers**

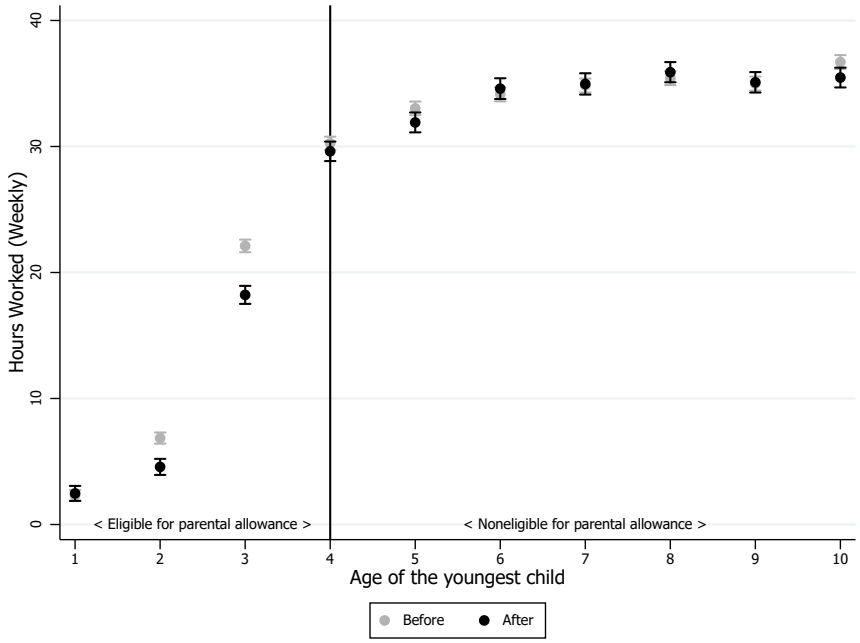


Notes: The figure shows shares of mothers (women 18-64 y.o. with at least one child of ten or younger) who actively participate in the labor market. The period before the reform is defined as Q1 2018 - Q3 2019, while the period after is Q1 2020 - Q4 2020. To exclude the anticipatory effect of the reform, the figure disregards the last quarter of 2019. The grey vertical line shows the child's age, one of the eligibility criteria. Mothers on the right of the vertical line are ineligible, while mothers on the left meet the child age eligibility criterion. 95 % confidence intervals are plotted.

Table 1 shows results for three specifications of the treatment and control groups for each outcome variable. The panel on the left shows our preferred specification of the treatment group with mothers whose youngest child was between the ages of 2.00 and 3.99. The control group is symmetric around the age of 4 years and consists of mothers whose youngest child was 4.00 to 5.99. The increase in parental allowance reduced maternal labor market participation by 6.1 percentage points (15%). The effect is statistically significant at the 1% level. The second column shows that the effect on average hours worked per week is also negative and statistically significant: The increase in parental allowance reduces hours worked by 2.2 per week (16%).

In the middle panel of Table 1, the treatment group consists of mothers whose youngest child was between the ages of 1.00 and 3.99, while the control group consists of mothers whose youngest child was 4.00 to 6.99. Under this specification, the increase in parental allowance reduces the labor participation rate by 4.6 p.p. (16%) and hours worked by 1.6 per week (17%). In the third specification, both treatment and control groups are

Figure 5: **Hours Worked by Mothers**



Notes: The figure shows the usual hours worked among women 18-64 y.o. with at least one child of ten or younger. The period before is defined as Q1 2018 - Q3 2019, while the period after is Q1 2020 - Q4 2020. To exclude the anticipatory effect of the reform, the figure disregards the last quarter of 2019. The grey vertical line shows the child's age, one of the eligibility criteria. Mothers on the right of the vertical line are ineligible, while mothers on the left of the vertical line meet the child age eligibility criterion. 95 % confidence intervals are plotted.

narrower than in the baseline. Mothers whose youngest child is between 3.00 and 3.99 are in the treatment group, whereas mothers whose youngest child is between 4.00 to 4.99 fall in the control group. Labor force participation decreases by 8.6 p.p. (13%) and hours worked by 3.4 per week (15%).

The effects on labor force participation and hours worked are negative and economically and statistically significant in all specifications. The effects are also convincingly stable across all specifications. The decline in labor force participation ranges from 13% to 16% and the effect on hours worked from 15% to 17%. Our results imply an aggregate short-term non-labor income labor supply elasticity of mothers with young children of -0.5.

4.3 Effect on Mothers by First-child Status

Prolonging a parental allowance and postponing a return to work is especially convenient for mothers who plan to have another child. They may use the extra money to cover the transition period without labor income. Since we cannot identify which mothers planned

Table 1: **Labor Force Participation and Hours Worked**
Different Treatment Groups, Mothers, Diff-in-diff Estimates

	Treated: 2-3 y.o.		Treated: 1-3 y.o.		Treated: 3 y.o.	
	LFP	HW	LFP	HW	LFP	HW
Post	-0.015	-0.764**	-0.006	-0.269	-0.016	-0.561
	(0.009)	(0.389)	(0.007)	(0.309)	(0.014)	(0.565)
Treated	-0.424***	-17.260***	-0.538***	-21.569***	-0.189***	-7.881***
	(0.009)	(0.334)	(0.007)	(0.266)	(0.012)	(0.483)
Post*Treated	-0.061***	-2.209***	-0.046***	-1.633***	-0.086***	-3.360***
	(0.014)	(0.551)	(0.011)	(0.411)	(0.021)	(0.851)
N	14,774	14,774	22,817	22,817	7,007	7,007
Adj. R-Square	0.28	0.30	0.38	0.41	0.16	0.16
Pre, Treated Mean	0.41	13.84	0.29	9.53	0.64	22.62

Notes: The table shows difference-in-differences estimates. The treatment group is mothers with 2-3 y.o. children in the first, 1-3 y.o. children in the second, and 3 y.o. children in the third column. The control groups are mothers with older children, organized symmetrically around the cutoff age of 4 y.o. children, i.e., the control group for mothers with 1-3 y.o. children are mothers with 4-6 y.o. children, etc. We control for education, age, quarters of the year, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2019 to mitigate the anticipation effect. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.[15]

another child, we rely on the expectation that mothers with a first child are likely to plan for a second child, and study the heterogeneous treatment effect with respect to first-child status. Our results show that the effect was indeed more pronounced among mothers exposed to the increase in parental allowance while they were actively drawing the parental allowance for their first child.

We re-estimate our preferred specification (mothers with a child between the ages of 2.00-3.99 in the treatment group) on a sample of mothers with only one child and on a sample of mothers who have an older child equal to or under the age of 10 at the time they give birth to their youngest child. The control groups are mothers with corresponding first-child status and those with children in the same age group, i.e., the control group for the mothers with their first child is mothers who have just one child between the ages of 4 and 5.

The left panel of Table 2 shows that the reform decreased the labor market participation rate among mothers with a first child by 10 percentage points (26%). The decline in hours worked is even more pronounced. Mothers reduced their working hours by 4 per

week (32%). The difference between the decrease in labor force participation and hours worked suggests an increase in part-time employment contracts after the reform among mothers with their first child. The right panel of Table 2 displays results for mothers who already had at least one child under 10 before they gave birth to their youngest child. They reduced their labor participation status by 4 percentage points (10%), and reduced their hours worked by 1.2 per week (8%). Note that the effect on mothers who already had a child is only marginally significant for labor force participation and not significant for hours worked. The results are thus consistent with the effect being largely driven by mothers who used the extra money to cover the period between their first and second child.

