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Mikkel Aagaard Houmark
Cecilie Marie Løchte Jørgensen
Ida Lykke Kristiansen
Miriam Gensowski

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CEBI
Department of Economics
University of Copenhagen
www.cebi.ku.dk

Effects of Extending Paid Parental Leave on Children's Socio-Emotional Skills and Well-Being in Adolescence *

Mikkel Aagaard Houmark† Cecilie Marie Løchte Jørgensen‡
Ida Lykke Kristiansen§ Miriam Gensowski¶

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Abstract

We study how children's socio-emotional skills and well-being in adolescence are affected by an increase in the duration of parental care during infancy. Exploiting a Danish reform that extended paid parental leave in 2002 and effectively delayed children's entry into formal out-of-home care, we show that longer leave increases adolescent well-being, conscientiousness and emotional stability, and reduces school absenteeism. The effects are strongest for children of mothers who would have taken short leave in absence of the reform. This highlights how time spent with a parent is particularly productive during very early childhood.

Keywords: Parental Leave, Early Childhood, Skill Formation, Parental Investments, Socio-Emotional Skills, Personality, Well-Being, Adolescence.

JEL classification: J13, J18, J24, I31

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†Department of Economics and Business Economics and TrygFonden's Centre for Child Research, Aarhus University. Email: mhoumark@econ.au.dk. Corresponding author.

‡Department of Economics and Business Economics, Aarhus University. clj@econ.au.dk.

§Department of Economics and CEBI, University of Copenhagen. ilk@econ.ku.dk

¶Rockwool Foundation Research Unit, and IZA, Bonn. mig@rff.dk

1 Introduction

While humans begin their existence as utterly dependent on their parents, over time, other caregivers enter their lives. Children's development is shaped by interactions with their parents and the environment outside the home. Early psychological studies and attachment theory suggest that separating a young child from their primary caregiver can impact their development (Belsky 1988; Bowlby 1973; Ainsworth et al. 2015); something that is echoed in the concept of *sensitive periods* in the literature on skill formation (Cunha and Heckman 2008). Children's well-being in the long run may depend on both *who* invests (depending on substitutability between caregivers or environments, see Blau and Currie 2006; Datta Gupta and Simonsen 2010), and on *when* these different investments are made.

Most children in Western societies will enter institutional care at some point before elementary school, with a timing that often coincides with the parents' return to work. Parents' choice of how long to stay at home with their child depends highly on the policy context – notably on parental leave provisions. A growing literature studies the effect of introducing or extending paid parental leave on children's academic performance and cognitive outcomes. Much less is known about how the time that the child is exclusively cared for by a parent influences children's *socio-emotional skills*. These skills are major determinants of a happy, healthy, and wealthy life (Almlund et al. 2011; Bleidorn et al. 2021).

This paper studies the causal effect of longer time cared for exclusively by parents on children's socio-emotional skills and well-being in adolescence. We exploit the implementation of a reform in Denmark which increased paid parental leave from 24 to 46 weeks. While the additional leave could be shared freely between mothers and fathers, only mothers reacted to the reform (Beuchert et al. 2016), increasing their average leave from 8.3 to 9.7 months (and the median from 7.5 to 10.7 months). Since informal care is practically never used in the Danish context (OECD 2021), the leave extension corresponds to children entering formal care later. The implementation of the reform coincides with a discontinuity in school-starting age. We therefore use a differences-in-discontinuities approach to estimate the causal effect of mother's length of leave on socio-emotional skills and well-being while also accounting for potential effects of differences in school-starting age on these outcomes.

Using The Danish Well-Being Survey (DWS), a nearly universal survey of the well-being of elementary school children, we construct measures of child socio-emotional skills following Andersen et al. (2020). Sufficiently many of the questions from the DWS resemble items from questionnaires based on the five-factor model of personality, making it possible to construct measures of conscientiousness, agreeableness, and emotional stability, as well as children's general well-being. The three traits are considered markers of a healthy and mature personality (Bleidorn et al. 2020; B. W. Roberts 2008).

We find that children of mothers who were induced to take longer leave have better outcomes along several dimensions. An additional month of leave increases children's self-reported well-being in adolescence by 4.7 percent of a standard deviation, conscientiousness by 3.5 percent, and emotional stability by 2.8 percent. For children of the median mother, who increased their leave by 3.2 months, this would correspond to increases of well-being by 15.04 percent of a standard deviation, 11.2 percent for conscientiousness and 9.0 for emotional stability.

We develop a simple model of skill formation to highlight *which* children should benefit most from extended parental leave. Consistent with the model's predictions, we show that the effects of additional leave are largest for children of mothers who would have taken relative short leave in absence of the reform. For these children, who would thus otherwise have entered daycare at a very young age, one additional month of leave increases their adolescent well-being, conscientiousness and emotional stability by 6.0, 6.5, and 5.6 percent, respectively. This heterogeneity is consistent with sensitive periods of childhood where parental investments are more beneficial than institutional care at early ages. Because the children of mothers who tend to take short leave also tend to have lower levels of all our outcomes of interest, this heterogeneity also implies that an overall increase in parental leave reduces inequality in child socio-emotional skills and well-being. [1]

[1] We emphasize that our theoretical framework is based on a single parent and is hence agnostic about whether the parent making the investments is a mother or father. Our findings are based on mothers rather than fathers for empirical reasons, but nothing in our framework suggests ex ante that the effect would be different if fathers took some or all of the additional leave. We also do not claim that it is costless for the parent to invest

While higher well-being is in itself desirable, and conscientiousness and emotional stability are important determinants of general life outcomes, they are measured with self-reported data. We supplement our results by considering a set of complementary objective outcomes; academic achievement and school absenteeism. Academic achievement is produced with a combination of cognitive and socio-emotional skills. Hence, one might expect leave duration to also raise academic achievement. While we find no significant effect on grades at the end of elementary school *on average*, we again find significant positive effects for the children of mothers who were most likely to take short leave in absence of the reform. We also consider school absenteeism, another objective measure which might be more closely related to socio-emotional skills and well-being than academic achievement. Indeed, we find that longer leave also reduces absenteeism, especially among children of mothers who are likely to take short leave. Together, these findings show that leave duration can affect a range of educationally relevant child outcomes.

Our results help to fill a gap in our understanding of how longer parental leave, as opposed to earlier entry into daycare, affects long-run human capital formation. Existing studies either look only at the short term effects, do not have a clear counterfactual mode of care, or have a narrow focus on cognitive skills. Most research does not find children's *cognitive skills* to be affected by parental leave extensions (Rossin-Slater 2017; Rasmussen 2010; Liu and Skans 2010; Dustmann and Schönberg 2012; Dahl et al. 2016; Danzer and Lavy 2018).[2] There is, however, some evidence that the effect varies across socioeconomic groups, with the pattern generally being that longer leave is beneficial for children of high-SES mothers (Liu and Skans 2010; Danzer and Lavy 2018), in some cases detrimental for children from low-SES families (Canaan 2022), but also that this is highly dependent on the counterfactual mode of care (Danzer et al. 2020). Only a few papers study the effect of extended leave on children's *socio-emotional skills*. Baker and Milligan (2015) and Huebener et al. (2019) find no average effect on personality traits or socio-emotional behaviors measured at ages 4-6, but Heisig and

in the child's skill formation. On the contrary, our model highlights the trade-off between consumption and investments that is inherent to the leave allocation decision.

[2]Introducing paid parental leave has, however, been shown to improve average child outcome such as health at the time of birth, high-school drop-out rates, and earnings at age 30 (Stearns 2015; Carneiro et al. 2015).

Zierow (2019) find that longer leave does increase children's life satisfaction measured at age 18 to 36.

A closely related literature exploits variation in the use of formal daycare. These papers study widely different programs of different quality. Age of exposure ranges from 0-6 years old and they are typically in settings where the counterfactual mode of care is a mix between parental care and informal daycare arrangements. Hence, they are not necessarily directly comparable to the literature on extending parental leave. The majority of papers study the short-term effect of daycare on socio-emotional skills.[3] While the short term effect may be relevant in itself, we are generally interested in whether there are permanent effects on child development, and so it is important to consider the longer run as well. Among the few papers analyzing longer-run outcomes, Fort et al. (2020) find that *earlier* daycare entry *reduces* openness, agreeableness and emotional stability at ages 8-14, though only among the high-earners in their sample, which already comes from a relatively affluent population. In addition, Baker et al. (2019) and Haeck et al. (2018) find negative effects on life satisfaction and anxiety measured at ages 12-20, Bach et al. (2019) and Berlinski et al. (2009) find positive effects on extraversion and class concentration at ages 8-15, and Kuehnle and Oberfichtner (2020) find insignificant effects on personality measures measured at age 15. However, the counterfactual in these papers is often a combination of parental care and other informal care arrangements, or the quality of the daycares are of a relatively low quality.

Our paper stands out from the previous literature through a combination of factors. Specifically, (1) we use a large administrative data set covering the entire population of births (2) to study the effect at a specific and relevant age window of children (around 6-10 months) (3) in a setting with a clear counterfactual mode of care (OECD 2021), (4) and with daycares of

[3]Of the papers that study the short-term effect (measured before age eight) of daycare on socio-emotional skills, eight papers find negative effects (Baker et al. 2019; Haeck et al. 2018; Kottelenberg and Lehrer 2014; Herbst 2013; Herbst and Tekin 2010; Baker et al. 2008; Loeb et al. 2007; Magnuson et al. 2007), while five papers find no significant effects (Kuehnle and Oberfichtner 2020; Felfe and Lalive 2018; Chor et al. 2016; Bernal and Keane 2011; Datta Gupta and Simonsen 2010), and one paper finds short-term positive effects (Yamaguchi et al. 2018). Of the eight papers that find short-term negative effects, three papers study the same roll-out of universal daycare in Quebec. The universal daycare in Quebec has received low-quality assessments, especially at its inception (see e.g. van Huizen and Plantenga 2018). The four other studies are in the context of the US. Datta Gupta and Simonsen (2012) find no average effect on SDQ or criminal behavior at age 11 by type of out-of-home care.

comparatively high quality. As far as we know, we are the first to show that longer parental care, as opposed to formal daycare, has long term positive effects on socio-emotional skills and well-being. This finding is particularly salient because most other countries have shorter parental leave standards and lower-quality formal daycares. The Danish setting could hence be considered one where we expect *small*, if any, effects of extending leave and delaying entry into institutional care. Introducing or extending paid parental leave in settings with no or shorter paid leave, or where daycares are of lower quality, could potentially be even more beneficial.[4] As we see no permanent effect on household income from the change in leave induced by the reform, and Beuchert et al. (2016) find no effects on child health, and only very limited effects on maternal health[5], we argue that our findings are most likely caused by increasing parental time investments in the early period of life.

