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

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August, 2023

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 of young children in the Czech Republic. Drawing a parental allowance does not preclude labor market activity, which allows us to study the income effect. The reform led to a 6 percentage point (15%) decrease in labor market participation of mothers of young children. The effect is particularly strong among mothers with only one child (10 p.p., 28%) and among university-educated mothers (16 p.p., 36%). We observe a virtually identical reduction in hours worked. We found no effect on the labor supply of fathers of young children.

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

*We would like to thank Peter Haan, Klára Kalíšková, Nikolas Mittag, and Barbara Pertold-Gebicka for their comments. We gratefully acknowledge support by the Ministry of Labor and Social Affairs (project 09-S4-2021 VUBP), this work was also financed by the Ministry of Education, Youth and Sports of the Czech Republic in grant projects SHARE-CZ and SHARE-CZ+.

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1 Introduction

The ability of cash transfers to promptly respond 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. Recent debate in the US about the expected effects of a possible replacement of the existing child tax credit with a parental allowance 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 on parents with children under the age of 4. Before the reform, recipients of parental allowance, regardless of their prior income, were entitled to a total fixed amount of CZK 220,000.[3] The reform we study maintained the established rules, but increased the total amount of the allowance by CZK 80,000 (36%) from CZK 220,000 (ca. EUR 8,900) to 300,000 CZK (ca. EUR 12,100). 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.[4]

[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] The total amount of CZK 220,000 applies to a single child; parents of twins and triplets, for example, were entitled to a more generous amount. To draw the allowance, parents choose monthly installments, which in turn determine the length of time they receive the allowance. For example, if parents choose to receive CZK 11,000 monthly, their allowance would span 20 months.

[4] Formally, in order to maintain their eligibility for the parental allowance, the child must not be enrolled in institutional child-care for more than 92 hours per month so long as s/he is younger than 24 months. Because the limit is quite generous and the public sector lacks any means to control it, this restriction does not effectively prevent labor market activity for recipients. There is no limit on

We rely on the difference-in-differences approach and show that the extra CZK 80,000 significantly reduced maternal labor market activity, but did not alter paternal 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, at around 15%.

The average effect among mothers masks a sizeable heterogeneity. The decline in labor market supply was more pronounced among mothers with only one child. These mothers reduced their labor force participation by 10 percentage points (26%), and their weekly hours worked fell by 4 (32%). Mothers with two or more children decreased their labor force participation by 4 percentage points, and the effect is only marginally statistically significant. We argue that mothers with one child are more likely to plan (and have) another child in near future, and the extra CZK 80,000 allowed them to cover the period before the second child is born. In contrast, mothers with two or more children are, on average, less likely to plan another child and more likely to return to work.

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, which, in combination with the institutional set-up of the allowance, gave them more opportunity to prolong parental allowance than other mothers. This, in turn, led to a larger drop in maternal labor market activity among the university-educated mothers.

We do not find any evidence that the reform had an impact on paternal labor supply. Whether it is a mother or a father who draws parental allowance is an intra-household

institutional child care for children older than 24 months.To mitigate potential concerns, our preferred specification focuses on children older than 2 years.

decision, hence the increase in parental allowance could also affect fathers. However, the effects on father's labor force participation and hours worked are not statistically significant.

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, 2020, which qualified them for the extra CZK 80,000. The postponed termination of the allowance contributes to the estimated short-term effect. 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 received the allowance after the reform became effective.

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. A mother's decision to postpone her planned return to work is also consistent with observed heterogeneity: mothers with one child could postpone their planned returns as they plan another child, and university-educated mothers could postpone because they had the opportunity to prolong the 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). In contrast, Mortenson et al. (2018) and Baker et al. (2021) find

approximately zero effects of increased child benefits on the labor supply of mothers. Baker et al. (2021) highlights the effect of higher child tax benefits on lower poverty rates of children in Canada.

However, overall, the effects of unconditional child benefits and on labor supply remain understudied, and they are likely dependent on the specific institutional arrangement. The Czech system, with its variable monthly installments and flexible duration of the allowance for different recipients, diverges from the typical design of unconditional child benefits that provide a fixed monthly amount until a specified age is reached.[5] These variations in institutional setup can contribute to different effects resulting from similar policy changes.

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

[5]For instance, in Hungary, parents receive childcare allowance until their child turns 2, and they have the option to return to work when their child is 6 months old.

(OECD., 2019). Direct parental support consists of maternity and parental leave and a parental allowance. Addtional, children who turn three years old in August of a given year have priority over younger children for a place in kindergarten. However, this does not guarantee that the child will actually have a place in the preferred kindergarten.

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, and is not tied to how a parent uses his/her parental leave. Parents receiving a parental allowance retain their entitlement even if they return to work, without any restrictions on their earnings. According to the Ministry of Labour and Social Affairs (MoLSA), mothers take parental leave and draw parental allowance in 98% of cases.

Recipient's choices of the amount of monthly installments of the allowance can vary. While recipients with a higher daily assessment base (higher prior income) tend to choose shorter parental allowance, they all generally draw the parental allowance for a longer time than the minimum. For example, approximately 25% of recipients have a daily assessment base of between CZK 700 and CZK 1,000. These parents thus can choose to draw the allowance for 14 (if assessment base of CZK 1,000) to 21 (if assessment base of CZK 700) months.[6] However, the average duration of parental allowance among these recipients is around 25 months (until the child reaches 31 months). A similar trend of longer parental allowance

[6]The maximum monthly installment is calculated as 70% of 30 of the daily assessment base.

than is the necessary minimum is observed for other income groups. Recipient's choice of a longer parental allowance suggests that they do not face immediate credit constraints.

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[7,8]. 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.[9]

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. In some cases, parents with children approaching the age of 4 needed to increase their monthly payments and to expedite the withdrawal of funds after January 1st, 2020, in order to receive the full amount of the increased allowance.

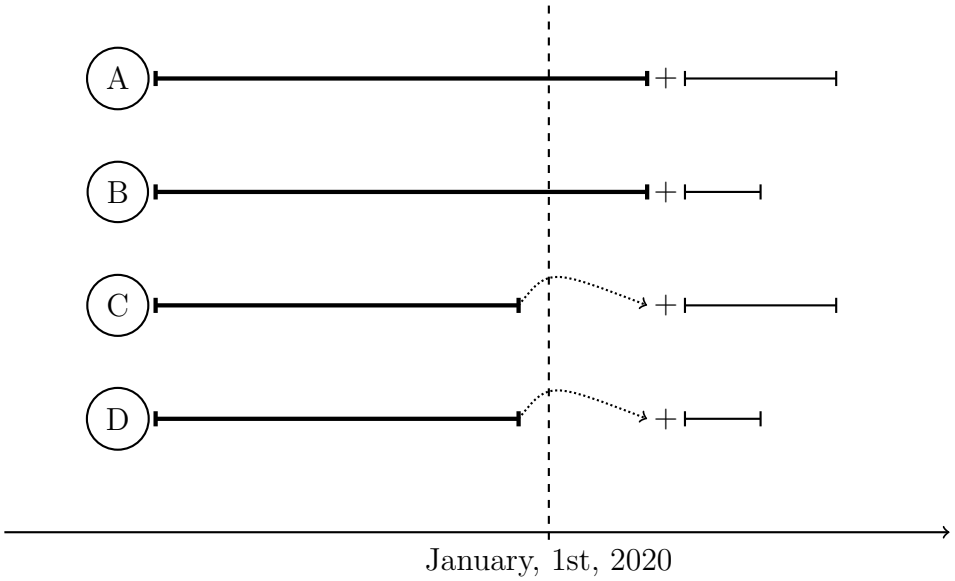
[7]https://www.mpsv.cz/rodicovsky-prispevek

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

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

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.