Table 2: **Labor Force Participation and Hours Worked**
By First Child Status, Mothers, Diff-in-diff Estimates

	With First Child		Already Have a Child	
	LFP	HW	LFP	HW
Post	-0.010	0.228	-0.007	-1.111**
	(0.017)	(0.701)	(0.013)	(0.542)
Treated	-0.450***	-18.185***	-0.416***	-16.742***
	(0.014)	(0.560)	(0.012)	(0.468)
Post*Treated	-0.100***	-4.009***	-0.040*	-1.187
	(0.023)	(0.899)	(0.021)	(0.783)
N	5,641	5,641	7,340	7,340
Adj. R-Square	0.30	0.32	0.27	0.29
Pre, Treated Mean	0.38	12.46	0.42	14.18

Notes: The table shows the difference-in-differences estimates. The treatment group is mothers with 2-3 y.o. children. The control group is mothers with 4-5 y.o. children. The regression equations are estimated separately for mothers with their first child and mothers who already have a child 10 or younger. We control for education, age, quarter, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2019 to mitigate the anticipation effect. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.

We rely on the triple difference estimator and test the effect between mothers with their first child and mothers who already had a child. The results confirm that the reduction in labor force participation among mothers with their first child is about 6.2 percentage points larger than among mothers who had a child before. Similarly, the effect on hours worked is 3 hours per week larger; for full results, see Table A2 in Appendix A.

The difference in the effect by the first child status is stable for several quarters after the policy change. Using the sample from Q1 2017 to Q4 2020 and our preferred specification, we estimate the following regressions for mothers with their first child and mothers who already had a child.

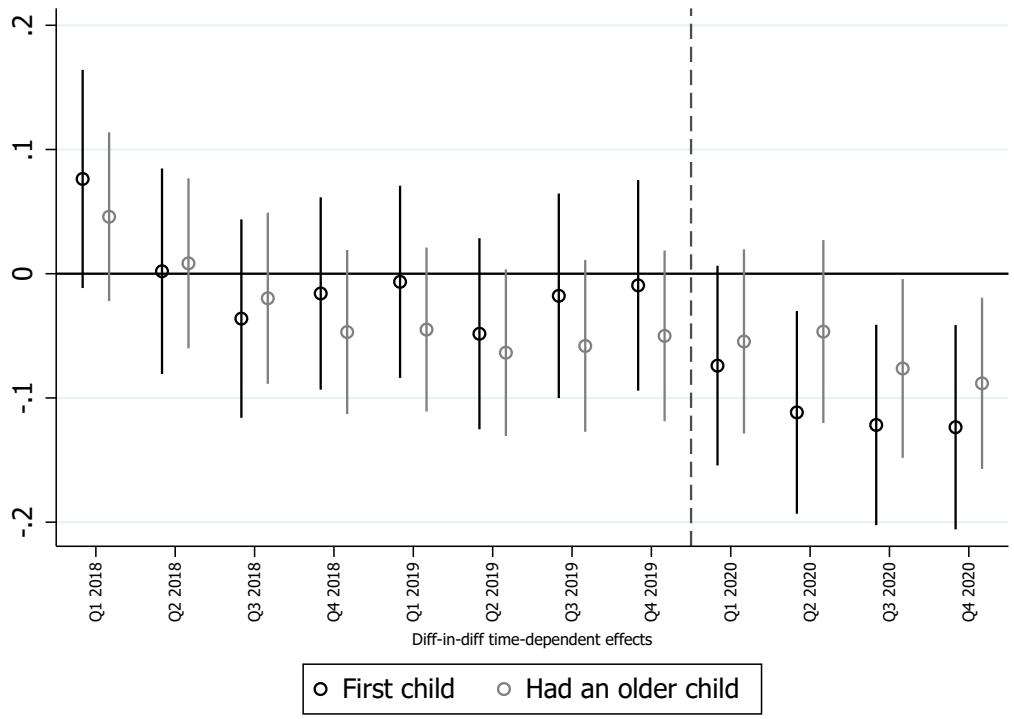
$$y = \alpha + \beta_1 MotChild2to3 + \sum_k \beta_2^k Q^k + \sum_k \beta_3^k Q^k * MothChild2to3 + \gamma X + \varepsilon \qquad (1)$$

where $k \in \{Q1\ 2018, Q4\ 2020\}$; y stands for either labor force participation or hours worked; α represents a conditional average value of y for the control group (mothers with a youngest child aged 4.00 to 5.99) over four quarters in 2017; β_1 represents the conditional difference between the treatment and control groups in 2017; β_2^k represents the conditional difference between the control group in the quarter k and average in 2017; and β_3^k represents the difference-in-differences estimates in a quarter k. We estimate the same specification for labor force participation status and hours worked.

Figure 6 plots β_3^k coefficients from regression 1 estimated on samples of mothers with their first child (black) and mothers who had a child already (grey). A visual inspection confirms that the treatment effect is about 10 percentage points (using the average of 2017 year as a baseline) among mothers with only one child. After a marginally significant drop in the first quarter after the reform, labor force participation falls by more than 10 percentage points for the next three quarters. There seems to be no treatment effect among mothers who already had a child. The labor force participation rate was about 5 percentage points lower in the last quarter of 2018 and throughout 2019. The figure thus confirms a substantial and time persistent effect among mothers with their first child and no effect among mothers who had a child already.

Figure 7 plots the β_3^k coefficients from a regression with hours worked as the outcome variable. The figure confirms the cumulative effects from Table 2: mothers with a new first child worked 5 hours per week fewer. The effect occurs immediately after the reform and remains stable over the next four quarters. In contrast, there is no effect on mothers who had an older child already.

Figure 6: **Labor Force Participation**
By First Child, Diff-in-diff Estimates with Quarterly Interactions



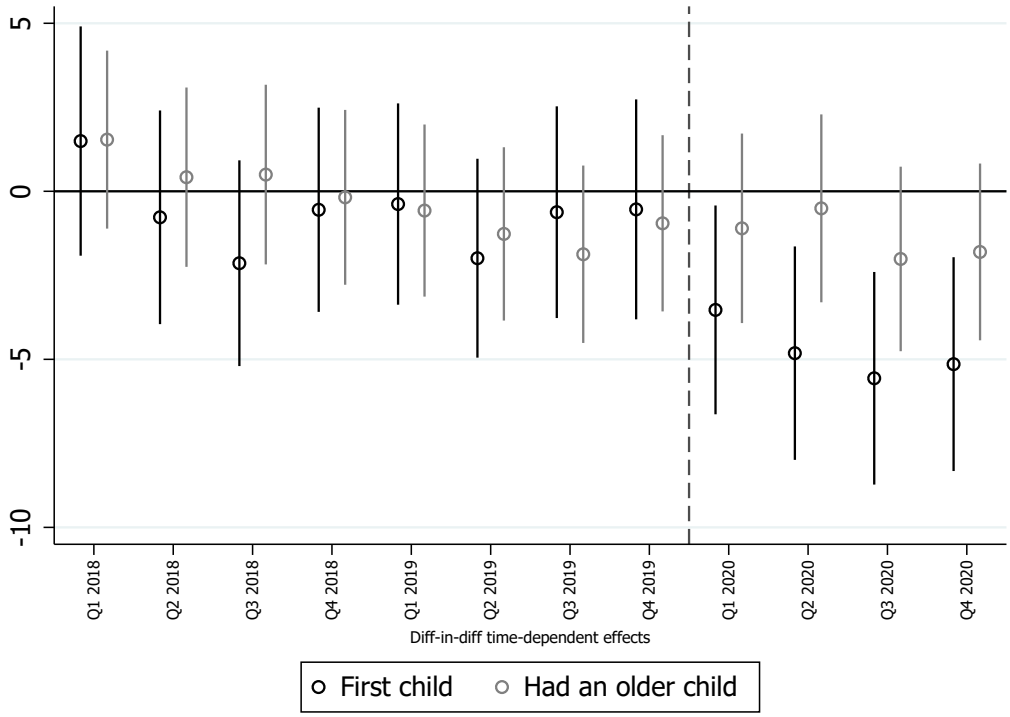
Notes: The dependent variable is labor force participation. The sample consists of women 18-64 y.o. with children 2.00-5.99 y.o. The graph shows the interaction coefficients from the diff-in-differences regression where the interaction of the treatment (mothers with children 2.00-3.99) and the "post" period is carried out separately for each quarter between 2018 and 2020. The regression equations control for age, post period by quarterly dummies, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household.