We contribute to knowledge about the origins of personality traits, on which there exists very limited causal evidence. Only recently have personality psychologists begun to systematically examine sources of changes in personality, being previously more concerned with demonstrating relative stability (Bleidorn et al. 2021).[6] We join a very short list of papers that exploit natural experiments to examine effects of exogenous shocks on personality. Nearly all examine how collective events (natural disasters, health events) shape adult personality (Damian et al. 2021; Schwaba et al. 2021; Sutin et al. 2020), though Akee et al. (2018) also show how an income transfer to households can benefit children's emotional health and personality traits.

The remainder of the paper is organized as follows: In Section 2, we describe the institutional setting and the parental leave reform used for identification. In Section 3, we develop

[4]It is however also possible that there is some complementarity between parental inputs and the quality of daycares, which would work in the opposite direction.

[5]While Beuchert et al. (2016) use the same reform as we do, they focus on health outcomes up to five years after the child's birth, not up to adolescence. Further, they use hospital-based diagnoses, which can be considered an indication of relatively severe health problems, in contrast to the continuous-type long-run outcomes we study which may be more sensitive. Their findings on maternal health are in line with the literature finding negligible benefits of leave beyond 6 months (see Canaan et al. (2022) and Rossin-Slater (2017) for a review).

[6]While our analysis examines the origins in an indirect way, without identifying specific parental behaviors or parenting styles (Ayoub et al. 2021), our set-up exploits exogenous variation in the age at which maternal care is substituted with formal daycare, that is otherwise highly correlated with other parental characteristics and behaviors.

a theoretical model used for predicting and interpreting our results, and in Section 4, we present our empirical strategy. In Section 5, we present our data, show descriptive statistics and predict leave taking based on observable characteristics. We then present our results in Section 6, and finally, we discuss and conclude the paper in Section 7.

2 Institutional setting and the parental leave reform

To study the effect of parental leave duration on child outcomes, we exploit a major Danish parental leave reform implemented in 2002. Discussions about changing the parental leave legislation began during general election campaigns in November 2001 and the reform was passed on March 20, 2002.[7]

There were two large changes to the leave system: 1) The reform extended the number of weeks of leave with full leave benefits from 24 to 46 weeks. The additional leave could be shared freely between the parents.[8] The full benefit level corresponds to the unemployment benefit rate paid by the government, leading to a replacement rate that is comparable to levels in other high-income countries with family-friendly policies (Olivetti and Petrongolo 2017).[9] 2) Before the reform, parents could choose to take up to 52 weeks at a reduced benefit rate (60%) after the 24 weeks at the full benefits. This was abolished, but parents were now given the right to extend their leave for up to 14 additional weeks without payment. The pre- and post-reform benefit rates are illustrated in Fig. 1, with a more detailed breakdown in Appendix Fig. A.2.

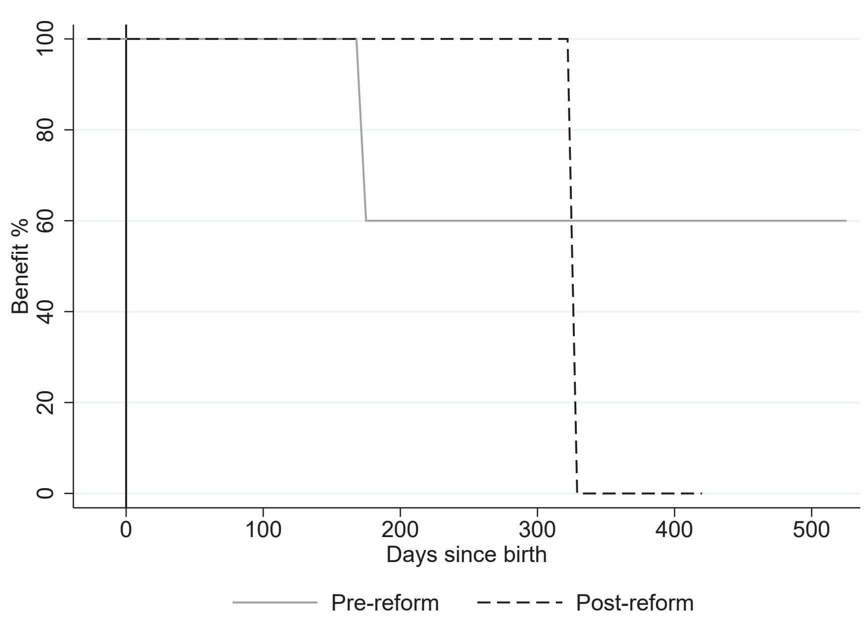
The new scheme applied to all births after March 27, 2002, while mothers of children born between January 1, 2002 and March 26, 2002 could choose between the old and the

[7]Denmark offered its first form of maternity leave in 1901. In 1960, mothers were granted the right to 14 weeks of leave with wage compensation at the level of sickness leave benefits. Parental leave was gradually extended over time as the welfare state expanded, with major changes taking place in the 1980s and 1990s.

[8]Before the reform, 14 weeks of leave were earmarked to the mother, 4 weeks were earmarked to the father, and an additional 10 weeks could be shared between the parents. After the reform, parents had 32 weeks to share at full benefits, in addition to the mother's 14 weeks. The leave earmarked to fathers was reduced to two weeks. See Appendix Fig. A.2 for an overview of the pre- and post-reform rules.

[9]Many employees have contracts that raise the effective replacement rate to 100% for some part of the leave period (for example, 24 weeks total for mothers in the public sector), where the difference between the leave benefit and the full wage rate is covered by the employer.

FIGURE 1: Benefit compensation



Notes: Overview of benefit compensation of the pre- and post-reform policies in Denmark, 2001-2002.

new scheme. The majority chose the new scheme.[10] Manipulation of child birth to benefit from the new scheme is unlikely because of the timing of the reform passing and its implementation retroactively, and the eligibility of mothers giving birth before the reform was passed. The reform increased the average duration of leave taken by mothers by 1.4 months (see Section 5.3). Fathers did not respond significantly to the reform, on average increasing their leave by only a quarter of a day (Beuchert et al. 2016). For this reason, our focus is on leave taken by mothers.

When their parents return to the labor market, Danish children almost always enter formal daycare. Denmark also features one of the most gender-equal labor markets in terms of participation rates (International Labour Organization 2017; Cascio et al. 2015). Hence, while parents are allowed a relatively long period of paid leave, they almost always both return to the labor market afterwards. This is aided by subsidized universal full-day daycare availability (Brewer et al. 2022). Most children are guaranteed a daycare spot in their munic-

[10] Only 12% of mothers giving birth in January or February 2002 took some leave at the reduced benefit rate (which was only possible under the old scheme).

ipality by law, and informal care (e.g., by grandparents) or home care is rarely used.[11] While the fraction of 0 to 2-year-olds in informal care varies between 20 and 40 percent for most OECD countries, the corresponding number for Denmark is less than 1 percent, the lowest in all of the OECD (OECD 2021). There are two types of daycare: center-based daycare (nurseries, around 37 percent of the 1-year olds in out-of-home care in 2002), and family daycare (63 percent, see Statistics Denmark 2022b, 2022c).[12] The average child-to-adult ratio at the time of the reform was similar across the two types of care, namely 3.5:1 in family daycares and 3.1:1 in nurseries in 2003 (Arbejderbevægelsens Erhvervsråd 2007). Thus, both child care options are age-appropriate, with favorable child-to-adult ratios compared to most other OECD countries (OECD 2020).

Children are expected to start in the first grade of elementary school in the calendar year where they turn seven.[13] The school starting rules create a discontinuity in the age distribution which persists throughout primary school. For any particular grade level, children born in December are on average the youngest in their class, and children born in January are on average the oldest.[14] This discontinuity presents a difficulty, as it coincides with the implementation of the parental leave reform. Section 4 elaborates on our strategy and how we deal with the discontinuity in school starting age.

[11] 238 out of 271 municipalities had a guaranteed a daycare spot arrangement in 2002 (Statistics Denmark 2002)

[12] There are quality differences between the two types of care (Datta Gupta and Simonsen 2010), as center-based daycares employ both university-educated pedagogues and uneducated staff, whereas no formal training is required for child-minders in their own homes for family daycare.

[13] One year earlier, most children would enter primary school by starting in *kindergarden class* (grade 0). This has since been made mandatory. Enrollment may be postponed one year if either the parents or the municipality recommend it (Danish Ministry of Children and Education 2019), which is more common for children born in October-December (see, for example, Landersø et al. 2017). Starting one year early is also possible, but rare.

[14] This discontinuity is present in many countries and has been used to identify the effect of school starting age on children's educational achievement or mental health (Bedard and Dhuey 2006; Black et al. 2011). The Danish context has been used to study the effects of school starting age on crime (Landersø et al. 2017), mental health (Dee and Sievertsen 2018), as well as outcomes for the family as a whole (Landersø et al. 2020).

3 Theoretical framework

We now develop a stylized model to show how parents trade off own consumption with investing in the child's skill formation via parental leave in the child's first years of life. The model features the most salient characteristics of the institutional framework just described, and speaks to which children would benefit most from delaying entry into childcare.