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.

2.2 Data

We use the Labor Force Survey (LFS) data for the Czech Republic from 2017-2020.[10,11] 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

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

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

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.[12] 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 were primary targets of the increase in parental allowance (98% of parental allowance recipients in 2020 were mothers: see Section 6 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.

[12]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.

3 Parental Allowance

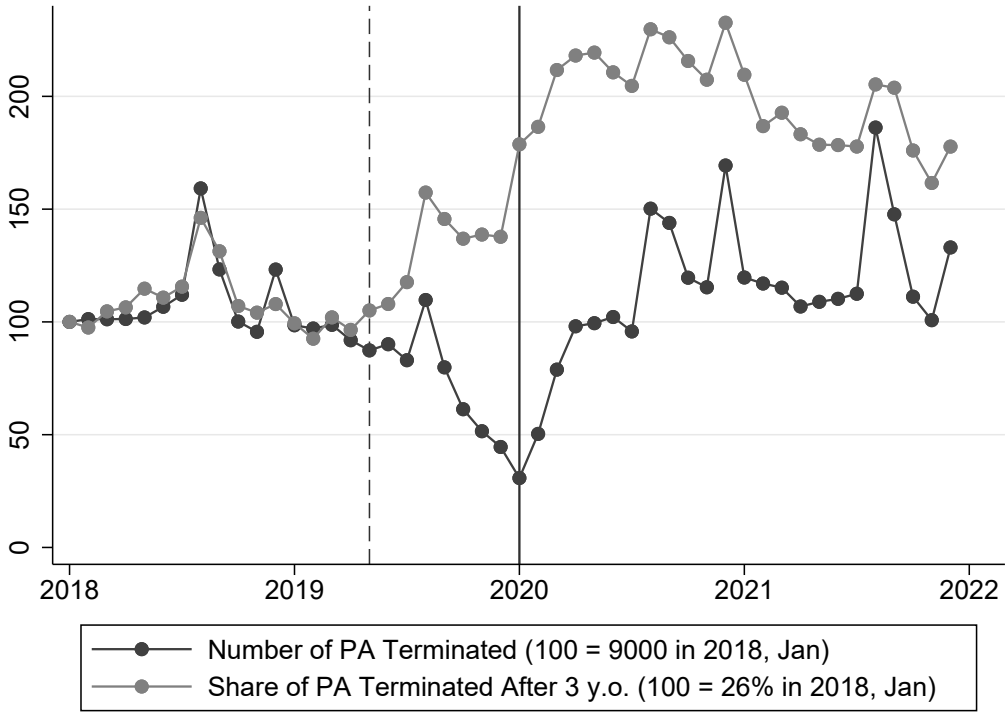
We document three patterns in the duration and the average amount of monthly payments of parental allowances related to the reform, using administrative data aggregated by months provided by the MoLSA. First, the total number of terminated allowances decreased rapidly just before the reform, which we 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 A1c which shows an increase in the average age of the children when parental allowance is terminated, from around 32.6 months before the reform to 36.3 months after the reform. Additionally, Figure A1a 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 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

Figure 2: **Parental Allowance Termination**



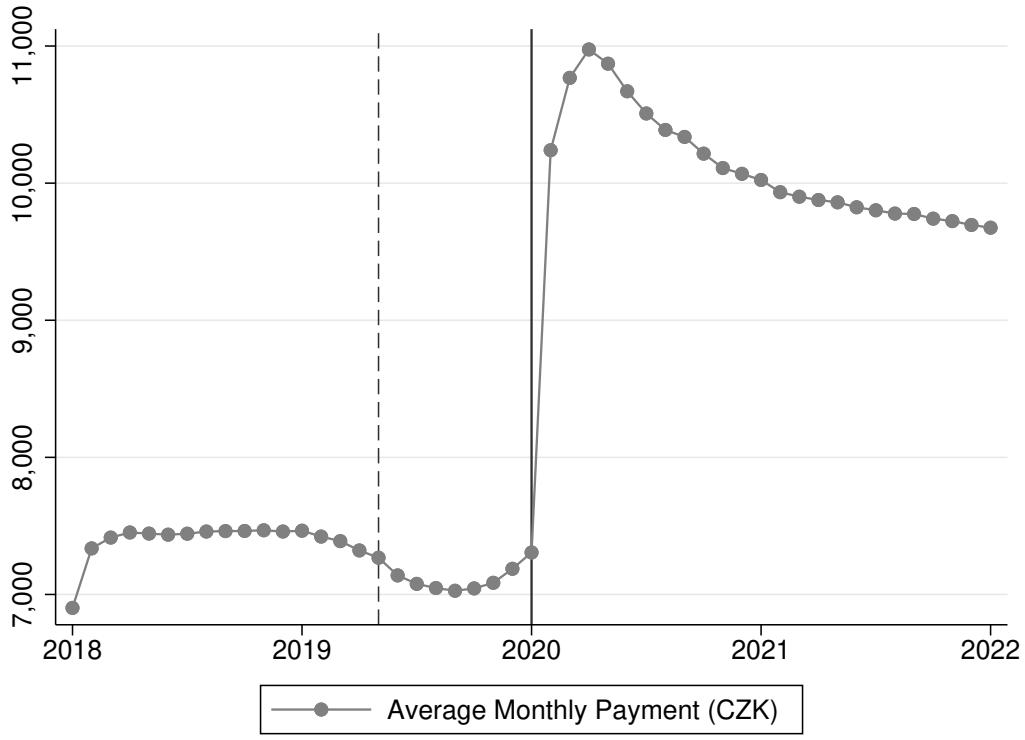
Notes: This figure shows the relative frequency of parental allowances termination and share of termination after a child reached the age of 3 over time. Both measures are normalized to 100 in January 2018. The dashed vertical line corresponds to May 2019, when the reform was approved by the government, 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%.

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.[13]

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

[13]The number of changes at the beginning of 2020 was exceptionally high, several times higher than usual. For instance, in 2021, one year after the reform, only 26,000 recipients changed their amounts within the first three months of the year.

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.

type of question with 5 options.[14] 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.

[14] 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 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.[15] 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.

5 Effect on Maternal Labor Supply

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 dots represent maternal labor force participation rates in the three years before (2017 - 2019) and one year after the allowance increase. A visual inspection reveals a systematic downward shift among mothers with children aged 2 (6.6 p.p.) and 3 (10.2 p.p.) years old.

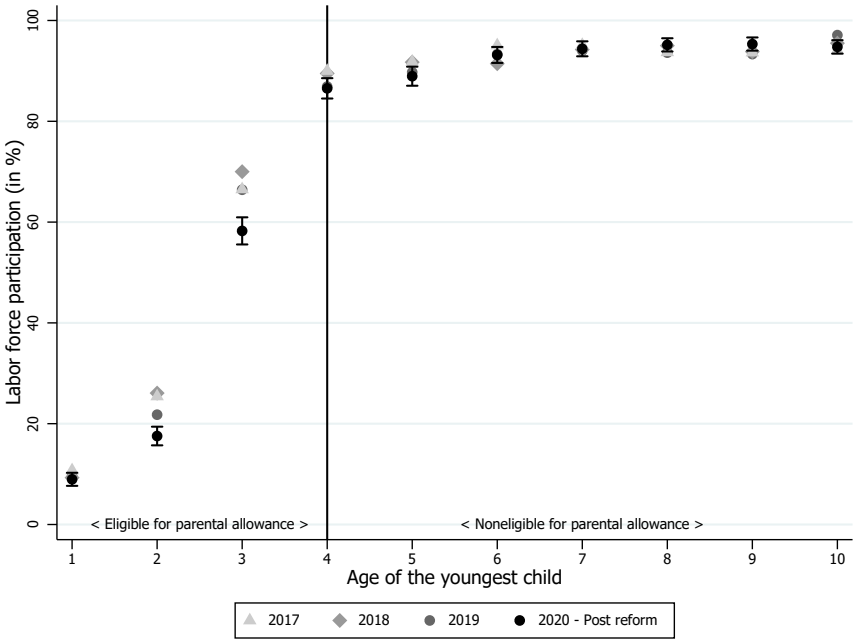
There appears to be minimal or no effect on mothers with children younger than 2 years old, which aligns with the interpretation that the decline in labor force participation is driven by their choice to use the additional funding to postpone their planned return to work. Because most mothers with children younger than 2 do not have immediate plans to reenter the workforce, the extra funding does not have an immediate impact on their decision-making. If they choose to postpone their planned return to work by several months, the effects are likely to manifest at a later stage, when the child is older.