4.4 Effects on Mothers by Education Attained

In the Czech system, university-educated mothers tend to take shorter parental leave and draw parental allowance faster. In our survey data, for example, about 80% of mothers without a university education took parental leave at least until the child was 3, while only 60% of university-educated mothers did so. The survey data further reveal that about 60% of all mothers intended to return to their previous employer, which sets a soft constraint on the termination of parental leave when the child reaches the age of 3, because after this milestone, employers are not required to re-hire the mothers. Taking these two observations together, we argue that university-educated mothers are less constrained in prolonging the length of their parental leave and parental allowance.

Figure 8 applies our survey data and shows lengths of parental leave for university-educated mothers and mothers without university education before and after the reform. A visual

Figure 7: **Hours Worked**
By First Child, Diff-in-diff Estimates with Quarterly Interactions



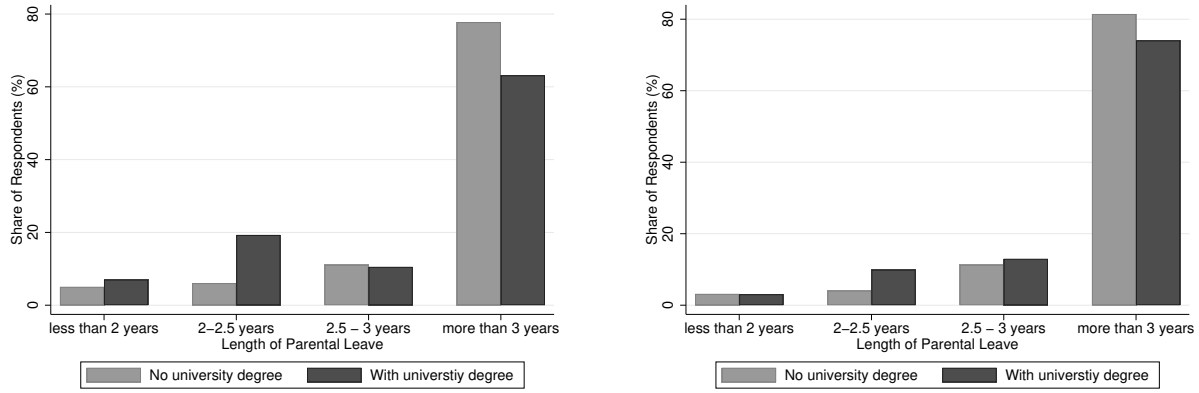
Notes: The dependent variable is hours worked. The sample consists of women 18-64 y.o. with children 2-5 y.o. The graph shows the interaction coefficients from the diff-in-differences regression where the interaction of the treatment (mothers with children 2-3 y.o.) and the "post" period is carried out separately for each quarter between 2018 and 2020. The regression equations control for age, post period by quarterly dummies, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household.

comparison confirms that, after the reform, the share of mothers taking parental leave at least until the child is 3 increased, and the effect is more pronounced among university-educated mothers. We next test whether an increase in the length of parental leave among university-educated mothers is associated with a more pronounced reduction in labor market activity.

For the next exercise, we define four educational attainment groups: primary school, high-school without a general exam, high school with a general exam, and a university degree. Anyone who has obtained at least an undergraduate degree is considered to hold a university degree. For each group, we separately estimate the main specification (treatment group: children aged 2-3; control group: aged 4-5).

Table 3 shows that the effect on labor market participation and hours worked is concentrated among highly educated mothers. A decrease in the share of labor market participation and

Figure 8: **Length of Parental Leave**



(a) Before the Reform (b) After the Reform

Notes: This figure is based on our survey data. The left panel (a) shows the frequency of different lengths of parental leaves before the reform, for university-educated mothers and mothers without a university education. The right panel (b) shows the same for mothers who were affected by the reform.

hours worked is evident only among university-educated mothers. The effect is substantial. Their labor force participation fell by 16.4 percentage points (33 %) and hours worked by 4.8 hours per week (30%). In the rest of the section, we restrict our attention to two groups: university-educated mothers and a collapsed group of those without university educations.

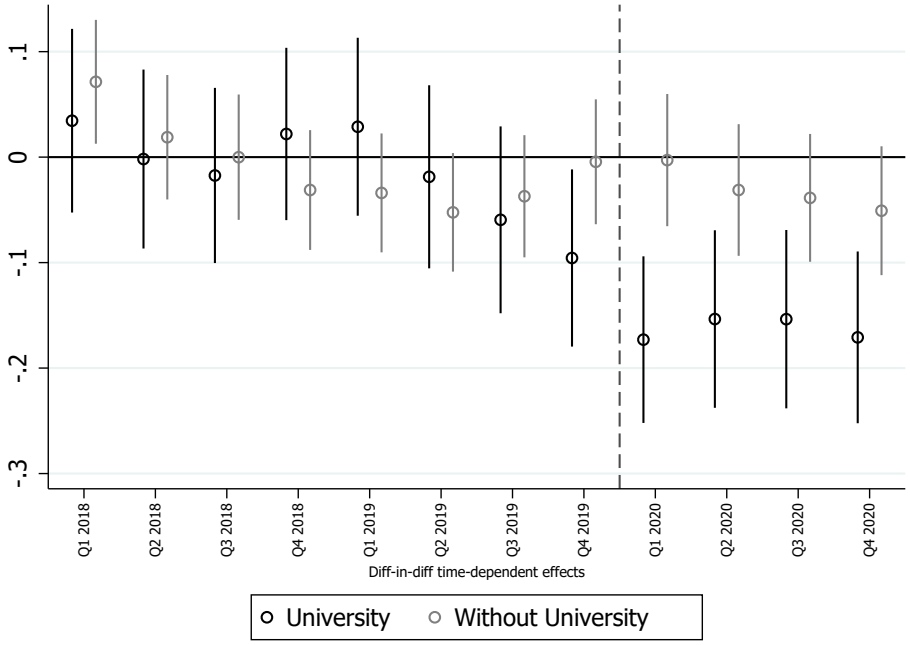
Table 3: **Labor Force Participation and Hours Worked**
By Education, Mothers, Diff-in-diff Estimates

	Primary		Secondary		Secondary with GE		University	
	LFP	HW	LFP	HW	LFP	HW	LFP	HW
Post	-0.083*	-2.383	-0.000	-0.619	-0.034**	-0.998*	0.027**	-0.141
	(0.046)	(1.872)	(0.023)	(0.900)	(0.014)	(0.597)	(0.013)	(0.594)
Treated	-0.297***	-11.423***	-0.444***	-18.150***	-0.455***	-18.280***	-0.394***	-16.841***
	(0.032)	(1.206)	(0.020)	(0.745)	(0.013)	(0.512)	(0.015)	(0.597)
Post*Treated	0.067	1.481	-0.053	-1.251	-0.010	-1.080	-0.164***	-4.768***
	(0.059)	(2.370)	(0.033)	(1.231)	(0.022)	(0.870)	(0.023)	(0.916)
N	1,158	1,158	3,133	3,133	6,135	6,135	4,348	4,348
Adj. R-Square	0.31	0.30	0.29	0.31	0.28	0.31	0.31	0.34
Pre, Treated Mean	0.23	8.27	0.33	11.30	0.42	14.14	0.50	16.24

Notes: The table shows the difference-in-differences estimates. The treatment group is mothers with 2-3 y.o. children. The control group is mothers of 4-5 y.o. children. The regression equations are estimated separately for mothers with primary, secondary, secondary with GE, and a university education. We control for age, quarters of the year, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2019 to mitigate the anticipation effect. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.