Consider a representative family with a parent and an infant. The child is either taken care of by the parent or is in daycare. The parent cannot work while providing full-time care and is on parental leave during this period. When the parent works, the child has to attend daycare. The model focuses on a single period of childhood, yet allows for the standard features of life-cycle skill formation, as explained below. The parent chooses how much of the period he or she will spend on parental leave versus working. We assume that leave is taken first, right after birth, and work follows as an absorbing state. Therefore, choosing the share of time on parental leave is *equivalent to determining the age at which a child starts daycare.*

The parent derives utility from own consumption c and future child skills θ^c, with $u = v(c) + z(\theta^c)$, and $v', z' > 0$ and $v'', z'' < 0$. The parent allocates a time endowment of 1 between parental leave, x, and work, $(1-x)$. Work earns a skill-specific wage that is higher for high-skilled parents than for low-skilled parents: $w(\theta^p_H) > w(\theta^p_L)$. While on leave, all parents receive a uniform payment $(LeavePay)$ which is lower than the low-skilled wage $(LeavePay < w(\theta^p_L))$. Before the reform, the amount of $LeavePay$ decreased to 60% after 24 weeks of parental leave. The reform extended the payment length, so that parents could receive the same amount of $LeavePay$ in weeks 25-46 as in week 24. The reform thus increased $LeavePay$ for parents at the 24-week margin (where many parents previously ended their leave).[15]

[15] As discussed in Section 2 the Danish public sector disburses a uniform amount to all parents on parental leave for the number of weeks that are set aside as paid parental leave. Individual employers can top up the public amount to increase the replacement rate, but will typically only cover a limited number of weeks. This will introduce heterogeneous earnings during parental leave despite the uniform payment to all parents, at least during the first weeks. It is nevertheless reasonable for our purposes to model $LeavePay$ as uniform, because the reform we exploit takes place at a different margin (shifting average leave-taking from about 36 to 42 weeks) where all parents receive the uniform (lower) payment. The amount of leave compensation decreases to zero after 46 weeks post-reform as well. But since very few parents are above this margin, we disregard it here.

Child skills θ^c are produced at home and in daycare and develop according to

$$\theta^c = f^\theta(\theta_i^p, x, Z_i). \tag{1}$$

Parent skills, θ^p, influence child skills in Eq. (1) in two ways. First, there is the direct effect that more highly skilled parents tend to have more highly skilled children already before the decision on how to allocate time x, because of heritability and differential in-utero investments. Second, parents' skills may also interact with x by influencing the productivity of the time spent with the child. Other characteristics of the environment, Z_i, such as siblings, may also enter the skill production process. Both time with the parent and time in daycare can be productive in themselves (have positive effects on θ^c), but x incorporates the inherent trade-off that time with the full-time parent at home means time *not* going to daycare. Therefore, $\partial f^\theta / \partial x$ represents the *combined* effect of increasing time with the parent on leave and reducing the time spent in daycare by delaying entry. This effect of a child spending time with the full-time parent or in institutional care is age-dependent and possibly non-monotonous. In the first days of a child's life, one would expect that an additional day at home is more beneficial for the child's skills than being in formal daycare (so that extending leave has a positive effect on skills, $f'_x > 0$). At some point, however, it is possible that additional time at home becomes less productive than the alternative, and thus delaying daycare entry further would reduce child skills (f'_x turns negative). We study this trade-off, and the possibly varying effect by age, in this paper.

Parents choose at which age the child enters daycare (equivalently, the share of the period that parents are on leave), and their return to work, x, to maximize their utility subject to their budget constraint without borrowing and the skill production technology:

$$\begin{aligned} \max_{c,\,x} \quad & u = v(c) + z(\theta^c) \\ \text{s.t.} \quad & c = w(\theta_i^p)(1-x) + x \cdot \text{LeavePay}, \\ & \theta^c = f^\theta(\theta_i^p, x, Z_i). \end{aligned}$$

This results in the following optimality condition:

$$\frac{\partial v(c)}{\partial c}(w(\theta^p) - LeavePay) = \frac{\partial z(\theta^c)}{\partial \theta^c}\frac{\partial f^\theta(\theta_i^p, x, Z_i)}{\partial x}. \tag{2}$$

The left-hand side (LHS) is the marginal utility from working, determined by the income premium from working, $(w(\theta_i^p) - LeavePay)$, and the marginal utility of consumption, $(\frac{\partial v(c)}{\partial c})$. Because the market wage is higher than $LeavePay$ for all groups, there will always be a consumption gain from shortening the leave duration and going to work instead (LHS is positive). The right-hand side (RHS) is the marginal benefit to child skills from postponing daycare entry. The effect stems from the marginal utility for the parent of having a more highly skilled child, which is always positive $\left(\frac{\partial z(\theta^c)}{\partial \theta^c} > 0\right)$, and the marginal effect of delaying age at daycare entry on the child's skills $\left(\frac{\partial f^\theta(\cdot)}{\partial x}\right)$, which, as explained, depends on the parent's child-rearing skills relative to daycare, as well as on the age of the child at which they enter daycare, x.

The optimality condition does not predict unequivocally which parent type takes shorter or longer leave, but clarifies the important channels.[16] High-skilled parents are more likely to take shorter leave than low-skilled parents if i) there are large wage returns to skills, ii) the curvature of the utility of consumption is such that the marginal utility is not falling too rapidly with additional consumption, iii) there is strong heritability of skills, and iv) the curvature of $z()$ is such that highly skilled parents have a clearly lower marginal utility from

[16] The left-hand side emphasizes that for high-skilled parents, since they earn higher wages, going to work instead of being on leave increases their consumption more than for less skilled parents. The marginal gain from further consumption increases, however, is likely lower for highly skilled parents than that of less skilled parents, since they have higher wages (unless they compensate higher wages with significantly lower working time to reduce overall income). Given that the wage effect is first-order (one-for-one increase from higher parent skills) whereas the reduction of marginal utility is a second-order phenomenon (at very high consumption, the second derivative of utility with respect to consumption will likely be near constant), it is likely that the left-hand side of Eq. (2) implies shorter leave-taking for highly skilled parents. The right-hand side of Eq. (2) furthermore indicates that there is less pressure on high-skilled parents to delay their child's start in daycare, at least from the first component $\partial z(\theta^c)/\partial \theta^c$: If children of high-skilled parents have higher initial skills via heritability (all else equal), the marginal benefit of increasing child skills further would be smallest for high-skilled parents. The second component, however, is increasing in parent skills—the marginal effect of a highly skilled parent spending time with the child at home is greater than for less skilled parents. Furthermore, to the extent that highly skilled parents have shorter leave durations, the marginal benefit from increasing leave length x is also greater.

additional child skills relative to their higher marginal gain to extending leave.

A reform that increases $LeavePay$ will affect optimal leave-taking, and accordingly child skills. The marginal benefit of going to work decreases (LHS of Eq. (2)) while the marginal benefit of staying home with the child (RHS) is unaffected. The reform thus unambiguously increases the optimal age at which the child enters daycare, x, for all parents.[17] There may be, however, heterogeneity across parent types regarding *how much* they increase x. The increase depends on each group's marginal utility of staying home with the child relative to working. All else equal, a marginal increase in $LeavePay$ changes the left-hand side by $-\frac{\partial v(c)}{\partial c}$, and this effect should be more pronounced for parents who are at a lower consumption level, i.e., low-wage parents and/or parents who choose a long leave duration. But it is not necessarily the case that the change in x that re-establishes optimality is larger for low skilled parents, as the change in the RHS for a given change in x depends on initial levels of x, parent skills θ^p, and the marginal utility of increasing child skills, $\partial z()/\partial \theta^c$.

The magnitude of the effect of the reform on child skills is determined not only by how much leave-taking changes, but also by the parent type, as well as the age at daycare entry in absence of the reform. Because of the concavity of the skill formation function, postponing daycare entry from a lower initial level yields a higher return. At the same time, spending more time with a high-skilled parent yields a higher return, holding everything else equal. But initial child skills are also expected to be higher for the high-skilled parents, which would conversely imply lower marginal gains to further investments. Thus, while there are mixed predictions as to whether children from highly skilled parents should benefit most from the reform, it is unambiguous that children experiencing the lowest initial leave should benefit the most from additional leave taking, all else equal.

[17] Unless f_x is negative - implying that the child is worse off by being at home. Note that in that case, the first order condition is not binding and thus there is no trade-off for the parent. Importantly in that special case, a reform of leave pay would not affect the leave decision unless the leave pay was larger than the wage.

4 Empirical approach

Our empirical strategy exploits the discontinuity created by the sudden introduction of the parental leave reform in the beginning of 2002. Because the discontinuity in parental leave length coincides with the discontinuity in school starting age, we combine a Regression-Discontinuity (RD) and a Difference-in-Differences (DiD) approach (sometimes referred to as an RD-DiD or a "differences-in-discontinuities" estimator). A simple RD design would not be sufficient to identify the effect of the reform, as it would also pick up differences in the outcomes that are due to differences in school starting age between children born in December 2001 and January 2002. We therefore have to isolate the effect of extended maternity leave on the outcomes from other effects. To this end, we introduce a control group of children, born around the 1st of January 2003. These children are all born post-reform and hence, a discontinuity around 1st of January should only reflect different school starting ages. Our main sample hence includes children born around the 1st of January 2002 (the "2002 cohort", or the treatment cohort) as well as children born around the 1st of January 2003 (the "2003 cohort", or the control cohort).[18] In our main specification, we include children born up to 60 days before and 60 days after January 1st in 2002 and 2003.

We estimate the following equation by two-stage least squares:

$$\mathbf{y}_i = \beta_0 + \beta_1 \mathbf{Leave}_i + \beta_2 \mathbb{1}[\mathbf{Cohort}_i\mathbf{=2002}] + \beta_3 \mathbb{1}[\mathbf{Birthday}_i > \mathbf{0}] \quad + f(\mathbf{Birthday}_i) + \delta X_i + \epsilon_i, \tag{3}$$

where we instrument the length of maternity leave ($\mathbf{Leave}_i$) by

$$\mathbf{Leave}_i = \mathbb{1}[\mathbf{Cohort}_i\mathbf{=2002}] \times \mathbb{1}[\mathbf{Birthday}_i > 0], \tag{4}$$

with $\mathbf{y}_i$ being the outcome of interest for child i. $\mathbb{1}[\mathbf{Cohort}_i\mathbf{=2002}]$ is an indicator for the child being born within 60 days of January 1st, 2002, rather than being born within 60 days of January 1st, 2003. $\mathbb{1}[\mathbf{Birthday}_i > \mathbf{0}]$ is an indicator for the child being born after December

[18] Another potential control group would be children born in late 2000 and early 2001, all under the pre-reform rules, but data on the main outcomes is not available for all children in this cohort.