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 mothers did 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.

[15] Results are presented in Appendix B.

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

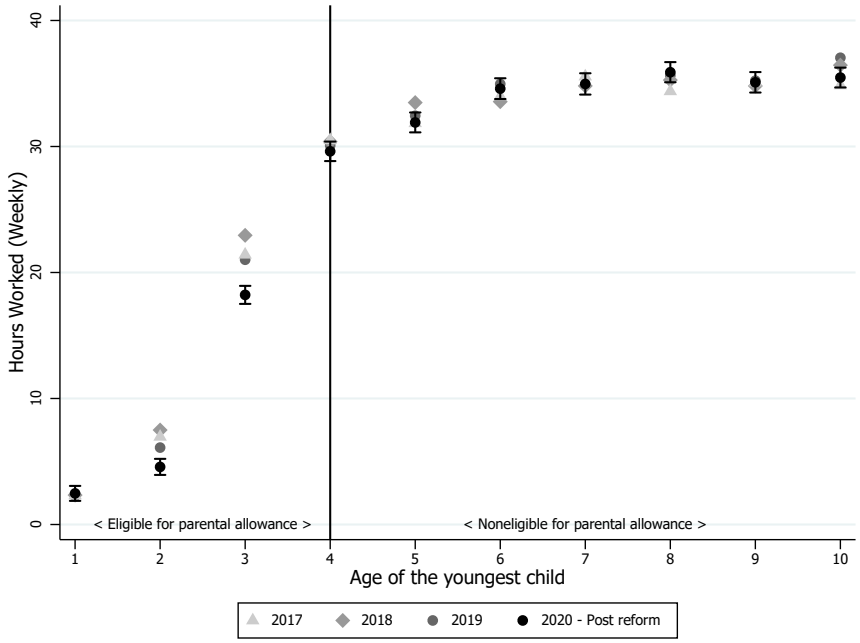


Notes: The figure shows shares of mothers (women 18-64 y.o. with at least one child of ten or younger) who actively participate in the labor market. To exclude the anticipatory effect of the reform, the figure disregards the last quarter of 2019. The grey vertical line shows the child's age, one of the 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 6.3 percentage points (14%). 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 4.9 p.p. (16%) and hours worked by 1.6 per week (17%). In the third specification, both treatment and control groups are narrower than in the baseline. Mothers whose youngest child is between 3.00 and 3.99 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. 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.

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 8.5 p.p. (12%) 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 12% to 16% and the effect on hours worked from 15% to 17%.

We next estimate the timing of the effect. We estimate the following regression using our preferred specification of the treatment (mothers whose child is between the ages of 2.00-3.99) and control (mothers whose child is between the ages of 4.00-5.99) groups

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

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

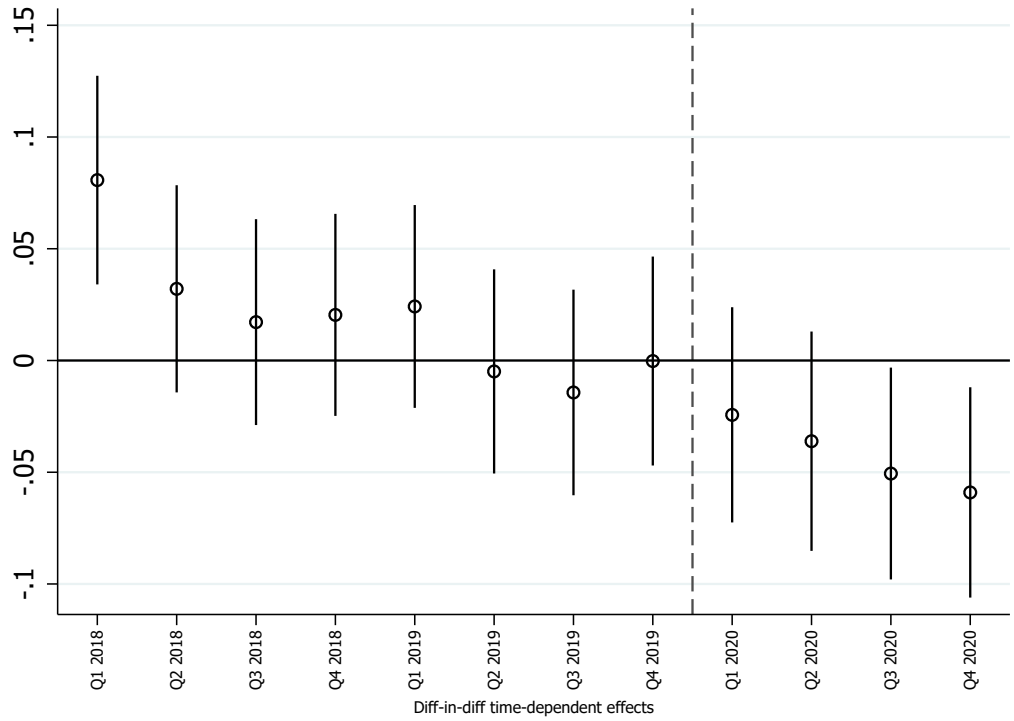
	Treated: 2-3 y.o.		Treated: 1-3 y.o.		Treated: 3 y.o.	
	LFP	HW	LFP	HW	LFP	HW
Post	-0.019**	-0.764**	-0.009	-0.269	-0.020*	-0.561
	(0.008)	(0.389)	(0.006)	(0.309)	(0.012)	(0.565)
Treated	-0.438***	-17.260***	-0.557***	-21.569***	-0.195***	-7.881***
	(0.008)	(0.334)	(0.007)	(0.266)	(0.011)	(0.483)
Post*Treated	-0.063***	-2.209***	-0.049***	-1.633***	-0.085***	-3.360***
	(0.014)	(0.551)	(0.010)	(0.411)	(0.020)	(0.851)
N	14,774	14,774	22,817	22,817	7,007	7,007
Adj. R-Square	0.29	0.30	0.40	0.41	0.15	0.16
Pre, Treated Mean	0.44	13.84	0.31	9.53	0.69	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. The significance levels of estimated parameters do not change when we cluster at the household level. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.

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 in 2017, which is a baseline year; 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 a quarter k and the average in 2017; and β_3^k represents the difference-in-differences estimates in a quarter k.

Figure 6 plots β_3^k coefficients from regression 1 with labor force participation as the outcome variable. Maternal labor force participation decreases gradually over time. Though in the first quarter the effect is small and statistically insignificant, the point estimates become more negative for each subsequent quarter in 2020. We argue that this slow onset of the effect is due to the increasing number of mothers who would otherwise have returned to the labor market, but did not. For several quarters after the reform, the longer after the reform, the more mothers have not returned to the labor market, and the overall aggregate fall in labour market participation rates enlarges over the period.