To test for statistically significant heterogeneity with respect to university education, we run a triple difference estimator. Results presented in Table A3 in Appendix A confirm that treatment heterogeneity is statistically significant. In particular, labor force participation among university-educated mothers declined by 13 percentage points more than among the less educated, and hours worked fell by almost 3 hours more.

Figure 9: **Labor Force Participation**
By Education Groups, Diff-in-diff Estimates with Quarterly Interactions



Notes: The dependent variable is labor force participation. The sample consists of women 18-64 y.o. with children 2-5 y.o. The graph shows the interaction coefficients from the difference-in-differences regression equation where the interaction of the treatment (mothers with 2-3 y.o.) and the "post" period is carried out separately for each quarter between 2018 and 2020. The regression equations control for age, post period by quarterly dummies, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household.

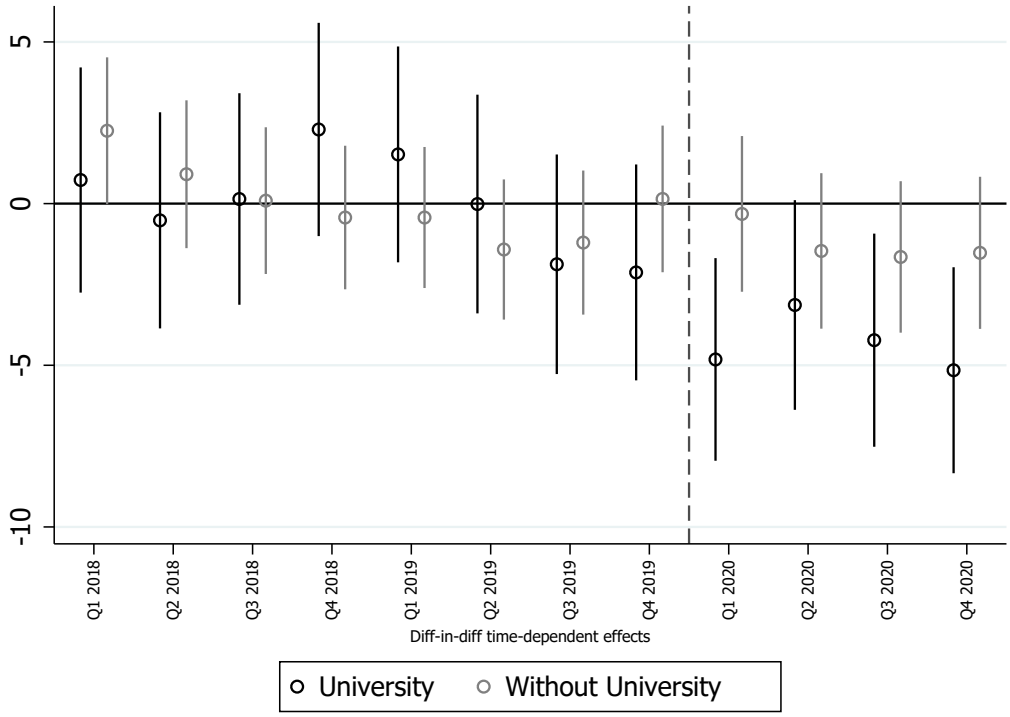
The effect on university-educated mothers seems stable over several quarters immediately after the treatment. Figure 9 plots coefficients β_3^k from regression 2. Interestingly, university-educated mothers already decreased their labor market participation rate in the last quarter of 2019.[16] There seems to be no effect among mothers without a university education. Figure 10 replicates the exercise with hours worked as the outcome variable and

[16]The drop in Q4 2019 among university-educated mothers is consistent with anticipation of the reform and the longest opportunity to adjust the length of their parental allowance. Many university-educated mothers who had planned to return to the workforce in Q4 2019 postponed their return, reducing labor force participation in that group.

confirms a stable effect among university-educated mothers and no effect among mothers without a university education.

$$y = \alpha + \beta_1 UniverEduc + \sum_k \beta_2^k Q^k + \sum_k \beta_3^k Q^k * UniverEduc + \gamma X + \varepsilon \qquad (2)$$

Figure 10: **Hours Worked**
By Education Groups, Diff-in-diff Estimates with Quarterly Interactions



Notes: The dependent variable is weekly hours worked. The sample consists of women 18-64 y.o. with children 2-5 y.o. The graph shows the interaction coefficients from the difference-in-differences regression equation where the interaction of the treatment (mothers with 2-3 y.o.) and the "post" period is carried out separately for each quarter between 2018 and 2020. The regression equations control for age, post period by quarterly dummies, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household.

Our survey data further support the observed heterogeneity. University-educated mothers in our survey sample postponed their return to work by 9.3 months, whereas less-educated mothers by 7.1 months (p-value of t-test 0.051). The results are based on 76 respondents who indicated that they or their partners intentionally prolonged parental allowance to become eligible for the increase in parental allowance amount.

5 Discussion

Our estimates imply a relatively large reduction in labor market activity caused by the income effect of an increased parental allowance. We discuss two reasons that can explain the magnitude of the effects. First, compared to policies that target the general population, this reform impacted only mothers (parents) of young children, whose non-labor income labor supply is arguably more elastic. While the selectively targeted population limits generalization of our effects, our estimates remain highly policy-relevant, as many social and family policies target the same type of population of mothers of young children.

Second, we believe that the impact of the reform is sensitive to the status quo of the mothers' labor market participation status: To return to work is a different decision than the decision to leave the labor market to care for a child. In our setting, many of the mothers were exposed to the reform during their existing parental leave, while they were already drawing an allowance. Their initial labor market inactivity further contributed to the magnitude of the effect. Consequently, we would expect less labor supply reduction if working mothers were given an extra CZK 80,000.

Part of the effect is only temporal and will fade out. The possibility to adjust the payment schedule and to prolong the payment period contributes to the effect. Part of the reduction of the labor market activity is thus driven by a temporal excess of mothers who opted to prolong their parental allowance. Figures 3 and A1a show decreasing trends in the average monthly payment and in the total number of monthly payments paid out in a given month. Both these measures support our view that a part of the estimated effect is temporal and that our estimates may not apply to a long-term time horizon, including mothers who initiate parental allowance after the reform was instituted, with its increased total amount of CZK 300,000.