31st in a given cohort. The discontinuity in leave caused by the reform is captured by the interaction term, $\mathbb{1}$**[Cohort**$_i$**=2002]**$\times\mathbb{1}$**[Birthday**$_i > 0$**]**. In addition, we control for a function of the running variable, $f($**Birthday**$_i)$, which in our main specification is linear. This running variable function is estimated jointly across cohorts, but we allow for different slopes before and after the cutoff. Finally, in our main specification we also include a vector of various background characteristics, X_i.[19]

4.1 Validity

To interpret our estimates as causal effects on child outcomes, several assumptions has to be made. Given the validity of these assumptions, our estimates are interpretable as local average treatment effects capturing the effects of increasing leave among children of mothers who increased their leave as a result of the reform.

First, mothers should not be able to manipulate the exact date of birth in order to strategically choose the parental leave scheme they prefer. As explained earlier, this is quite unlikely because of the sudden implementation of the reform. Nevertheless, we consider the possibility by first visually inspecting the frequency of births around the discontinuity. Fig. A.3 shows the number of births by week and cohort in the three birth cohorts around the implementation of the reform. There are generally fewer births just before 1st of January than immediately after. As this pattern appears for all cohorts, this strongly suggests a case of general seasonality rather than manipulation of the timing of births in response to the reform. We also formally test for a discontinuity using the density estimator introduced by Cattaneo et al. (2020). The test reveals that there is no significant additional discontinuity in the frequency of births around 1st of January in the treatment cohort (p-value. 0.211).

Second, the average effect of school starting age on the child outcomes must be the same for treated and control cohorts such that we can isolate the effect of longer leave from the effect of different school starting ages. While we cannot directly test this, we can perform a placebo test using two control cohorts. Specifically, we run a reduced form version of our

[19] While this should in principle not be necessary if the timing of birth is random, we do see a few imbalances around the discontinuity that are most likely random but nevertheless may affect the estimates (see Table 2).

main specification using children born around January 1st 2003 and 2004. Using the two control cohorts, we find no significant effects on our main outcomes.[20] This is consistent with the effect of school starting age on socio-emotional skills being similar across cohorts.

Third, the standard IV approach imposes an assumption about monotonicity, implying that the instrument only shifts the treatment variable in one direction. In our setting, monotonicity is thus violated if the reform induces some mothers to take shorter leave than they would have done in absence of the reform. We argue that this is not a large concern for interpretation of our results. In particular, the concern is greatly alleviated by the fact that mothers giving birth immediately after the reform had the option to choose the old scheme. This choice makes sense for mothers with a strong preference for a long leave duration. However, a potential issue remains if some mothers with less strong preferences for long leave but still strong enough to take more than 46 weeks of leave pre-reform prefer the new rules and are thereby induced to take less leave (but at a higher compensation rate). Looking at Fig. 2b, we do observe an indication that this happens. Specifically, while the distributions of leave length of mothers look identical for more than one year of leave, there is a small part of the distribution just below one year that appears to shift slightly in the negative direction. The reform induces these mothers to reduce their length of leave from 50 to 46 weeks.[21] While this does represent a violation of the monotonicity assumption, the bias caused by this should be small. The bias will be proportional to the average shift in the first stage relative to the shift in the reduced form coming from a difference in average treatment effects between compliers and defiers (Angrist et al. 1996). Based on Fig. 2b, the size of the reverse first stage compared to the main first stage suggests a bias of between 0 and 5 percent.[22] An alternative interpretation is that with some relatively small group of defiers, the LATE is still

[20]The p-values are 0.77, 0.27, 0.35, and 0.87, for well-being, conscientiousness, agreeableness, and emotional stability, respectively.

[21]Closer examination reveals that there is some pre-reform bunching happening at exactly 50 weeks of leave. Presumably, this reflects some sort of preference for round numbers, where the additional 2 weeks of leave earmarked to the father brings the total parental leave up to exactly one year.

[22]If the size of the defier group is one tenth of the complier group, and the average negative shift in length of leave is one fifth of the positive shift among compliers (e.g., a 4 weeks reduction vs. a 20 weeks increase), the bias will be 5 percent if at the same time, the true effect among defiers is zero. If the true effect among defiers is instead half of that of the effect among compliers, the bias will only make up 2.5 percent. If average treatment effects are the same, there will not be any bias.

unbiased but simply identifies the average treatment effect among a slightly different group of compliers (De Chaisemartin 2017).[23]

Finally, if the variation induced by the reform is exogenous, it should be independent of not only the potential outcomes but also other pre-determined characteristics, implying that the observable variables should display continuity around the reform cutoff. We relegate the investigation of differences in observables to after the description of the data in Section 5.2.

5 Data

The data for our empirical analyses is based on a link between population-wide administrative registers and a nearly universal survey of school children's well-being. We link the registers and the survey via unique identifiers for the children, which provides us not only with measures of the children's socio-emotional skills and well-being, but also with child characteristics (e.g., exact day of birth, child birth weight), and a link to their parents and the associated parental information (e.g., education and earnings). Because the reform only affected the leave taken by mothers (Beuchert et al. 2016), we disregard the leave taken by fathers to simplify the analysis without any loss of variation. To measure the length of maternity leave, we use the sick leave register (the parental leave compensation is based on the unemployment/sick leave benefits *dagpenge*).

In this section, we first describe how we obtain our outcomes of interest. We then present a range of summary statistics and investigate covariate balancing, and finally, we provide details on how the reform affected leave taking.

5.1 Outcomes

Our main outcomes of interest are measures of children's socio-emotional skills and well-being, which we construct from The Danish Well-Being Survey (DWS, *trivselsmålinger* in

[23]Intuitively, if there exists a subgroup of compliers of the same size and with the same treatment effect as the defiers, they will simply cancel each other out, and the LATE will still identify the treatment effect among the *remaining* compliers.

Danish). Since 2015, public schools are required to administer the survey yearly.[24] Students in grades 0-9 fill out the surveys electronically during a class session. They are informed that the purpose of the survey is to improve their well-being and that their responses will not be shown to their parents or teachers. While participation is voluntary, response rates are high (over 90 percent).

The questionnaire contains 40 questions (students in grades 1-3 answer a shorter 20-question version) addressing different aspects of well-being and school life in general. The outcomes are constructed following Andersen et al. (2020). While the DWS was not originally designed to measure personality, sufficiently many of the questions resemble items from questionnaires based on the five-factor model of personality (the "Big Five"), the dominant psychological model of the core domains of personality. Three dimensions of personality can be extracted and have been validated against the Big Five (Andersen et al. 2020): conscientiousness, agreeableness, and emotional stability. **Conscientiousness** captures the child's tendency to be organized, responsible, and hardworking (American Psychological Association 2022). **Agreeableness** captures the child's tendency to cooperate and act in an unselfish manner. **Emotional Stability** describes the child's robustness to stress and anxiety. In addition, we also construct a measure of **general well-being**. Table 1 shows the items used to construct each measure. All items are measured on a five-point Likert scale. We construct personality and well-being scales by standardizing each item across years and within each grade level.[25] We then average the items within each domain and standardize again.

The treated and control cohort used in our analysis are observed in the DWS in 7th and 8th grade in the waves 2015-2018, allowing for both early and late school enrollment (timeline in Appendix Table A.2).[26] We may observe an answer from the same student in both 7th and

[24] Most children attend public schools (77 % overall and 75-77% of children in grades 7 and 8 in 2015-2018, see Statistics Denmark 2022a). We do not find that the reform affected enrollment in private schools.

[25] We do not standardize the items within school year, as this risks eliminating the variation that our estimation strategy relies on. However, all the main results are robust to other standardizing procedures. For example, we control for survey wave-effects by regressing each outcome on dummies for each school year, using only the "always treated" cohorts. We then used the residuals from this, which accounts for any systematic differences across waves, as alternative outcomes, see Appendix Table A.1.

[26] It is important to allow for early and late school starting, given that the length of paid maternity leave could also affect the distribution of school starting age. We do, however, not find any evidence of this, see Table A.3.

Table 1: Items used to measure socio-emotinal skills and well-being

Conscientiousness	How often can you complete what you commit yourself to? Are you able to concentrate in class? If I'm interrupted, I can quickly concentrate again.
Agreeableness	I try to understand my friends when they are sad or angry. I am good at collaborating with others.
Emotional Stability	Do you feel lonely? How often do you feel safe at school? Other students accept me for who I am.
General well-being	Are you happy with your school? Are you happy with your class?

8th grade. As the survey responses should be considered noisy measures of the underlying socio-emotional skills and well-being, observing two responses increases precision.

We complement our analyses of well-being and self-reported socio-emotional skills by examining the effect on academic achievement in 9th grade and school absenteeism. Both of these outcomes are objective (as opposed to self-reported survey information), yet they are likely influenced by well-being and socio-emotional skills. Therefore, they can be seen as objective measures complementing our main findings. Absenteeism in particular can be seen as a direct function of well-being and emotional stability (feeling happier and more secure at school means fewer reasons to avoid it), as well as of conscientiousness (a desire to follow the rules or to work towards achieving ones goals are reasons to attend school). For academic achievement, there is also a well-established link to conscientiousness, whereas it is less clear whether well-being and other personality traits also affect children's achievement directly (see, e.g., Andersen et al. 2020).

Teachers register which students are absent each day. We measure school absenteeism as the fraction of days of the school year that the student has been absent. Our measure of academic performance relies on grades given in 9th grade.[27] We consider both exit exams

[27]Since this is one year later than the DWS, the data on grades extends to the school year 2019/2020. We do not include the DWS from 9th grade because many students choose to take 9th grade at a private boarding school (*efterskole*) where the DWS is not mandatory, and the answers are therefore missing for a substantial fraction of 9th graders. School grades are, however, recorded at the private boarding schools.

and the continuous assessment in 9th grade. Exit exams are assessed with external censors, whereas the continuous assessment is based on the teacher's evaluation of class participation throughout the year. The teacher evaluation is thus "non-blind" in contrast to the exit exam grades, and thus will likely reflect more of the students' characteristics than the "quasi-blind" exit exam grades (Burgess and Greaves 2013). Hence, socio-emotional skills are thought to drive the continuous assessment marks more than exit exams. Each of these outcomes are standardized within each school year.[28]

Table A.5 shows descriptive statistics of our outcome variables across family and child characteristics. As one would expect, we see a clear SES gap in socio-emotional skills and well-being, as children of mothers with higher incomes and, in particular, longer educations score more favorably on all outcomes. The same is true for first born children, while the differences between boys and girls go in different directions.