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

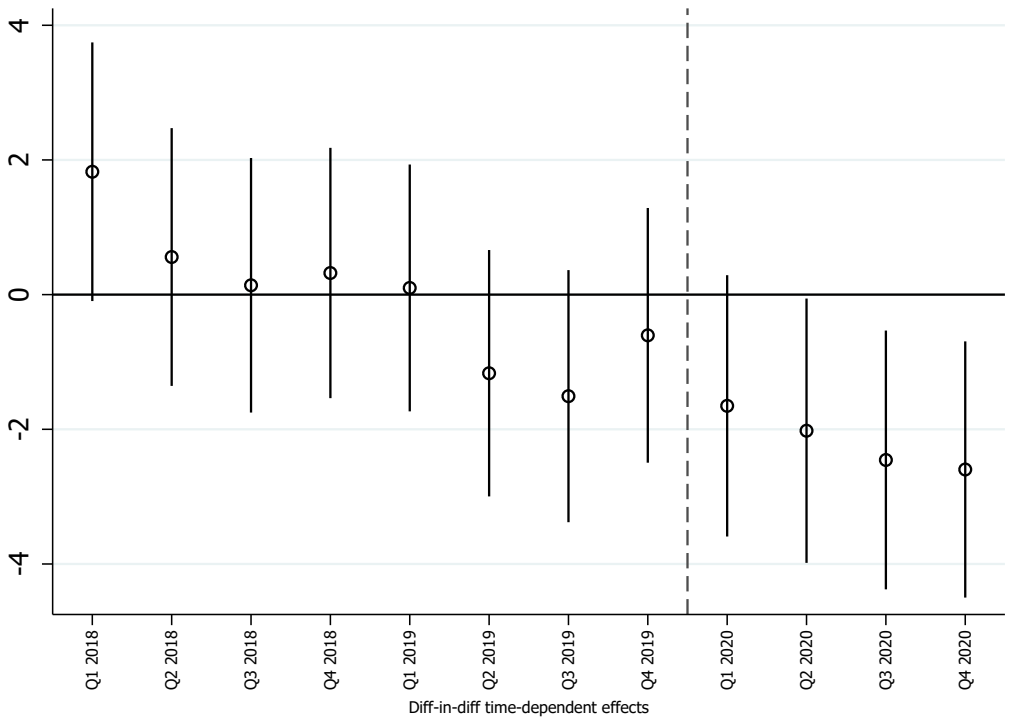


Notes: The dependent variable is labor force participation. The sample consists of women 18-64 y.o. with children 2-5 y.o. The graph shows the interaction coefficients from the difference-in-differences regression equation where the interaction of the treatment (mothers with 2-3 y.o.) and the "post" period is carried out separately for each quarter between 2018 and 2020. The baseline corresponds to the average over four quarters in 2017. 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.

Figure 6 also shows that the conditional difference in maternal labor force participation rate between the treatment and control groups in Q1 2018, two years before the reform, was positive and statistically significant. To demonstrate that our overall effect is not driven by this period, we re-estimate our main specification from Table 1 using a specific dummy variable which controls for maternal labor force participation in Q1 2018 in the treatment group. Table A2 in Appendix A reports the results. The effect remains economically and statistically significant.

Figure 7 shows that the decline in hours worked is around 2 hours per week in all four quarters after the reform. Similarly to labor force participation, the effect becomes stronger and the point estimates become more negative gradually over time.

Figure 7: **Hours Worked**
Diff-in-diff Estimates with Quarterly Interactions, Mothers



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 difference-in-differences regression equation in which the interaction of the treatment (mothers with 2-3 y.o. child) and the "post" period is carried out separately for each quarter between 2018 and 2020. The baseline corresponds to the average over four quarters in 2017. 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.

5.1 Effect on Mothers by Number of Children

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. Because we cannot identify which mothers planned another child, we use the observation that mothers with one child are more likely to plan (and have) another (second) child than mothers with two or more children are to plan another child. We therefore split the sample into mothers with only one child and mothers with two or more children, and study the effect of the reform on each group separately. 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 i.e., among mothers with only one child.

Table 2: **Labor Force Participation and Hours Worked**
By Number of Children, Mothers, Diff-in-diff Estimates

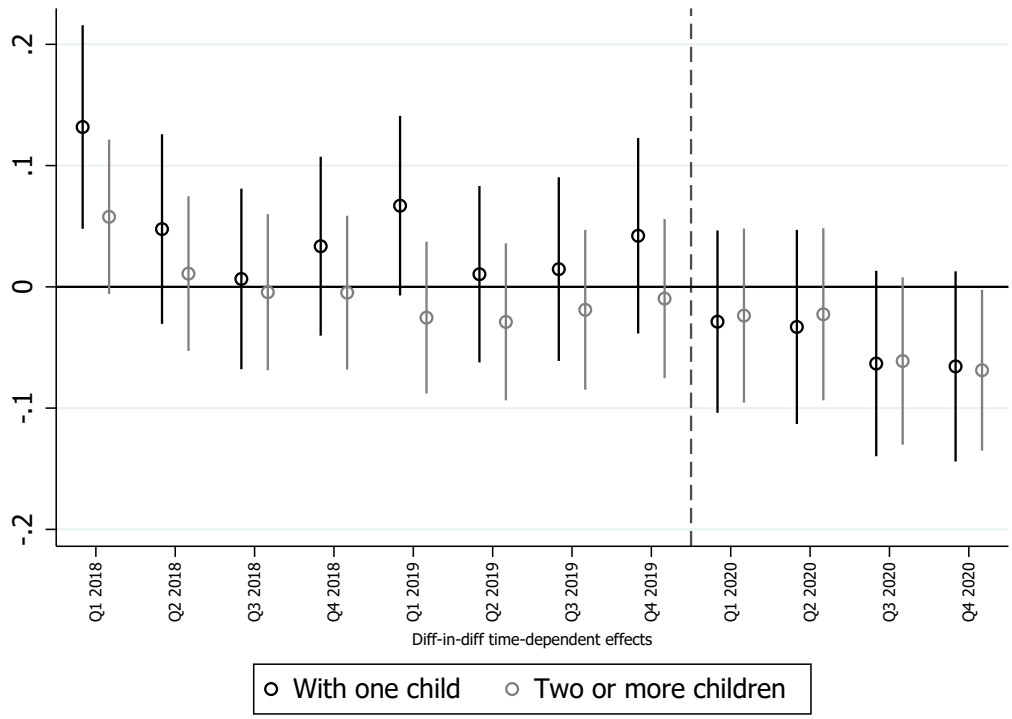
	With One Child		Two or More Children	
	LFP	HW	LFP	HW
Post	-0.021	0.228	-0.015	-1.111**
	(0.016)	(0.701)	(0.012)	(0.542)
Treated	-0.462***	-18.185***	-0.434***	-16.742***
	(0.014)	(0.560)	(0.012)	(0.468)
Post*Treated	-0.091***	-4.009***	-0.040**	-1.187
	(0.022)	(0.899)	(0.020)	(0.783)
N	5,641	5,641	7,340	7,340
Adj. R-Square	0.32	0.32	0.28	0.29
Pre, Treated Mean	0.41	12.46	0.46	14.18

Notes: The table shows the difference-in-differences estimates. In the left panel, the treatment group consists of mothers with only one 2-3 y.o. child, and the control group of mothers with one 4-5 y.o. child. In the right panel, the treatment group consists of mothers with one 2-3 y.o. child and at least one older child, while the control group consists of mothers with one 4-5 y.o. child and at least one older child. The regression equations are estimated separately for each panel. 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.