A potential propagation channel of the effect is an increase in the fertility rate. As the outside option to labor market participation increases, staying home with a child becomes a more attractive choice, as it reduces the opportunity cost of parenting (i.e., the substitution effect).[17] We test for the effect on the fertility rate using a difference-

[17]So long as another child is a normal good, the mechanism is further boosted by the income effect.

in-differences approach. Figure A4 in Appendix A shows that there is no fertility effect among university-educated mothers, among whom we documented the largest impact on labor market activity. However, we observe an increased fertility rate among mothers with a high-school education, a group with little or no reaction to the increase in parental allowance on labor market activities.

The fertility effect of the reform among high-school educated mothers appears too early after the reform. To observe an increase in fertility rates in Q1 of 2020, parents would need to plan for a new child as early as Q3 of 2019, which coincides with the time of the announcement of the reform. Evaluating this effect is more problematic, because, even if we observe an increase in fertility in the months after the policy reform, it may be only a temporal substitution.

While Covid-19 cannot explain the effect initiated in Q1 2020, it could contribute directly (mothers postpone return to the labor market due to health concerns) or indirectly (anti-Covid measures such as the closure of child-care institutions) to labor supply reduction. For example, adverse circumstances and prospects caused by Covid-19 may have led some mothers to postpone their return to the labor market. To provide additional evidence that our effect is not driven by Covid-19, we run a triple difference estimator using Czech and Slovak mothers. The Czech Republic and Slovakia are similar countries with similar family policies. The first wave of the pandemic hit these countries in similar fashion, with lockdowns imposed at the same time. The estimated effect of the parental allowance increase remains negative and of a similar magnitude. The full results are reported in Appendix A.

6 Concluding Remarks

We study an increase in parental allowance in the Czech Republic and estimate the labor supply effects of unconditional cash transfers on the population of mothers with young children. Each recipient of parental allowance is entitled to a fixed budget and chooses their monthly installments, which in turn determines the length of the parental allowance. The reform we study increased the total amount available by 36%, and any parent whose

child was under the age of 4 and who drew parental allowance on and after January 1st, 2020 received up to CZK 80,000 more. First, using administrative and our own survey data, we show that the reform led to longer parental leave and parental allowances. The share of parents who ended their parental allowance after their child's third birthday doubled after the reform. The increase in parental allowance reduced maternal labor market participation by roughly 15% and hours worked by 2.2 per week. The drop in labor force participation is more pronounced among university-educated mothers than less-educated mothers. Mothers who drew a parental allowance on their first child also reduced their labor supply more than mothers with two and more children. We find no effect on the labor supply of fathers, which is not surprising, as fathers represent only two percent of all recipients of parental allowance.

Our results add to the ongoing discussion about potential unintended effects of unconditional cash transfers, which in our case resulted in lower employment of mothers. Furthermore, we also add to the literature studying sources of motherhood employment and the gender pay gap, which is still substantial in many European countries including the Czech Republic. Our evidence suggests that access to an unconditional parental allowance may be an important source of the relatively low labor market attachment of mothers of young children. Finally, we estimate the short-term effect on ongoing recipients of the parental allowance with a default option to prolong the duration of the allowance. The potential effect of the reform on parents who started their parental allowance after the reform may differ.

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A1 Appendix A

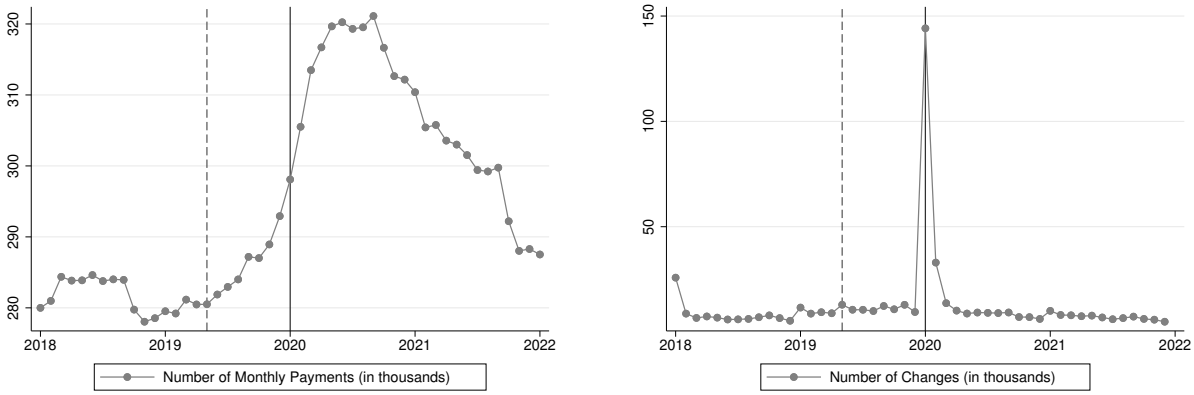
Table A1: **Descriptive Statistics**
Mothers, Treatment and Control Groups, Q1 2019 & 2020

	Q1 2019			***Q1 2020***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Treated						
LFP	0.41	0.49	816	0.38	0.49	721
HW	13.80	18.06	816	12.09	17.25	721
Age	33.60	5.55	816	33.34	5.44	721
Grandparent	0.03	0.18	816	0.04	0.20	721
HH size	3.80	1.03	816	3.77	1.07	721
MS: Single	0.30	0.46	816	0.30	0.46	721
MS: Married	0.65	0.48	816	0.64	0.48	721
MS: Widowed	0.01	0.08	816	0.00	0.06	721
MS: Divorced	0.04	0.21	816	0.06	0.23	721
Educ.: Primary	0.08	0.28	816	0.07	0.26	721
Educ.: Secondary	0.23	0.42	816	0.18	0.38	721
Educ.: Secondary with GE	0.40	0.49	816	0.41	0.49	721
Educ.: University	0.28	0.45	816	0.33	0.47	721
Controls						
LFP	0.86	0.34	625	0.87	0.34	518
HW	31.75	14.43	625	31.83	14.33	518
Age	36.09	5.30	625	36.21	5.27	518
Grandparent	0.04	0.20	625	0.06	0.23	518
HH size	3.87	1.13	625	3.87	1.07	518
MS: Single	0.26	0.44	625	0.27	0.44	518
MS: Married	0.64	0.48	625	0.65	0.48	518
MS: Widowed	0.00	0.04	625	0.00	0.06	518
MS: Divorced	0.10	0.29	625	0.08	0.27	518
Educ.: Primary	0.08	0.27	625	0.08	0.26	518
Educ.: Secondary	0.20	0.40	625	0.22	0.41	518
Educ.: Secondary with GE	0.43	0.50	625	0.41	0.49	518
Educ.: University	0.28	0.45	625	0.28	0.45	518

Notes: The table shows descriptive statistics for mothers with children 2-3 y.o. (treatment group) and 4-5 y.o. (control group) for Q1 2019 (before the reform) and Q1 2020 (after the reform).