5.2 Descriptive statistics

Table 2 presents summary statistics for our main sample, which consists of children born within a window of 60 days before and after January 1st 2002 or before and after January 1st 2003. The first column displays sample means and standard deviations for various child and parental background characteristics. The table also reports RD-regression results checking for discontinuities in these background variables, first separately in the 2002 and 2003 cohorts (columns 2-3), and then testing for differences between these two cohorts (columns 4-5).

Several variables show some discontinuity around January 1st in both cohorts. The *differences* between the treatment and the control cohorts, however, are only significant for child birth weight and maternal education. The p-values imply that this is approximately what we would expect by chance, and hence, this does not cause us to question the identification strategy. We will include the background characteristics as controls in our preferred specification. While the relative difference in birth weight is small (32 grams) and probably not economi-

[28] We standardize school grades within school year, as the exams differ each year, making the average grade fluctuate across school years. Standardizing takes out differences in the difficulty of the exam in that particular school year. The DWS, in contrast, is the same each year.

TABLE 2: Descriptive statistics and covariate balancing

	(1) Sample mean	(2) RD (treatment) Pre-Post 1/1/2002	(3) RD (control) Pre-Post 1/1/2003	(4) RD DiD (3)-(2)	(5) p-value of (4)
Child characteristics					
Female	0.488	0.018	0.026	-0.005	0.692
	(0.500)	(0.019)	(0.019)	(0.013)	
Birth weight	3471.223	-6.486	-48.510	-32.345	0.090
	(719.541)	(27.659)	(27.152)	(19.085)	
APGAR score	9.733	0.040	-0.120	-0.041	0.241
	(1.308)	(0.050)	(0.050)	(0.035)	
Gestational age	274.530	0.930	-2.050	0.033	0.969
	(31.767)	(1.251)	(1.165)	(0.843)	
Birth order	1.771	-0.044	0.016	-0.023	0.335
	(0.905)	(0.034)	(0.035)	(0.024)	
Immigrant	0.035	0.011	0.005	0.001	0.840
	(0.184)	(0.007)	(0.007)	(0.005)	
Maternal characteristics					
Age at birth	30.315	-0.361	0.066	0.078	0.518
	(4.527)	(0.172)	(0.173)	(0.120)	
Years of education	13.058	-0.099	-0.232	0.147	0.034
	(2.628)	(0.099)	(0.101)	(0.070)	
Income (1,000kr)	223.625	2.054	2.556	1.579	0.545
	(98.417)	(3.943)	(3.524)	(2.610)	
Unemployment	0.057	-0.006	0.003	-0.001	0.708
	(0.146)	(0.006)	(0.005)	(0.004)	
Paternal characteristics					
Age at birth	32.516	-0.141	0.062	0.163	0.290
	(5.813)	(0.220)	(0.223)	(0.154)	
Years of education	12.496	-0.075	-0.198	0.057	0.463
	(2.947)	(0.111)	(0.114)	(0.078)	
Income (1,000kr)	312.840	34.009	12.396	5.251	0.664
	(455.798)	(22.774)	(7.905)	(12.093)	
Unemployment	0.030	0.004	0.006	-0.004	0.142
	(0.113)	(0.004)	(0.004)	(0.003)	
N	22,755	11,692	11,063	22,755	

Notes: Covariate means (or differences) and standard deviations. Col. (1) presents simple means for the full sample. The difference between the subsample born before minus born after January 1st in 2002 is shown in col. (2) and 2003 in col. (3). Columns 4 and 5 tests the difference in the discontinuities between the two cohorts.

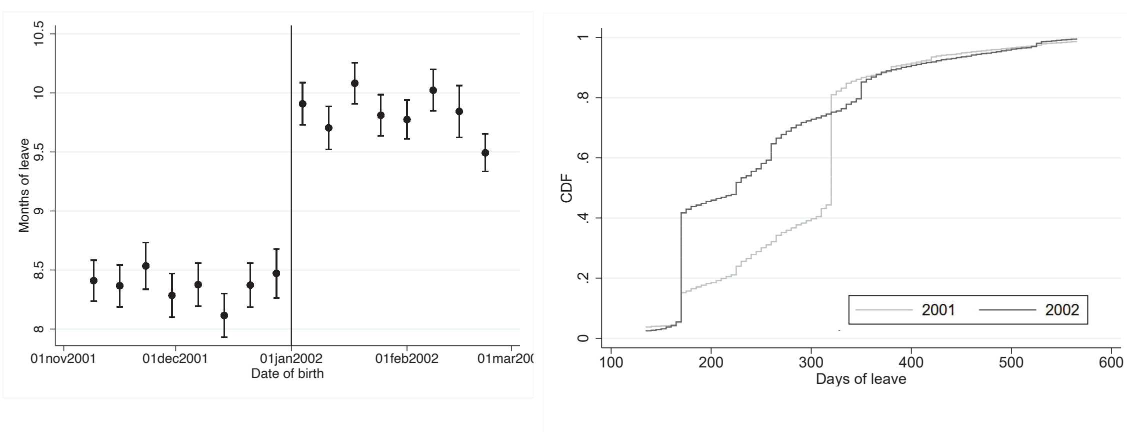
cally significant, the difference in maternal education (0.15 years) should not be ignored.

5.3 Leave taking

We now show to what extent the reform affected leave taking. Fig. 2a illustrates the increase in average leave taking around January 1st 2002. The reform increased average leave by 1.4 months, thus delaying average child age at entry into daycare from 8.3 to 9.7 months (and the median from 7.5 to 10.7 months). Fig. 2b illustrates the distribution of leave length before and after the reform. While a large share of mothers chose to take leave corresponding precisely to the maximum full benefit duration both before and after the reform, a lot of variation remains. Around 40 percent of pre-reform mothers took no more than 24 weeks of leave (the length at which they could receive the full benefit level). Of those who did take additional leave at a reduced benefit rate, many did so for several months. More than 40 percent of post-reform mothers did not take advantage of the full 46 weeks of leave available at the full benefit level, and more than 80 percent took 46 weeks of leave or less.

FIGURE 2: First stage and distribution of leave

(A) Average length on maternity leave (B) Cumulative distribution of leave



Notes: Panel A shows the average months of leave by week of birth for mothers who gave birth before and after the implementation of the reform. Panel B plots 2SLS point estimates from separate regressions with controls.

First-stage estimates are given in Table A.4, which also reports how they differ by some of the background characteristics. We see a significant increase of leave for all groups, ranging from an average of 1 to 2 months of additional leave.[29] Mothers in the highest income tercile are less responsive to the reform, while mothers with a medium length of education (12 years) are very responsive. We do not find differences with respect to birth order or sex.

Our theoretical model of skill formation (Section 3) highlights the role of *initial* leave duration as a moderator of the effect of extending leave. Because of sensitive periods, the marginal effect of spending one additional day with the parent rather than starting in daycare are likely to be greater at the youngest ages. To look more into this heterogeneity, we first predict mothers' leave duration in absence of the reform on the basis of observable family characteristics. To do this, we regress a dummy for short leave (24 weeks or less) on the full set of background characteristics from Table 2 using a probit model.[30] We restrict the estimation sample to mothers giving birth before the reform, and predict preferences for all mothers. Thus, the predicted value is interpretable as the likelihood that mothers, *in the absence of the reform,* would take no more than 24 weeks of leave (the number of weeks with full benefit compensation in the pre-reform period). The marginal effect of each covariate on this probability is shown in Table A.6.

Several patterns are interesting and can be related to our theoretical model. As high-income mothers earn a higher wage, they may increase their consumption relatively more if they shorten their leave duration. On the other hand, for a given leave duration, high-income mothers have a higher consumption level, which makes additional consumption relatively less valuable (as $v'' < 0$). In Section 3, we argue that the former mechanism is more likely to dominate, given that it is first-order rather than second-order. Indeed, we find that high-income mothers take shorter leave, thus suggesting that the bigger increase in consumption generally outweighs the lower marginal utility of additional consumption. Conversely, we observe that higher paternal income is associated with a preference for longer leave. This is predicted unambiguously by the model, as higher household income will shift the consump-

[29] This also speaks in favor of the monotonicity assumption. Specifically, it has been shown that a less strong version of the assumption, so-called *stochastic monotonicity*, is sufficient for identification. This implies that the first-stage should be non-negative for any sub-group defined by a set of covariates (Small et al. 2017)

[30] We allow for non-linearities by including a full set of dummy variables for educational level and birth order, as well as by including a squared term for age and income. We also include a set of two-digit sector code dummies (not shown) for the occupation of each parent.

tion level upwards, implying a lower marginal utility of additional consumption. Mothers with higher educational attainment are generally more likely to take short leave.[31] Higher education might be associated with higher returns to parental time investments, which all else equal would lead to highly educated mothers taking more leave. However, such greater productivity would also lead to a higher child skill level to begin with (especially if there is also strong heritability of skills) and thereby lower marginal utility from taking more leave to increase skills further (as $z'' < 0$). This mechanism appears to outweigh the first, as higher-skilled mothers, holding income constant, are more likely to take short leaves. Other associations include younger mothers being more likely to take short leave and mothers of first born children typically taking longer leave.

Interestingly, the children whom we predict to be most likely to enter daycare at a very early age in absence of the reform have lower levels of socio-emotional skills, well-being and academic performance, and higher absenteeism (see Table A.5). Though the marginal effects of educational attainment would suggest a negative relationship between leave duration and child skills, we note that most of the mothers have either 12 or 16 years of education. Mothers with 16 years of education generally take longer leave, compared to mothers with 12 years of education, which may explain a large part of the difference in child skills between mothers who are more or less likely to take short leave. At the same time, other characteristics also predict leave taking. While a marginal increase in maternal income is associated with taking shorter leave, other characteristics that are generally associated with *lower* socioeconomic status also predict *shorter* leave taking, e.g., lower income of the father, either parent being younger at child birth, and higher birth order. Taken together, this implies that the children who are most likely to experience short spells of parental leave are negatively selected on all our outcomes of interest. With regards to the parental characteristics, we see that on average, the children who are most likely to experience short leave also have parents with less education, and fathers with lower income (see Section A.2), which may explain the differences with respect to child skills.