First, we re-estimate our preferred difference-in-differences specification using a sample of mothers who have only one child. The treatment group consists of mothers whose child is between the ages of 2.00-3.99, while the control group consists of mothers with only one child between the ages of 4.00 and 5.99. The left panel of Table 2 shows that the reform resulted in a significant decrease in the labor market participation rate among mothers with one child, with a decline of 9.1 percentage points (22%). There is also a notable reduction in the number of hours worked. Mothers with one child reduced their working hours by an average of 4 per week (32%), double the effect estimated on a full sample of all mothers. This difference between the decrease in labor force participation and hours worked suggests an increase in part-time employment contracts among mothers with one child following the reform.

Second, we re-estimate the same specification on a sample of mothers with at least two children. In this case, the treatment group consists of mothers with two or more children, where the youngest child is between the ages of 2.00 and 3.99, while the second youngest

Figure 8: **Labor Force Participation**
By Number of Children, Diff-in-diff Estimates with Quarterly Interactions



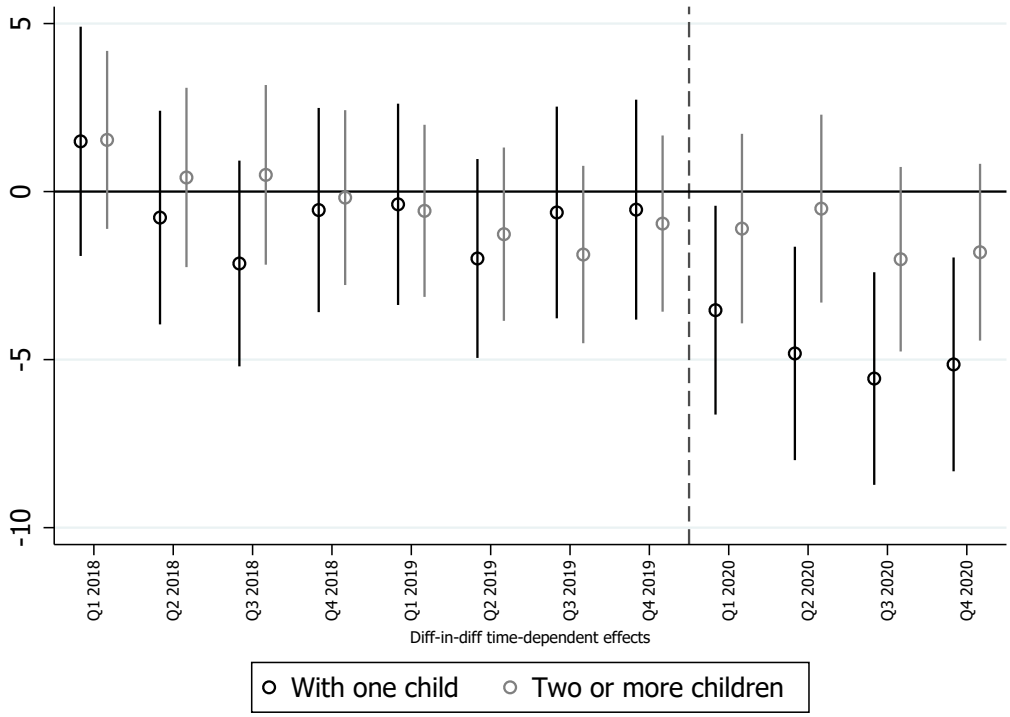
Notes: The dependent variable is labor force participation. The sample consists of women 18-64 y.o. with children 2.00-5.99 y.o. The graph shows the interaction coefficients from the diff-in-differences regression where the interaction of the treatment (mothers with children 2.00-3.99) and the "post" period is carried out separately for each quarter between 2018 and 2020. The baseline corresponds to the average over four quarters in 2017. 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.

child was 10 or younger when the youngest child was born. The control group is defined similarly, except that the youngest child is between the ages of 4.00 and 5.99. The right panel of Table 2 displays results for mothers with two or more children. Their labor force participation decreased by 4 percentage points (9%), as did their hours worked, by an average of 1.2 per week (8%). While the effect on labor force participation is marginally significant, the decrease in hours worked is statistically insignificant. These results suggest that the overall effect is largely driven by mothers with one child, who likely used the additional financial support to cover the period between their first and second child.

We further estimate the triple difference and test the difference between the effects among mothers with one child and mothers with two or more children. The results confirm that the reduction in labor force participation among mothers with one child is about 5.3

percentage points larger than among mothers with more children. Similarly, the effect on hours worked is 3 hours per week larger; for full results, see Table A3 in Appendix A.

Figure 9: **Hours Worked**
By Number of Children, 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 baseline corresponds to the average over four quarters in 2017. 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.

Figure 8 plots β_3^k coefficients from regression 1 using labor force participation as the outcome variable estimated on samples of mothers with one child (black) and mothers of two or more children (grey). A visual inspection reveals that the average level of labor force participation among mothers with one child fell more than among mothers with two or more children, compared to their pre-reform levels.

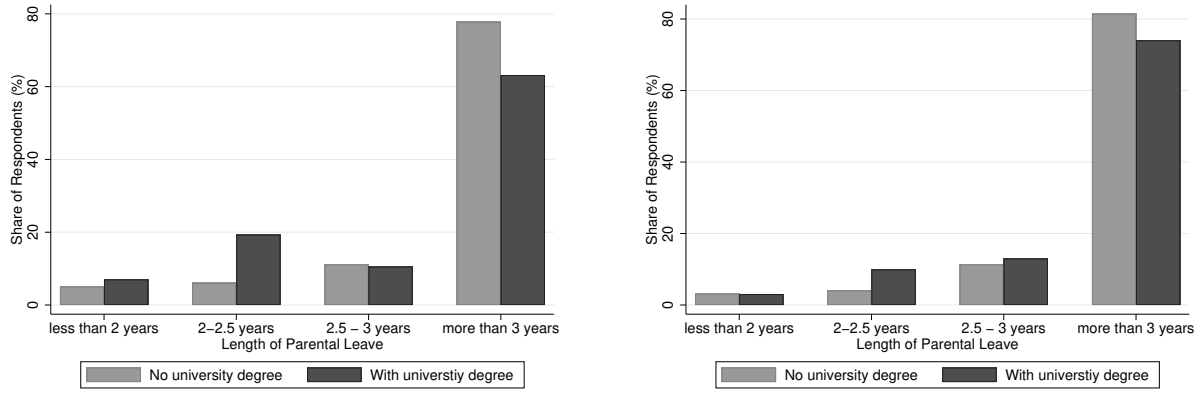
Figure 9 plots the β_3^k coefficients from a regression with hours worked as the outcome variable. The figure confirms the cumulative effects from Table 2: the reform resulted in a reduction of 5 hours worked per week among mothers with one child. The effect occurs immediately after the reform and remains stable over the next four quarters. In contrast, there is no effect on mothers who had an older child already.

5.2 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 obliged 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 10 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 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.

Figure 10: **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.

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 hours worked is evident only among university-educated mothers. The effect is substantial. Their labor force participation fell by 16 percentage points (31 %) 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.