A1.1 Figures

Figure A1: **Parental Allowance**

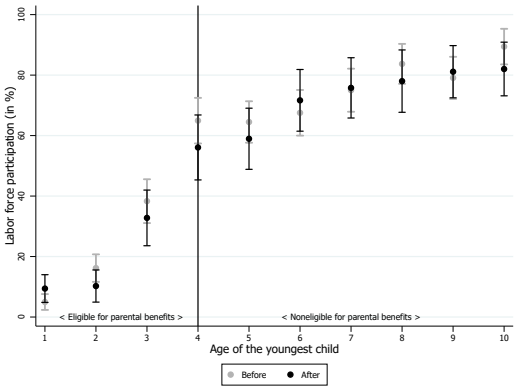


(a) Number of Monthly Payments (b) Number of Changes

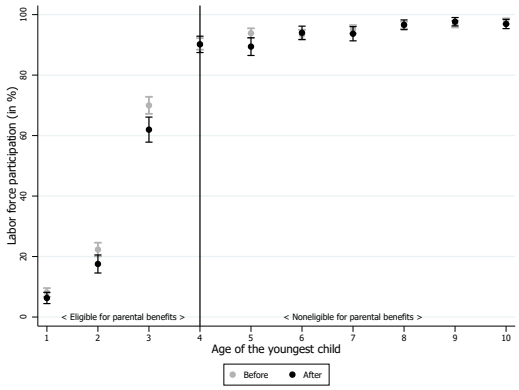
Notes: The left panel (a) shows the number of parental allowance payments in a given month. As recipients tend to postpone the termination of their allowances, the number of parents drawing the parental allowance increased by 40,000 after the reform. The right panel (b) shows the number of changes in the monthly amount. In the first three months after the reform, almost 60% of the recipients changed their amount.

Figure A2: **Education Heterogeneity**

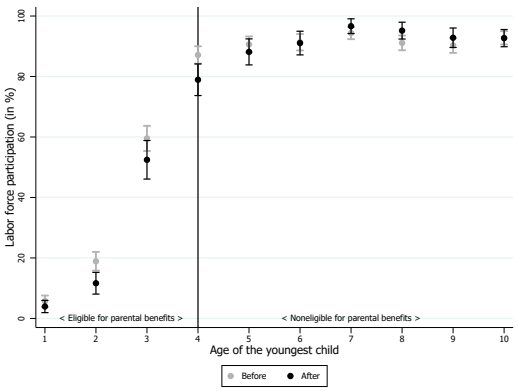
(a) **LFP of Mothers**
Primary Education



(c) **LFP of Mothers**
Second. w. GE Educ.



(b) **LFP of Mothers**
Secondary Education



(d) **LFP of Mothers**
University Education

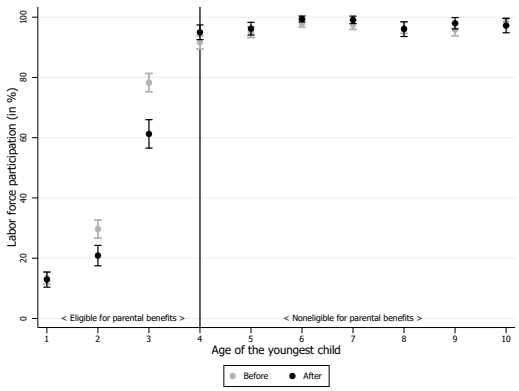
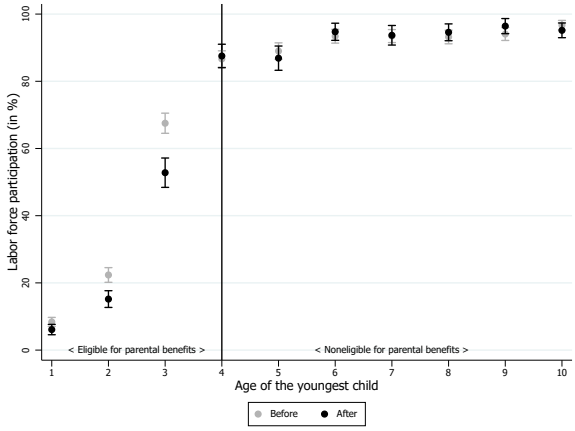


Figure A3: **First Child Heterogeneity**

(a) **LFP of Mothers**
With First Child



(b) **LFP of Mothers**
Had an Older Child

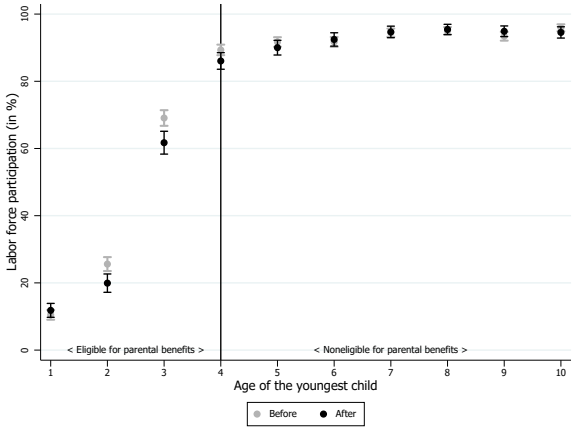
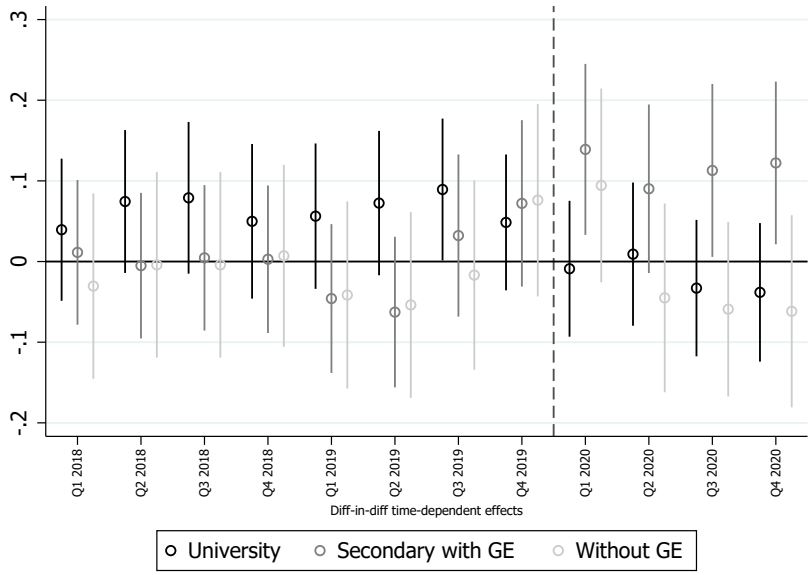


Figure A4: **Fertility**
By Education, Diff-in-diff Estimates with Quarterly Interactions



Notes: The dependent variable is the indicator of whether women have a child younger than 1 y.o. The sample consists of women 18-64 y.o. with children <5 y.o. The graph shows the interaction coefficients from the difference-in-differences regression equation where the interaction of the treatment (woman has a child younger than 1 y.o.) and "post" period is carried out separately for each quarter between 2018 and 2020. The regression equations control for education, age, post period by quarterly dummies, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household.

A1.2 Tables

Table A2: **Labor Force Participation and Hours Worked**
Mothers, Interactions with First Child, Triple Diff-in-diff Estimates

	LFP	HW
First Ch.	0.020	0.732
	(0.020)	(0.795)
Post	0.003	-0.720
	(0.013)	(0.545)
Post*First Ch.	-0.013	0.957
	(0.021)	(0.884)
Treated	-0.407***	-16.384***
	(0.012)	(0.465)
Treated*First Ch.	-0.027	-1.269*
	(0.018)	(0.711)
Post*Treated	-0.041**	-1.345*
	(0.021)	(0.781)
Post*Treated*First Ch.	-0.062**	-2.984**
	(0.031)	(1.188)
N	13,127	13,127
Adj. R-Square	0.26	0.28
Pre, Treated Mean	0.40	13.40

Notes: The table shows triple difference-in-differences estimates. The treatment group includes mothers with a youngest child 2-3 y.o. The control group includes mothers with 4-5 y.o. children. On top of standard difference-in-differences interactions, we also interact "post" and "treatment" variables with a binary indicator of whether the child is first-born in a family. The base category corresponds to mothers who already had a child before the youngest one. We control for age, education, quarters of the year, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2020 to mitigate the anticipation effect. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.