[31] One exception to the generally increasing probability of short leave with rising education is the group with 16 years of education. Teachers, nurses and pedagogues make up the majority of this group, and women in these professions are very unlikely to take short leave—likely reflecting both preferences and job characteristics.

6 Effects of leave duration on outcomes in adolescence

We now present our main results of the effects of extending parental leave duration on child outcomes. Estimates from the RD-DiD regressions are reported in Table 3. They show the effect of an additional month of leave on child socio-emotional skills and well-being. In the first panel, we use the baseline specification from Eq. (4) without control variables. We next add the background characteristics from Table 2. In the last panel, we add birth municipality fixed effects. As explained in Section 4.1, our preferred specification includes control variables, as we observe differences in a few parental and child characteristics between the treatment and control cohort.[32]

Increasing the length of leave significantly improves adolescent outcomes along several dimensions. One additional month of leave increases well-being in adolescence by 4.7% of a standard deviation in our preferred specification. One more month of leave also increases conscientiousness by 3.5% and emotional stability by 2.8% of a standard deviation. For children of the median mother, who increased leave by 3.2 months, this would correspond to increases of well-being by 15.04 percent of a standard deviation, 11.2 percent for conscientiousness and 9.0 percent for emotional stability (assuming linearity between the effect and lenght of leave). We find no effect on agreeableness, regardless of the specification.

In Section A.1, we show that the main results are robust to varying the window size, and for controlling for systematic differences across DWS waves.

6.1 Heterogeneous effects

Our theoretical framework pointed to several factors that could lead to greater effects of extending time with the parent at home and delaying entry into formal care. One major factor is the age at which the child would enter institutional care, as this influences the curvature of the production function of skills with respect to the inputs of parental investments relative to

[32] Additional investigation reveals that the differences in results between the first and second panels stem almost entirely from controlling for the mother's years of education, which, as explained in the previous section, constitutes the only significant covariate imbalance. This induces a negative bias because maternal education is negatively correlated with our treatment variables, but positively correlated with our outcome variables, especially conscientiousness (see Appendix Table A.5).

TABLE 3: Effects of leave duration on child outcomes

Outcome:	(1) Well-being	(2) Conscient.	(3) Agreeableness	(4) Emot. Stability
Months of leave	0.043**	0.022	0.003	0.025
	(0.017)	(0.017)	(0.017)	(0.017)
Controls				
Municipality F.E.				
Months of leave	0.048***	0.032*	0.005	0.028*
	(0.017)	(0.017)	(0.017)	(0.017)
Controls	X	X	X	X
Municipality F.E.				
Months of leave	0.047***	0.035**	0.006	0.028*
	(0.017)	(0.017)	(0.017)	(0.017)
Controls	X	X	X	X
Municipality F.E.	X	X	X	X
N	40,236	40,082	40,167	39,340

Notes: This table reports parameter estimates from 2SLS regressions estimating the effect of the length of maternity leave on personality and well-being in 7th and 8th grade, following Eq. (4). Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors are reported in parentheses and clustered at the individual child level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

daycare. In this section, we therefore begin by examining whether additional leave is more beneficial for children of mothers who, in absence of the reform, would have taken short leave, Thereafter, we look at heterogeneous effects by specific maternal and child characteristics.

Baseline age at daycare entry Table 4 uses the predicted leave taking behavior in absence of the reform described in Section 5.3, splitting the sample by whether a mother's probability of short leave in absence of the reform is above or below the median.[33]

We find that for children who would have been likely to enter daycare at an early age in absence of the reform, there are large and significant positive effects of longer leave. For

[33]The median pre-reform leave length is 7.5 months, and the median prediction is a 40.5 percent probability of taking no more than 5.6 months of leave in absence of the reform.

TABLE 4: Heterogeneous effects of leave by initial leave duration

Outcome:	(1) Well-being	(2) Conscient.	(3) Agreeableness	(4) Emot. Stability
Months leave (Short leave unlikely)	0.025	-0.018	-0.031	-0.021
	(0.032)	(0.032)	(0.029)	(0.039)
Months leave (Short leave likely)	0.060***	0.065***	0.027*	0.056***
	(0.020)	(0.020)	(0.016)	(0.019)
N	40,236	40,082	40,167	39,340

Notes: This table reports parameter estimates from 2SLS regressions estimating the effect of the length of maternity leave on personality and well-being in 7th and 8th grade, estimated separately by whether the mothers are predicted to have a likelihood of taking short leave above or below the median. Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors are reported in parentheses and clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

these children, an additional month of leave leads to a 6.2% of a standard deviation increase in well-being, a 5.6% of a standard deviation increase in conscientiousness and a 5.0% of a standard deviation increase in emotional stability. Children of mothers who were least likely to opt for a short leave duration in absence of the reform, and would have entered day care at a relatively older age, did not benefit from a further increase in leave duration in terms of any of the main outcomes.

The group of mothers who were most likely to take long leave already pre-reform also responds less to the reform in terms of the first stage effect on leave duration (see Table A.4). This implies that the reform made leave durations more equal. Furthermore, because children of mothers who were most likely to take short leave are negatively selected on all outcomes (see Table A.5), these results also imply that the biggest increase in socio-emotional skills and well-being happened among children who would otherwise have been particularly worse off along these dimensions. In other words, the change in parental leave induced by the reform decreased inequality in socio-emotional skills and well-being. Relative to the main effects in Table 3, the heterogeneities are particularly strong for the personality measures, which could reflect that these outcomes are more sensitive to the timing of investments compared to general well-being.

Maternal and child characteristics We also consider the possibility of heterogeneous effects across maternal income and education as well as child sex and birth order. We do not find clear evidence that any of these characteristics give rise to additional important heterogeneities. Table 5, Panel A, shows that there appears to be an income gradient, with larger effects for children of higher income mothers. However, the gradient could stem solely from the fact that high-income mothers, conditional on the other characteristics, tend to take short leave, as shown in Table A.6. While it could also interpreted as evidence of higher productivity of parental investments among high income mothers, this is not consistent with what we see in Table 5, Panel B, as additional leave does not appear to be beneficial for highly educated mothers specifically. On the other hand, this is again consistent with initial leave duration being the important distinction, as highly educated mothers tend to take relatively long leave.[34] We do not find any significant heterogeneities by child sex or birth order, c.f. Tables A.8 and A.9.

6.2 Academic achievement and absenteeism

We find no effects of longer leave on academic achievement on average (see columns 1-4 of Table 6), but if we again consider heterogeneity by the likelihood of taking short leave in absence of the reform, we find effects that follow the same pattern as our main outcomes. That is, children of mothers who would have taken short leave in absence of the reform benefit from expanding the paid leave period. An additional month of leave for this group of children increases grades in both Danish (4.2%) and mathematics (3.4%) in terms of the continuous assessments throughout the school year. The results point in the same direction for GPA on exit exams, although the effects are not significant.

The effect on the continuous assessment could stem from an indirect effect via improved socio-emotional skills. Alternatively, the effect could stem directly from the longer leave duration improving achievement independently of the socio-emotional skills. The former is

[34]While this is not true for the most highly educated mothers, the majority of the "more than 12 years" category stem from a group with 16 years of education, which includes professions such as teachers, pedagogues and nurses, and they tend to take the longest leave.

TABLE 5: Heterogeneous effects of leave by maternal income and education

	(1) Well-being	(2) Conscientiousn.	(3) Agreeablen.	(4) Emot. Stability
(A) Effects by Maternal Income				
Income tercile 1	0.017	0.001	-0.019	0.008
	(0.028)	(0.028)	(0.028)	(0.028)
Income tercile 2	0.048^{*}	0.055^{*}	0.010	0.001
	(0.029)	(0.029)	(0.028)	(0.029)
Income tercile 3	0.078^{**}	0.049	0.028	0.076^{**}
	(0.033)	(0.031)	(0.031)	(0.032)
(B) Effects by Maternal Education	(1)	(2)	(3)	(4)
Educ $<$ 12 years	0.016	-0.022	-0.034	-0.104
	(0.074)	(0.074)	(0.071)	(0.078)
Educ $=$ 12 years	0.078^{***}	0.064^{***}	0.024	0.074^{***}
	(0.023)	(0.022)	(0.022)	(0.023)
Educ $>$ 12 years	0.010	0.013	-0.005	-0.006
	(0.027)	(0.027)	(0.027)	(0.027)
N	40,236	40,082	40,167	39,340

Notes: Columns (1)-(4) report 2SLS estimates of the effect of the length of maternity leave on child outcomes in 7th and 8th grade. The effects are estimated across different levels of yearly income of the mother measured the year before birth. Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth.Standard errors in parentheses are clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

probably more likely, given that the continuous assessments in particular are thought to be closely related to socio-emotional skills. Furthermore, the estimates for academic achievement square up neatly with the effect one would obtain by multiplying the effect sizes we find on socio-emotional skills with existing estimates of the effects of these skills on academic achievement measured with standardized tests. Looking at the same measures as we do, Andersen et al. (2020) find that a one standard deviation increase in conscientiousness is associated with 30% of a standard deviation higher reading ability in grade 8. An additional month of leave leads to a 6.5% increase of a standard deviation in conscientiousness among children who were likely to have entered daycare at an early age in absence of the reform. This would translate to an effect of ($6.5 \times 0.30 =$) 2.0 percent of a standard deviation increase in reading skills from an additional month of leave. This is consistent with the estimated effects found in Table 6, Columns 1-2 (1.5-2.5 percent of standard deviation on Danish/math exit exam grades).

TABLE 6: Academic performance and absenteeism

	Exit exams		Continuous Assessment		Absenteeism
	Danish	Math	Danish	Math	Share of year
	(1)	(2)	(3)	(4)	(5)
Overall Effect					
Months of leave	-0.006	0.005	0.011	0.006	-0.0017^{*}
	(0.010)	(0.012)	(0.011)	(0.013)	(0.0010)
Results by Mother Type					
Months leave (Short leave unlikely)	-0.017	-0.006	-0.005	-0.008	0.0005
	(0.019)	(0.023)	(0.022)	(0.026)	(0.0016)
Months leave (Short leave likely)	0.015	0.025	0.042^{***}	0.034^{**}	-0.0024^{**}
	(0.012)	(0.016)	(0.014)	(0.015)	(0.0010)
N	35,382	35,266	35,491	35,463	69,573

Notes: This table reports parameter estimates from 2SLS regressions estimating the effect of the length of maternity leave on grade point averages from exit exams or continuous assessment (teacher-assigned grades) in 9th grade, and absenteeism (measured in fraction of total school days over the year). Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors in parentheses are clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Extended paid parental leave also improves school attendance (see column 5 of Table 6), with one additional month of leave reducing the fraction of the school year that the child is absent by 0.17 percentage points. As the average student is absent for 6.4% of the year, this corresponds to a 2.7% reduction in absence. Again, we find significant heterogeneities, with no effects for children of mothers unlikely to take short leave, but a significant 0.24 percentage points reduction for the children of mothers who would be likely to take only short leave in the absence of the reform. As was the case for the main outcomes, the heterogeneous effects on academic performance and absenteeism also imply that the increase in leave decreased overall inequality in these objective school outcomes.