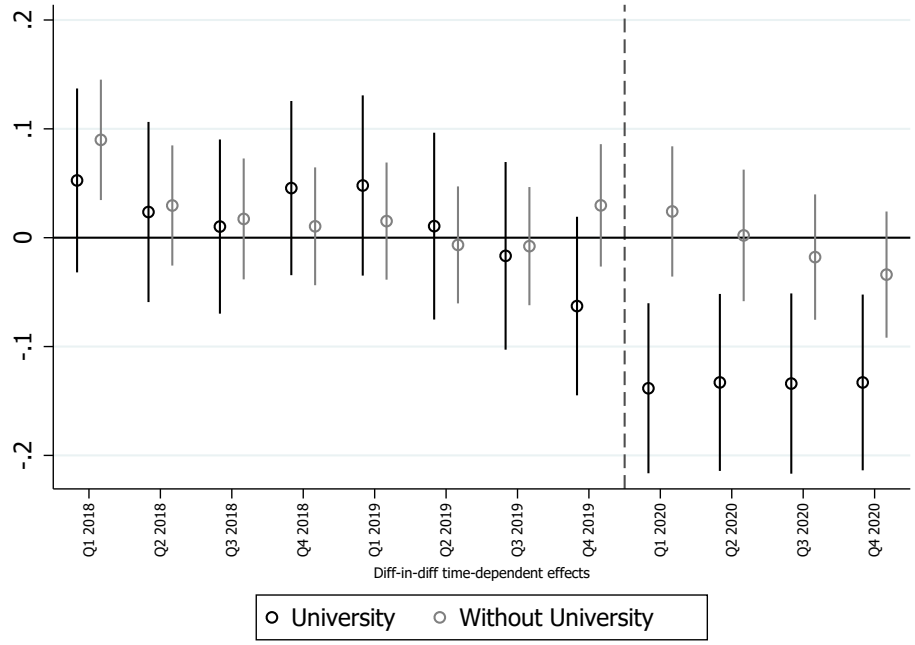
Table 3: **Labor Force Participation and Hours Worked**
By Education, Mothers, Diff-in-diff Estimates

	Primary		Secondary		Secondary with GE		University	
	LFP	HW	LFP	HW	LFP	HW	LFP	HW
Post	-0.041	-2.383	-0.033	-0.619	-0.025**	-0.998*	0.024*	-0.141
	(0.049)	(1.872)	(0.020)	(0.900)	(0.012)	(0.597)	(0.013)	(0.594)
Treated	-0.355***	-11.423***	-0.485***	-18.150***	-0.458***	-18.280***	-0.386***	-16.841***
	(0.034)	(1.206)	(0.018)	(0.745)	(0.013)	(0.512)	(0.015)	(0.597)
Post*Treated	-0.007	1.481	-0.034	-1.251	-0.023	-1.080	-0.160***	-4.768***
	(0.063)	(2.370)	(0.031)	(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	0.30	0.30	0.33	0.31	0.30	0.31	0.31	0.34
Pre, Treated Mean	0.28	8.27	0.38	11.30	0.44	14.14	0.52	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 A4 in Appendix A confirm that treatment heterogeneity is statistically significant. In particular, labor force participation among university-educated mothers declined by 12 percentage points more than among the less educated, and hours worked fell by almost 3 hours more.

Figure 11: **Labor Force Participation**
By Education Groups, Diff-in-diff Estimates with Quarterly Interactions



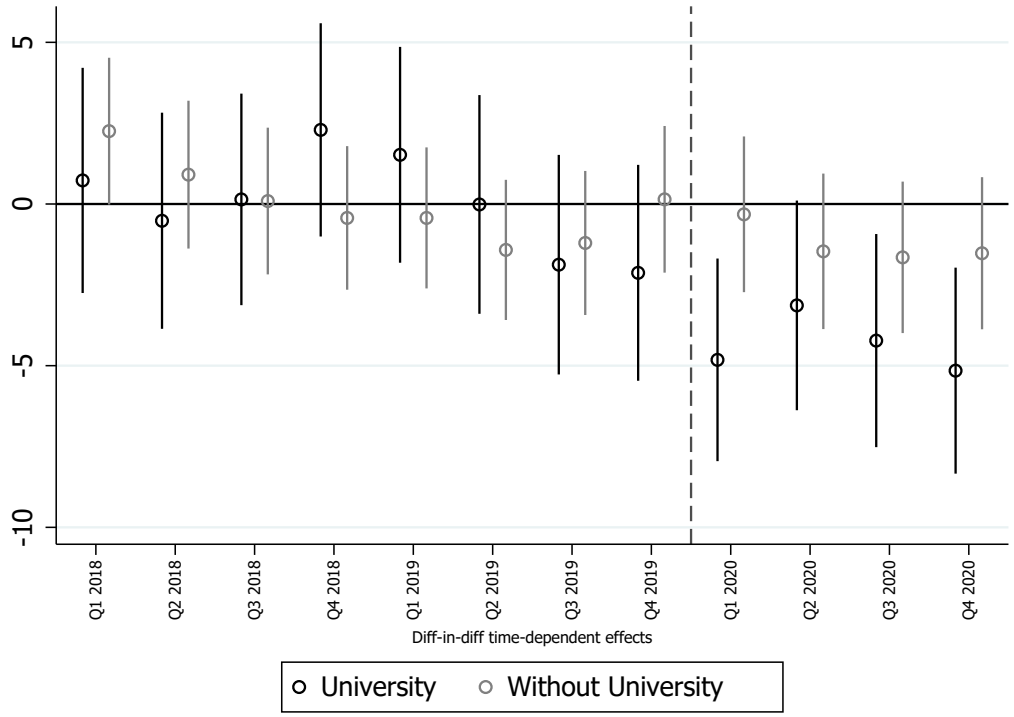
Notes: The dependent variable is labor force participation. The sample consists of women 18-64 y.o. with children 2-5 y.o. The graph shows the interaction coefficients from the difference-in-differences regression equation in which the interaction of the treatment (mothers with 2-3 y.o. child) and the "post" period is carried out separately for each quarter between 2018 and 2020. The baseline corresponds to the average over four quarters in 2017. 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 11 plots coefficients β_3^k from regression 1 on a sample of university-educated mothers and mothers without a university education, respectively. 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 12 replicates the exercise with hours worked as the

[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.

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

Figure 12: **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 in which the interaction of the treatment (mothers with 2-3 y.o. child) and the "post" period is carried out separately for each quarter between 2018 and 2020. The baseline corresponds to the average over four quarters in 2017. 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.

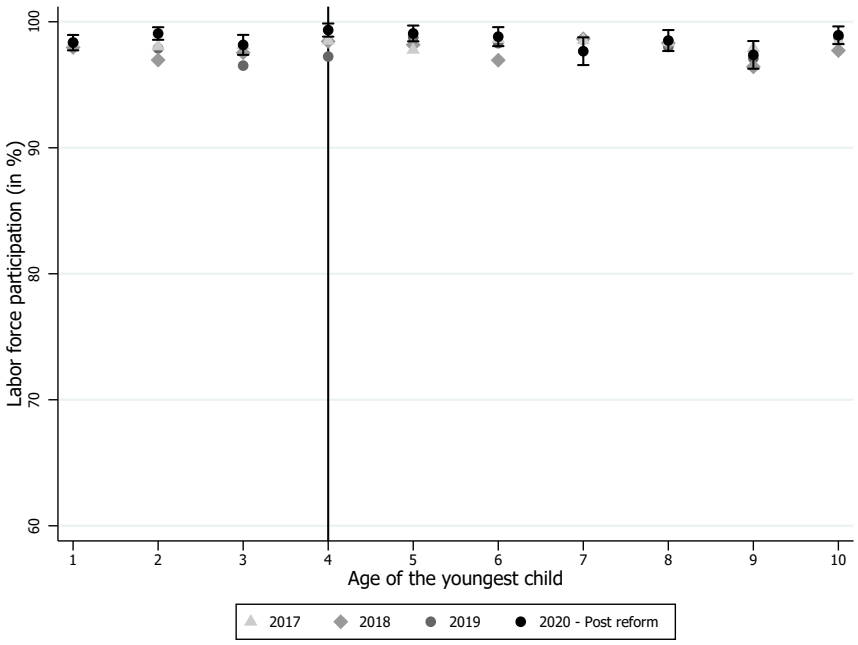
Our survey data further support the observed heterogeneity. University-educated mothers in our survey sample postponed their return to work by 9.3 months, whereas less-educated mothers by 7.1 months (p-value of t-test 0.051). The results are based on 76 respondents who indicated that they or their partners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

6 Effect on Paternal Labor Supply

In principle, fathers are eligible to be recipients of the parental allowance, too. The rules even allow parents to switch roles in drawing in the course of drawing a parental

allowance.[17] Therefore, we examine whether the reform had an impact on the labor supply of fathers. We start by showing that the labor supply of fathers and mothers with young children differ substantially. Figure 13 shows that, regardless of the child's age, nearly all fathers are active on the labor market. The labor force participation rate remains close to 100% for all categories of child age, without a discernible pattern of increasing labor force participation as the child grows older. Figure 13 further indicates that the reform did not change the paternal labor supply.