Table A3: **Labor Force Participation and Hours Worked**
Mothers, Interactions with University Education, Triple Diff-in-diff Estimates

	LFP	HW
Educ: University	0.067***	2.769***
	(0.011)	(0.457)
Post	-0.026**	-0.748
	(0.012)	(0.497)
Post*Educ:University	0.044***	0.263
	(0.017)	(0.757)
Treated	-0.418***	-16.718***
	(0.010)	(0.401)
Treated*Educ:University	0.012	-0.694
	(0.018)	(0.696)
Post*Treated	-0.023	-1.425**
	(0.018)	(0.680)
Post*Treated*Educ:University	-0.129***	-2.894**
	(0.029)	(1.134)
N	14,928	14,928
Adj. R-Square	0.26	0.28
Pre, Treated Mean	0.41	13.84

Notes: The table shows triple difference-in-differences estimates. The treatment group includes mothers with a youngest child 2-3 y.o. The control group includes mothers with 4-5 y.o. children. On top of standard difference-in-differences interactions, we also interact "post" and "treatment" variables with a binary indicator of whether the mother obtained a university education. The base category corresponds to mothers without a university education. We control for age, quarters of the year, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2019 to mitigate the anticipation effect. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.

A1.3 Fathers in Labor Market

Figure A5: **Fathers' Labor Force Participation**
Before-After Comparison

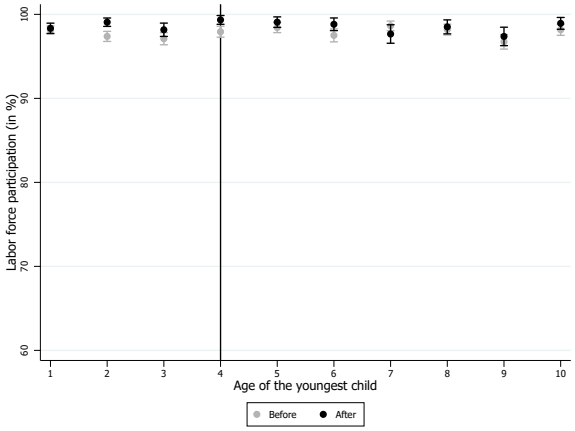


Table A4: **Labor Force Participation and Hours Worked**
Fathers, Diff-in-diff Estimates

	Baseline		University Education		First Child	
	LFP	HW	LFP	HW	LFP	HW
Post	0.003	-0.393	0.010	-1.008**	0.011	0.547
	(0.005)	(0.285)	(0.006)	(0.442)	(0.008)	(0.433)
Treated	-0.009**	-0.124	0.005	0.469	-0.009	0.322
	(0.004)	(0.233)	(0.005)	(0.387)	(0.006)	(0.349)
Post*Treated	0.006	-0.136	-0.003	0.228	-0.002	-0.818
	(0.007)	(0.366)	(0.007)	(0.557)	(0.010)	(0.543)
N	12,457	12,457	2,965	2,965	4,578	4,578
Adj. R-Square	0.08	0.07	0.06	0.09	0.05	0.07
Pre, Treated Mean	0.96	40.86	0.99	42.35	0.97	41.07

Notes: The table shows the difference-in-differences estimates for fathers. The treatment group includes fathers with a youngest child 2-3 y.o. The control group includes fathers with 4-5 y.o. children. The first panel corresponds to our baseline specification. The second panel includes only fathers with a university education. The third panel shows results for fathers of first-born children. We control for education, age, quarters of the year, presence of grandparents in the household, county of residence, family status, household size, and relationship to the head of household. The estimates are based on Q1 2018 - Q4 2020 data; we exclude Q4 2019 to mitigate the announcement effect. Significance levels (robust standard errors in parenthesis): *** 0.01, ** 0.05, * 0.1.

A1.4 Czech and Slovak Mothers

We use the EU Labour Force Survey data for the Czech Republic and Slovakia to apply a cross-border difference-in-differences estimation strategy.[18] Slovakia and the Czech Republic share similar historical backgrounds and have similar legislative environments and public institutions. Due to their geographic proximity, the spread of Covid-19 infections and policy responses were timed similarly through 2020. This is supported by strong co-movements in Stringency indexes for the Czech Republic and Slovakia during 2020.[19,20]

We estimate the following equation.

$$y = \alpha + \beta_1 \text{CZE} + \beta_2 \text{Child 0-4} + \beta_3 \text{Post} + \beta_4 \text{CZE} \times \text{Child 0-4} + \beta_5 \text{CZE} \times \text{Post} + \beta_6 \text{Child 0-4} \times \text{Post} + \beta_7 \text{CZE} \times \text{Child 0-4} \times \text{Post} + \varepsilon$$

The estimates from the triple difference-in-differences estimator, using a subsample of mothers, are shown in Table A5. Similarly to our baseline specification, the dependent variables are labor force participation and hours worked. The independent variable "CZE" is a binary indicator with a value equal to one for Czech mothers and zero for Slovak mothers. The variable "Post" is equal to one if an observation was collected in any quarter of 2020 and zero if collected in quarters between 2018-2019. The variable "Child 0-4" is equal to one if a mother has a child between 0 and 4 and is equal to zero if it is a mother with a child between 5 and 9[21] The coefficients of interest (β_7) are shown in the last row of Table A5. Results are qualitatively consistent with our baseline results in Table 1, suggesting that the decrease in the labor force participation and hours worked was larger for Czech mothers of children 0 and 4 However, the estimates from Table A5 are not

[18]For details about EU LFS, see `https://ec.europa.eu/eurostat/web/lfs`.

[19]The Stringency Index incorporates nine response metrics to Covid-19 (such as school closure, workplace closure, restrictions on internal movements, etc.; for details, see `https://github.com/OxCGRT/covid-policy-tracker/blob/master/documentation/index_methodology.md`)

[20]See `https://ourworldindata.org/metrics-explained-covid19-stringency-index`).

[21]The EU LFS data are less detailed than the Czech LFS used in our main analysis. Specifically, respondents' age is indicated by five-years age groups. Since the maximum length of parental leave in Slovakia and the Czech Republic differ (parents in Slovakia can collect parental allowance until a child reaches the age of three; the age of four in the Czech Republic), the groups of mothers with children 0 and 4 also include those not eligible for the parental allowance. This, however, does not invalidate our results. The estimates can be considered lower-bound estimates, making them not directly comparable to results presented in Section 4.

directly comparable in magnitude to results presented in Section 4 due to the different data and methodology applied.