6.3 Assessing the importance of the effects

Having just demonstrated that longer leave during infancy, and thus delaying entry into daycare, significantly improves socio-emotional skills in adolescence, we now try to get a sense of what this implies for more classical economic outcomes in adulthood.

While we do not observe adult outcomes in our sample, we know from existing research that "personality traits predict outcomes in virtually all major life domains" (Bleidorn et al. 2021). The magnitude of effects of personality traits on life outcomes is as large or larger than other "predictors of success," such as IQ or educational attainment. They also work independently from IQ or general cognitive ability (Conard 2006; Spengler et al. 2015). Therefore, establishing that *any* of those socio-emotional skills, or personality traits, are significantly affected by the reform and age at entry of daycare, implies significant long-term outcomes in practically all domains of life. These include educational attainment (which is to be expected as an extension of academic performance, see for example, Beuchert and Nandrup 2018 or Poropat 2009), work productivity and earnings (Cubel et al. 2016; Fletcher 2013; Gensowski 2018; Heineck and Anger 2010; Müller and Plug 2006; Nyhus and Pons 2005), crime (Eysenck and Eysenck 1970), relationship satisfaction, marriage and divorce (Roberts and Bogg 2004; Wagner et al. 2015), mental health (Widinger 2011), health (Moffitt et al. 2011), and longevity (Friedman et al. 2010; Savelyev 2020). Our own findings also suggest that school achievement and absenteeism may belong on the same list.

In terms of the magnitudes of the effect sizes, our findings are in the same order of magnitude, though somewhat larger than Fort et al. (2020), who find that a relatively high SES sample of children see reduced openness (-1.4%), agreeableness (-1.2%), and emotional stability (-0.9%) at ages 8-14 from an extra month in daycare before age 2.[35] The magnitude of our findings is also comparable to effect sizes in the few other causal studies of determinants of personality. Schwaba et al. (2021), for example, show that an increase in lead

[35]One source of the difference in magnitudes could be that the children in Fort et al. (2020) are older at the daycare entry, and they come from a relatively affluent sample.

pollution in childhood by one standard deviation (a disconcertingly large increase in a serious toxicant) led to a decrease by .03 of a standard deviation in adult agreeableness and .079 in conscientiousness. Other studies found no generalized effects of the Covid pandemic on conscientiousness (Sutin et al. 2020), and no effects of Hurricane Harvey on any personality traits (Damian et al. 2021). Somewhat relatedly, there are also studies of personality change following certain life events in adulthood. Denissen et al. (2019), for example, find that adults increase conscientiousness by .03 and life satisfaction by .05 of a standard deviation in the year after transitioning into employment.

One could also compare the magnitudes we find for parental leave to effect sizes from early-childhood interventions. These cover different ages, of course, but are interesting nonetheless. The overview by Murano et al. (2020) shows that effect sizes of successful skills-based interventions on social and emotional competence range from about .2 to .4 of a standard deviation. A single additional month of full-time parental care would thus give effects corresponding to a fifth to a quarter of the magnitude of specific interventions. This is of course given that the effects of the interventions do not *fade out* over time, which is often the case (Bailey et al. 2020). It is not surprising that these targeted interventions find larger effect sizes than we do as they are evaluated right after treatment. This is in line with patterns described in the fade out literature and stress the importance of looking at long run outcomes.

7 Discussion

In this paper, we shed new light on the question of whether children benefit from an extended period of parental care (as opposed to entering daycare) at an early age. Exploiting a Danish reform which extended the length of paid parental leave, we show that children whose mothers were induced to take longer leave were better off many years later. Specifically, we find that longer leave increased the children's well-being, conscientiousness, and emotional stability in adolescence. We find that the effects are strongest for children who, in absence of the reform, would have been most likely to enter daycare at a particular early age. While we find no effect on the children's GPA on average (consistent with previous studies: e.g.

Rossin-Slater 2017; Rasmussen 2010; Dustmann and Schönberg 2012; Dahl et al. 2016), there are significant effects on continuous assessment grades for the children who benefited most in terms of their socio-emotional skills and well-being. We also find that longer leave reduces school absenteeism.

To interpret our results, we relate them to a stylized theoretical framework describing the parental leave decision and its consequences for child development. The finding that longer leave is particularly beneficial for children who would otherwise have entered daycare at a very early age is consistent with child socio-emotional skills being an increasing but concave function of parental leave duration. This is also consistent with the skill formation literature stressing the importance of the very early childhood (Cunha and Heckman 2008) as well as psychological theory on distinct phases of attachment development (Bowlby 1973).

In contrast to most of the parental leave literature, our setting is particularly well suited to make the comparison between parental care and formal child care, as female labor market participation is high in Denmark, and the use of other informal childcare arrangements is rare. One reason why other studies do not find effects of extended leave policies may thus be that the effects of parental care versus daycare is conflated with the effects of informal care, provided, for example, by the extended family. Another reason may be that parents often remain the main carers even if they do not have access to paid parental leave.

The setting also makes it more likely that the relevant mechanism by which longer leave affects child skills is through an increase in parental time investments. This mechanism is also standard in the skill formation literature, but alternative mechanisms include household financial investments or intermediate effects on health. However, we do not observe any effect on household income once the parental leave is over, which is probably due to the comparatively gender-equal Danish labor market's generally high participation rates. Thus, it is unlikely that the income channel plays an important role in this context. Furthermore, other research suggests that longer leave induced by the reform had no effects on child health (and very limited effects on maternal health) (Beuchert et al. 2016). Arguably, our findings are therefore most likely caused by increased parental time investments (and later entry into daycare) in the early period of life.

Our findings emphasize the need to consider multiple dimensions of child development. A narrow focus on cognitive development, which has been dominant in this literature, risks missing important aspects. While we are not the first to consider socio-emotional outcomes, we are, as far as we are aware, the first to show directly that longer leave with the mother positively affects child socio-emotional skills. A few studies have found related effects, which our findings should be viewed in connection with. In particular, Heisig and Zierow (2019) find that longer parental leave increases life satisfaction of the affected children in adulthood. Taken together with our findings of positive effects on well-being in adolescence, this suggests that longer leave may have permanent effects on happiness throughout life. Our findings on personality are reminiscent of Fort et al. (2020), who show that additional early daycare can have negative effects on several personality traits. We extend their findings to a representative sample where early daycare is replaced with parental care specifically.

While arguments for extending paid leave is often made on the grounds of labor market inequality (between high- and low-earners or between men and women), there may be an additional argument in the form of inequality in child development. An implication of our findings is that more generous parental leave policies have the potential to close part of the socio-economic gap in skills, well-being, school absenteeism, and academic achievement.

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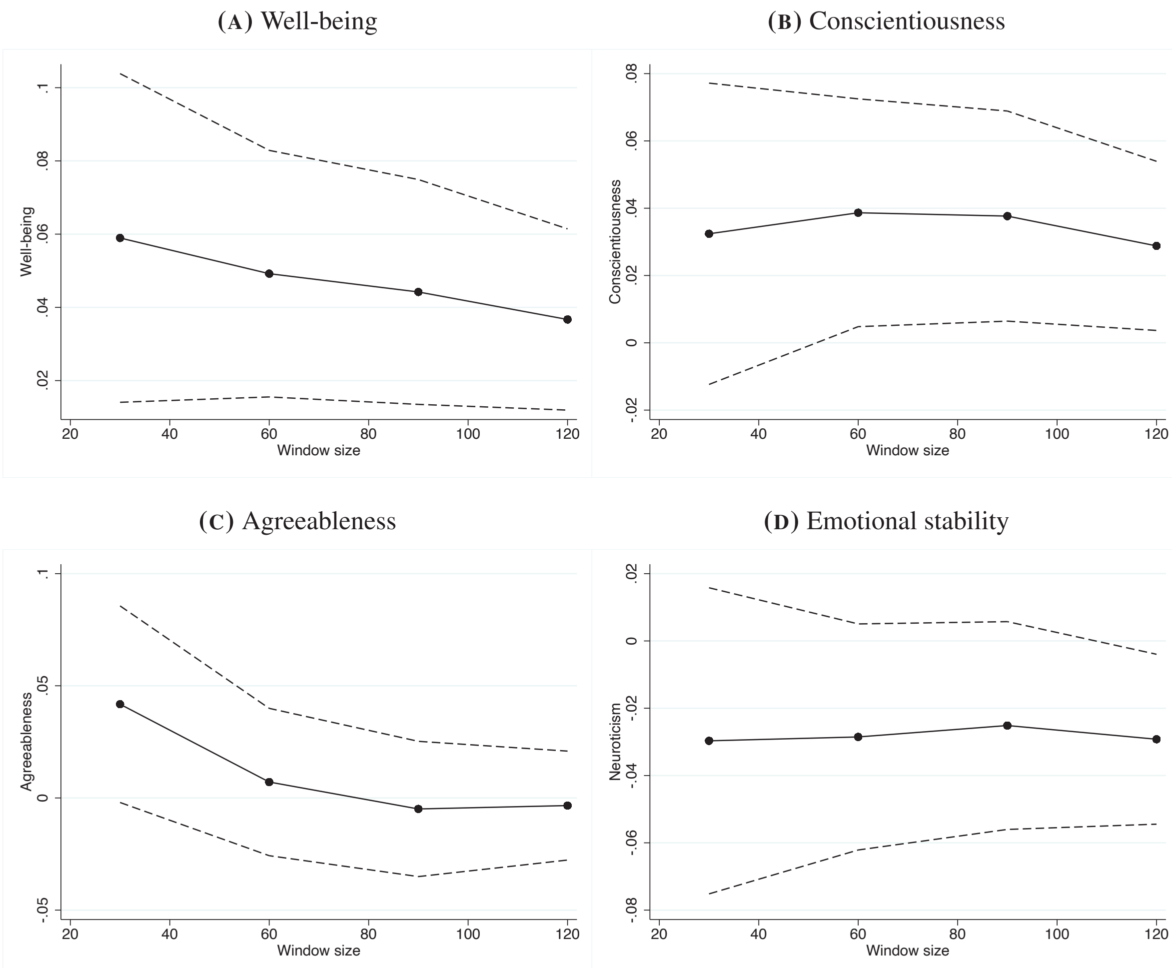
A Appendix

A.1 Robustness check

In this section, we test the robustness of our results. First we vary the window size of our estimation sample. Varying the window size can be thought of as entailing a bias-variance trade-off. Increasing the size means using more observations and therefore increasing precision. But this might come at the expense of bias if there is unobserved heterogeneity with respect to the running variable (date of birth). Appendix Fig. A.1 presents the main results with windows sizes of 30, 60 (the main specification), 90, and 120 days on each side of the cutoff. We find that increasing the window size only has a small effect on the estimated effects and does not change any of the main findings.