Figure 13: **Labor Force Participation of Fathers**



Notes: The figure shows shares of fathers (men 18-64 y.o. with at least one child of ten y.o. or younger) who actively participate in the labor market. 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. Fathers on the right of the vertical line are ineligible, while fathers on the left meet the child age eligibility criterion. 95 % confidence intervals are plotted.

We replicate the same empirical exercises conducted on the samples of mothers, including the heterogeneity analyses. We limit the presented results to the preferred specification, in which the treatment group consists of fathers with children between the ages of 2.00 and 3.99, while in the control group there are fathers with children between the ages of 4.00 and 5.99. Furthermore, from the heterogeneity analysis, we report the effects on a sample of university-educated fathers and on a sample of fathers with one child, i.e., samples on which we identified the largest effects among mothers.

[17]But at any given time, no more than one parent can draw parental allowance.

Table 4: **Labor Force Participation and Hours Worked**
Fathers, Diff-in-diff Estimates

	All Fathers		University Education		One 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 left panel corresponds to a sample with all fathers. The middle panel includes only fathers with a university education. The right panel shows results for fathers with only one child. 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.

We do not find any evidence that the reform had an impact on the paternal labor supply. Table 4 displays three panels of results, each corresponding to our preferred specification (treatment group with child between the ages 2.00 and 3.99) estimated on samples of all fathers, fathers with a university education, and fathers with one child, respectively. In the left panel, we present results estimated on a sample of all fathers with children in the relevant age category. The estimated effects on the paternal labor force participation rate and hours worked are small and statistically insignificant. Similarly, the middle panel, which presents results for a sample of university-educated fathers, also indicates no significant effects for either outcome measure. Finally, the right panel presents results estimated on a sample of fathers with one child. Even in this subgroup, the point estimates of the effects on both outcome measures are not statistically different from zero. Overall, fathers did not adjust their labor force participation or hours worked in response to the reform. This null effect holds true even among demographic groups that exhibited significant effects among mothers.

7 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).[18] We test for the effect on the fertility rate using a difference-

[18]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.

8 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 with only one child also reduced their labor supply more than mothers with two or 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

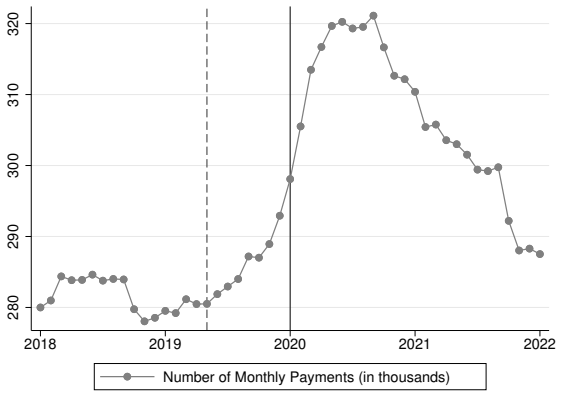
Table A1: **Descriptive Statistics**
Mothers, Treatment and Control Groups, Q1 2019 & 2020

	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

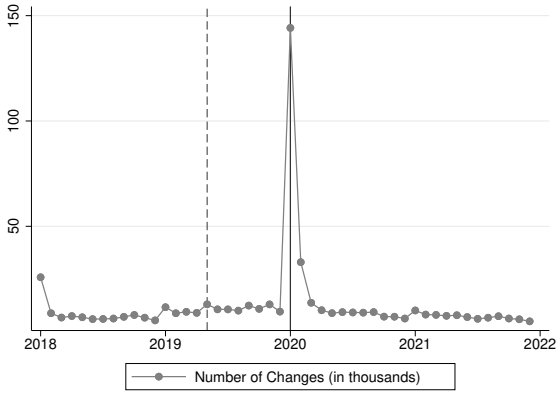
Notes: The table shows descriptive statistics for mothers with children 2-3 y.o. (treatment group) and 4-5 y.o. (control group) for Q1 2019 (before the reform) and Q1 2020 (after the reform).

A1.1 Figures

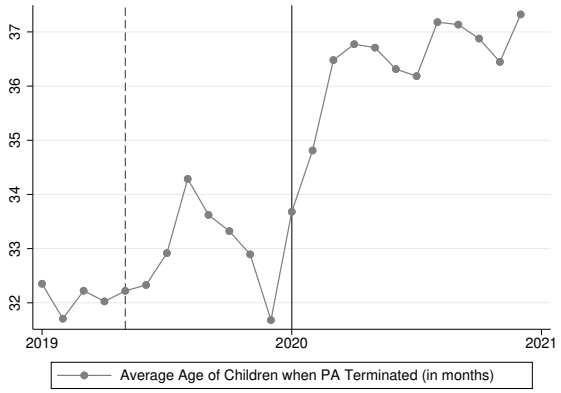
Figure A1: **Parental Allowance**



(a) Number of Monthly Payments



(b) Number of Changes

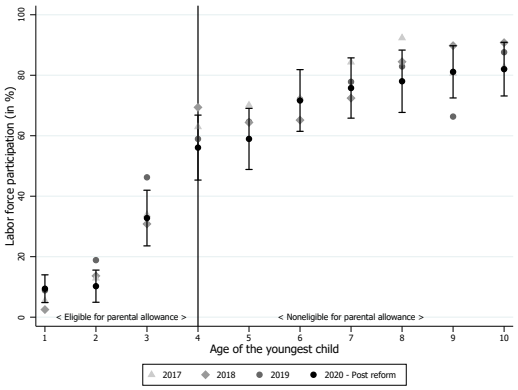


(c) Age of Children when PA Terminated

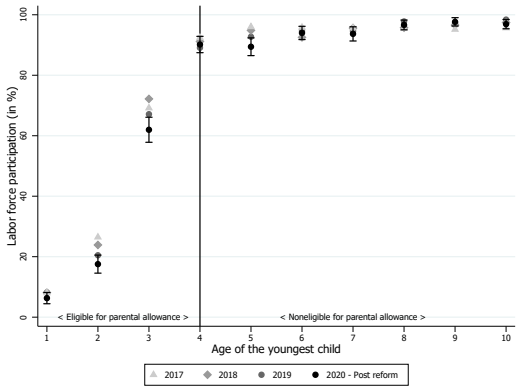
Notes: The upper left panel (a) shows the number of parental allowance payments in a given month. As recipients tend to postpone the termination of their allowances, the number of parents drawing the parental allowance increased by 40,000 after the reform. The upper right panel (b) shows the number of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