Table A5: **Labor Force Participation and Hours Worked**
Mothers, Diff-in-diff Estimates, CZE-SVK Comparison

	All mothers		With University Educ.		With First Children	
	LFP	HW	LFP	HW	LFP	HW
CZE	0.074***	1.472***	0.026***	-0.985*	0.067***	1.324***
	(0.006)	(0.279)	(0.009)	(0.524)	(0.007)	(0.310)
Post	-0.030***	-3.759***	-0.018	-2.463***	-0.031***	-3.565***
	(0.010)	(0.432)	(0.014)	(0.809)	(0.010)	(0.480)
Child 0-4	-0.310***	-12.075***	-0.384***	-15.540***	-0.290***	-11.529***
	(0.008)	(0.309)	(0.013)	(0.563)	(0.010)	(0.391)
Post*Child 0-4	0.007	1.698***	-0.015	-0.060	0.028	1.780***
	(0.014)	(0.547)	(0.022)	(0.991)	(0.017)	(0.679)
CZE*Child 0-4	-0.079***	-3.127***	-0.051***	-1.819***	-0.087***	-3.401***
	(0.009)	(0.360)	(0.014)	(0.654)	(0.011)	(0.452)
Post*CZE	0.029***	1.009*	0.018	1.183	0.026**	0.627
	(0.011)	(0.517)	(0.016)	(0.967)	(0.012)	(0.575)
Post*CZE*Child 0-4	-0.072***	-1.661**	-0.060**	-1.208	-0.098***	-1.981**
	(0.016)	(0.649)	(0.025)	(1.174)	(0.020)	(0.799)
N	58,689	58,689	18,584	18,584	36,618	36,618
Adj. R-Square	0.312	0.274	0.264	0.278	0.356	0.303
Pre, CZE, Child 0-4 Mean	0.391	9.839	0.447	10.391	0.386	9.656

Notes: The table shows the triple difference-in-differences estimates for mothers. The treatment group are Czech mothers with a youngest child 0-4 y.o. The control groups are Slovak mothers with 0-4 y.o. children, and Czech and Slovak mothers with 5-9 y.o. children. The first two columns include all mothers. The estimates in the third and fourth columns are based on observations of mothers with a university education. The fifth and sixth columns show results for mothers of first-born children. We control for education, age, and quarters of the year. The estimates are based on Q1 2018 - Q4 2020 data. Significance levels (robust standard errors in parenthesis): *** 0.01, ** 0.05, * 0.1.

B1 Appendix B

To receive the CZK 80,000 increase in parental allowance, mothers (fathers) had to satisfy two eligibility conditions: having a youngest child below the age of 4 (or any age in different specifications) and and to be drawing a parental allowance on and after. The former condition is observed in data and cannot be manipulated, while the latter is not observed by us and can be - to some extent - manipulated by parents intentionally prolonging the allowance period. We next discuss how these concerns affect the interpretation of our estimated parameters.

In all our specifications, the estimated coefficient (β^{est}) of our interest is:

$$\begin{aligned}\beta^{est} = \mathbb{E}(Y|E_1 = 1, P = 1) - \mathbb{E}(Y|E_1 = 1, P = 0) \\ - (\mathbb{E}(Y|E_1 = 0, P = 1) - \mathbb{E}(Y|E_1 = 0, P = 0))\end{aligned}$$

where Y correspondence to labor force participation and hours worked, P is an indicator of post-reform observation, E_1 is the first eligibility condition corresponding to the age of the youngest child, e.g., the youngest child being younger than 4 years old. We use the age of the youngest child to define the treatment status of mothers.

There are three subgroups of mothers in the treatment group from the post-reform period: i) mothers who satisfy both eligibility conditions ($E_1 = 1$ and $E_2 = 1$) and do so without manipulation (ω); ii) mothers who satisfy both conditions ($E_1 = 1$ and $E_2 = 1$) because they prolonged the parental allowance period (ω^n); iii) mothers who do not satisfy the second condition ($E_2 = 0$) and were not treated. In the treatment group from the before-reform period, there are two types of mothers: those who satisfy both conditions ($E_1 = 1$ and $E_2 = 1$) are therefore treated, and those who do not satisfy the second condition ($E_2 = 0$) and are not treated. Under the assumption that the populations of mothers in the treatment group before and after the reform would be the same (the same distribution of length of parental allowance), should there be no reform, the share of mothers satisfying both conditions is ω and the share of mothers who do not satisfy the second condition is $1 - \omega$. Then the estimated parameters correspond to the following:

$$
\begin{aligned}
\beta^{est} &= \omega \mathbb{E}(Y|E_1 = 1, E_2 = 1, P = 1) \\
&+ \omega^n \mathbb{E}(Y|E_1 = 1, E_2^n = 1, P = 1) \\
&+ (1 - \omega - \omega^n) \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 1) \\
&- \omega \mathbb{E}(Y|E_1 = 1, E_2 = 1, P = 0) \\
&- (1 - \omega) \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 0) \\
&- \mathbb{E}(Y|E_1 = 0, P = 1) \\
&+ \mathbb{E}(Y|E_1 = 0, P = 0)
\end{aligned}
$$

We use Δ to denote the difference between after and before the reform, and show that the overall estimated effect is a sum of four differences.

$$
\begin{aligned}
\beta^{est} &= \omega \left\{ \mathbb{E}(Y|E_1 = 1, E_2 = 1, P = 1) - \mathbb{E}(Y|E_1 = 1, E_2 = 1, P = 0) \right\} \\
&+ (1 - \omega) \left\{ \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 1) - \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 0) \right\} \\
&+ \omega^n \left\{ \mathbb{E}(Y|E_1 = 1, E_2^n = 1, P = 1) - \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 1) \right\} \\
&+ \mathbb{E}(Y|E_1 = 0, P = 0) - \mathbb{E}(Y|E_1 = 0, P = 1) \\
\\
&= \omega \mathbb{E}(\Delta Y|E_1 = 1, E_2^x = 1) + (1 - \omega) \mathbb{E}(\Delta Y|E_1 = 1, E_2 = 0) \\
&+ \omega^n \left\{ \mathbb{E}(Y|E_1 = 1, E_2^n = 1, P = 1) - \mathbb{E}(Y|E_1 = 1, E_2 = 0, P = 1) \right\} \\
&- \mathbb{E}(\Delta Y|E_1 = 0)
\end{aligned}
$$

We assume that everyone who satisfies both eligibility conditions ($E_1 = 1$, $E_2 = 1$) is actually treated ($T = 1$), and anyone who does not satisfy the first eligibility condition ($E_1 = 0$) cannot be treated. We define c as $c = \mathbb{E}(\Delta Y|E_1 = 1, E_2 = 0) - \mathbb{E}(\Delta Y|E_1 = 1, E_2 = 1)$ and ATT as $\text{ATT} = \mathbb{E}(\Delta Y|T = 1) - \mathbb{E}(\Delta Y|T = 0)$.

$$
\begin{aligned}
\beta^{est} &= ATT \\
&+ \underbrace{\omega^n(\mathbb{E}(Y|E_1=1,E_2^n=1,P=1) - \mathbb{E}(Y|E_1=1,E_2=0,P=1))}_{\text{additional effect caused by manipulation}} \\
&+ \underbrace{(1-\omega)c}_{\text{misclassification}}
\end{aligned}
$$

The first term represents the effect if mothers could not adjust their allowance plan and we were able to identify who drew a parental allowance. The second term represents the boost in the treatment effect caused by mothers who postponed the termination of their parental allowance. Two properties are worthy of discussion. First, the term is likely to be negative, i.e., mothers who do not draw a parental allowance are more likely to work (be active in the labor market) than those who opted themselves into the treatment. Second, the share of this group and, thus, its importance for the overall effect, will fade over time. Finally, the third term represents the consequences of misclassification of the treatment group. Because c is likely positive, the whole term is likely positive and thus attenuates the negative treatment effect. This term will also fade over time.

We assume that the treatment is the same for every treated unit; everyone receives CZK 80,000. While most of the parents received the whole amount, a non-trivial share of parents, especially those who terminated their parental allowance shortly after January 1st, 2020. might receive only a fraction of the amount. Our assumption that every treated parent received the whole amount tends to attenuate the effect. A negative effect is less negative than it would have been if everyone received CZK 80,000.