Figure A2: **Education Heterogeneity**

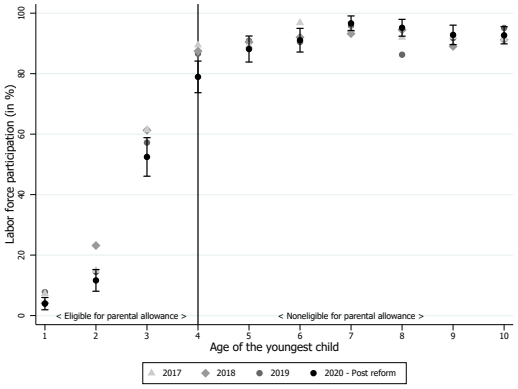
(a) **LFP of Mothers**
Primary Education



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



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



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

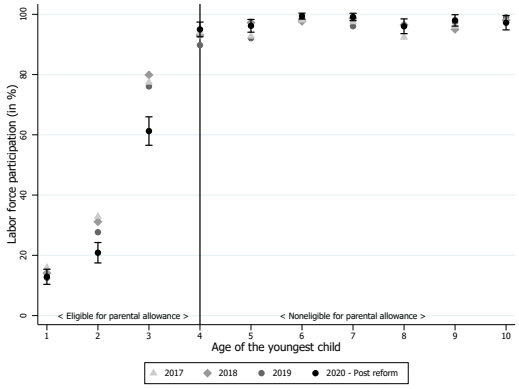
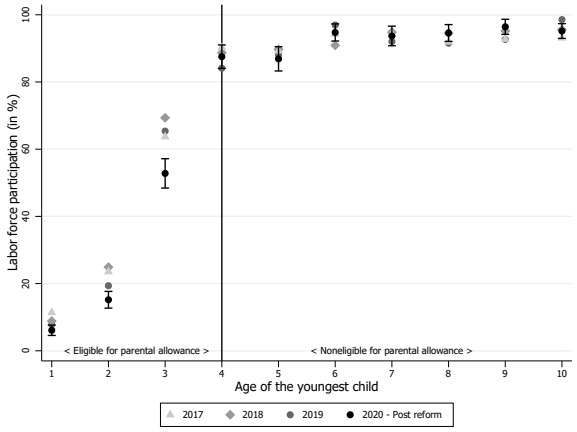


Figure A3: **Number of Children Heterogeneity**

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



(b) **LFP of Mothers**
With Two or More Children

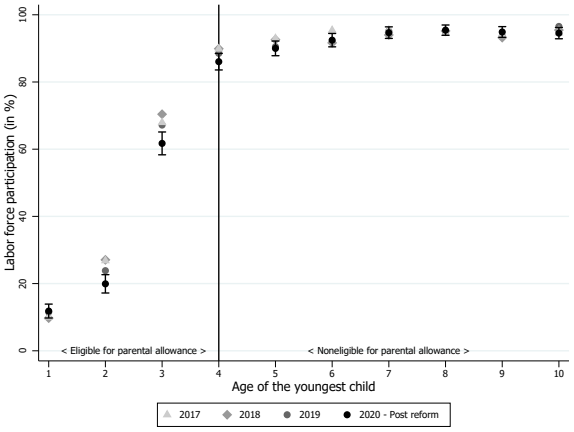
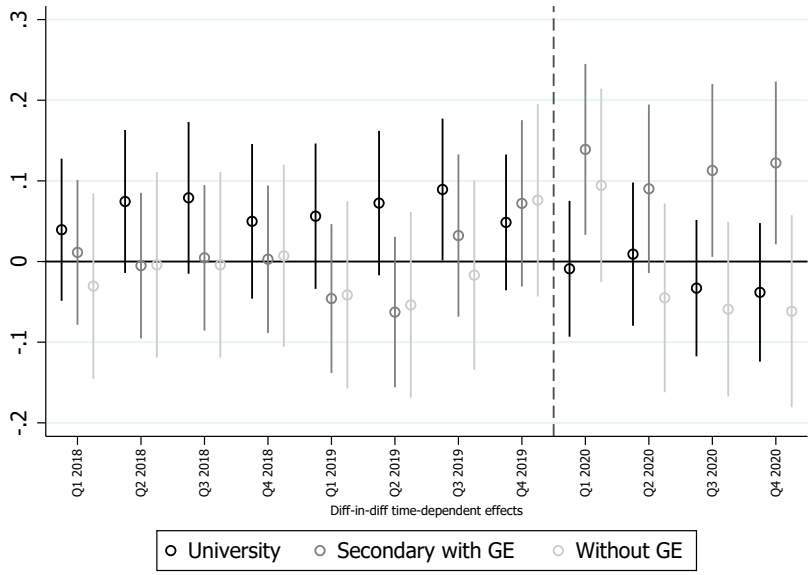


Figure A4: **Fertility**
By Education, Diff-in-diff Estimates with Quarterly Interactions



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 which the interaction of the treatment (woman has a child younger than 1 y.o. child) 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**
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	-0.023***	-0.880**	-0.013*	-0.412	-0.027**	-0.730
	(0.009)	(0.399)	(0.007)	(0.318)	(0.012)	(0.578)
Treated	-0.448***	-17.488***	-0.564***	-21.759***	-0.205***	-8.100***
	(0.009)	(0.359)	(0.007)	(0.286)	(0.012)	(0.520)
Post*Treated	-0.054***	-1.946***	-0.042***	-1.425***	-0.075***	-3.140***
	(0.014)	(0.566)	(0.010)	(0.422)	(0.021)	(0.872)
N	13,392	13,392	20,677	20,677	6,343	6,343
Adj. R-Square	0.30	0.30	0.41	0.41	0.16	0.16
Pre, Treated Mean	0.44	13.60	0.31	9.38	0.69	22.46

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. We include an indicator variable that is equal to one for treated observations in 2018 Q1 to show that our baseline results presented in Table 1 are not driven by the spike in Q1 2018, visible in Figure 6. Significance levels (robust standard errors in parentheses): *** 0.01, ** 0.05, * 0.1.[19]

Table A3: **Labor Force Participation and Hours Worked**
Mothers, Number of Children, Triple Diff-in-diff Estimates

	LFP	HW
One Child	-0.010	0.732
	(0.019)	(0.795)
Post	-0.009	-0.720
	(0.012)	(0.545)
Post*One Child	-0.014	0.957
	(0.019)	(0.884)
Treated	-0.428***	-16.384***
	(0.011)	(0.465)
Treated*One Child	-0.026	-1.269*
	(0.017)	(0.711)
Post*Treated	-0.040**	-1.345*
	(0.020)	(0.781)
Post*Treated*One Child	-0.053*	-2.984**
	(0.029)	(1.188)
N	13,127	13,127
Adj. R-Square	0.28	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 with two or more children. 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 A4: **Labor Force Participation and Hours Worked**
Mothers, Interactions with University Education, Triple Diff-in-diff Estimates

	LFP	HW
Educ: University	0.026***	2.769***
	(0.010)	(0.457)
Post	-0.029***	-0.748
	(0.011)	(0.497)
Post*Educ: University	0.042***	0.263
	(0.016)	(0.757)
Treated	-0.443***	-16.718***
	(0.010)	(0.401)
Treated*Educ: University	0.044**	-0.694
	(0.017)	(0.696)
Post*Treated	-0.030*	-1.425**
	(0.017)	(0.680)
Post*Treated*Educ: University	-0.118***	-2.894**
	(0.028)	(1.134)
N	14,928	14,928
Adj. R-Square	0.28	0.28
Pre, Treated Mean	0.44	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 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.[20] Lacking detailed information on the ages of children in the data, we use a sample of mothers with childen between the ages of 0 and 4. 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.[21,22]

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[23] 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,

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

[21]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`)

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

[23]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 6.

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 directly comparable in magnitude to results presented in Section 6 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 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.