We want to check that our results are not driven by differences across the Danish Well-being Survey (DWS) waves. We do not add wave fixed effects in our main results, as this might take some of the variation out that we want to estimate. To control for systematic differences across DWS waves, we regress each outcome on indicators for each school/survey year and each grade level, only for those individuals who are born after the reform and who are "on time" in terms of their age and grade level. We then use the residuals (for all individuals) from these regressions as outcomes. We find that our main results do not change if we control for systematic differences across DWS waves. Results are displayed in Table A.1. Hence, such wave effects do not appear to be important.

FIGURE A.1: Main results by window size



Notes: This figure plots 2SLS point estimates from separate regressions with controls.

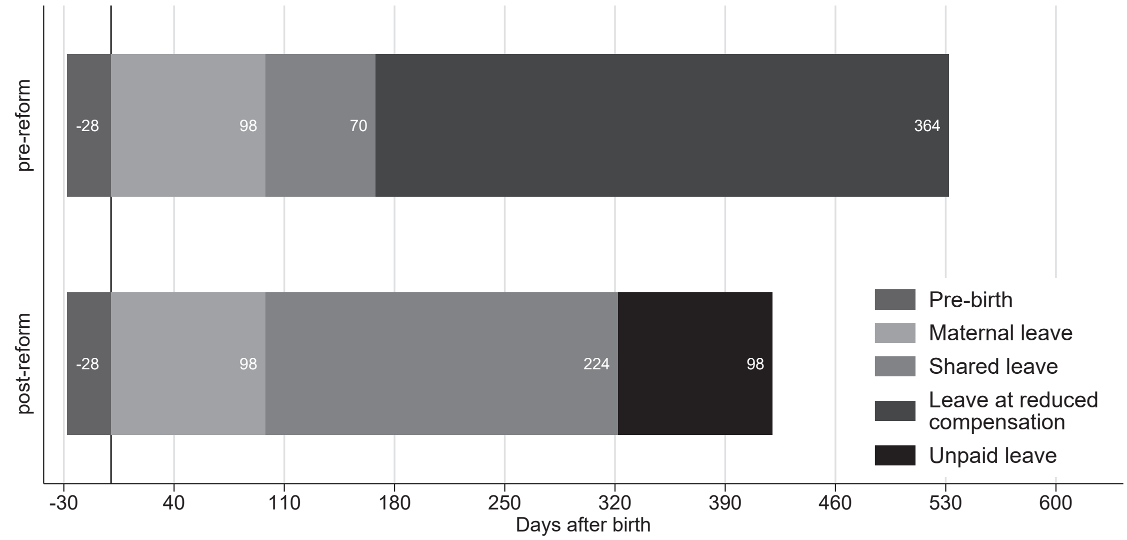
Table A.1: Main results, controlling for survey-wave effects

	(1)	(2)	(3)	(4)
Outcome:	Well-being	Conscientiousness	Agreeableness	Emotional Stability
Months of leave	0.041^{**}	0.029^{*}	0.008	0.029^{*}
	(0.017)	(0.017)	(0.016)	(0.017)
N	40236	40082	40167	39340

Notes: This table reports sensitivity analysis of the main results. We reproduce the main results where we additionally control for systematic differences across survey waves. We do this by regressing each outcome on indicators for each school/survey year and each grade level. This is done only for those individuals who are born after the reform and who are "on time" in terms of their age/grade level. We then use the residuals (for all individuals) from these regressions as outcomes. This shows that all of the estimates are very similar to the main results. Hence, such wave effects do not appear to be important. Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors are reported in parentheses and clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

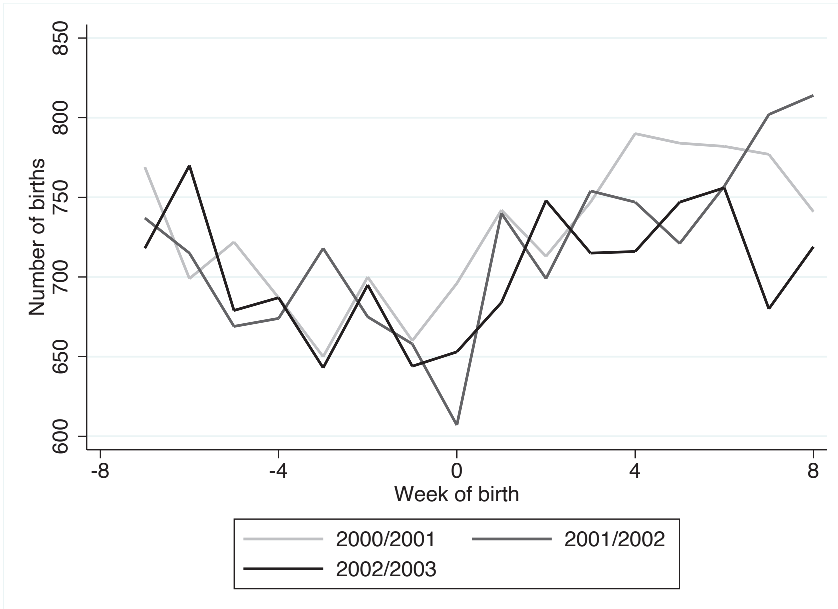
A.2 Figures and tables

FIGURE A.2: Parental leave policies



Notes: Overview of the pre- and post-reform policies in Denmark, 2001-2002. Pre-birth, maternal leave, and the shared leave are compensated at full leave benefit, equivalent to unemployment benefit. Many employees have contracts that raise the effective replacement rate to 100% for some part of the leave period. Leave at reduced compensation corresponds to 60% of the full leave benefit. In addition to the mothers' leave and the shared leave, there were 2 weeks of earmarked leave for fathers in the post-reform period (right after birth), and 4 weeks earmarked to fathers pre-reform (two weeks right after birth, and two weeks 25 weeks after the birth)

FIGURE A.3: Number of births by week of birth and cohort



Notes: This figure shows the number of births by cohort and week relative to New Years Day.

TABLE A.2: Participation in Well-being Survey by birth year, school start, and school grade

Birth year	2001		2002			2003		
School starting age	On time	Late	Early	On time	Late	Early	On time	Late
7th grade	2015	2016	2015	2016	2017	2016	2017	2018
8th grade	2016	2017	2016	2017	2018	2017	2018	2019

Note: Overview of the relationship between birth year, school starting age and observed survey year/grade level. The year 2015 refers to school years 2014/2015 and so on (DWS was administered in the 2nd half of the school year). Note that children born in November/December are already the youngest in their class from starting school on time. Therefore, they rarely start school early. Only about 0.1 percent of children take this option. Since for the year 2001, we are only interested in children born in December (the pre-reform children of the treated cohort), we disregard December-born early starters in 2001.

TABLE A.3: School starting age by time of birth

	Treated cohort		Control cohort	
	Nov/Dec 2001	Jan/Feb 2002	Nov/Dec 2002	Jan/Feb 2003
Early (6 years)	0.001	0.072	0.001	0.079
On time (7 years)	0.485	0.896	0.498	0.921
Late (8 years or older)	0.507	0.032	0.501	0.000
N	5959	5702	5423	5449

Notes: This table reports the share of children born in a particular year and month who start in the 1st grade in primary school at a particular age. The age refers to the age they turn in the calendar year where they start school. For example, a child born in December 2001 who starts 1st grade in August 2008 is on time, because the child turns 7 in that year.

TABLE A.4: First stage: Change in length of leave (in months) as a response to the reform

	(1) Sample mean	(2) RD (treatment) Pre-Post 1/1/2002	(3) RD (control) Pre-Post 1/1/2003	(4) RD DiD (3)-(2)	(5) p-value of (4)
Full sample	9.573	−1.655	0.020	−1.371	0.000
	(3.401)	(0.129)	(0.125)	(0.088)	
Maternal Income					
Income tercile 1	9.886	−1.928	−0.090	−1.464	0.000
	(3.227)	(0.226)	(0.187)	(0.145)	
Income tercile 2	9.766	−1.831	0.132	−1.426	0.000
	(3.446)	(0.217)	(0.224)	(0.155)	
Income tercile 3	9.067	−1.202	−0.0145	−1.212	0.000
	(3.468)	(0.221)	(0.225)	(0.156)	
Maternal Education					
Educ $<$ 12 years	9.813	−1.125	0.065	−0.997	0.000
	(3.270)	(0.361)	(0.271)	(0.228)	
Educ $=$ 12 years	9.609	−1.899	0.070	−1.528	0.000
	(3.475)	(0.184)	(0.187)	(0.131)	
Educ $>$ 12 years	9.427	−1.526	−0.119	−1.297	0.000
	(3.346)	(0.207)	(0.204)	(0.141)	
First born	9.362	−1.742	0.004	−1.411	0.000
	(3.129)	(0.177)	(0.173)	(0.123)	
Later born	9.734	−1.579	0.019	−1.324	0.000
	(3.591)	(0.184)	(0.179)	(0.125)	
Girls	9.569	−1.773	0.281	−1.387	0.000
	(3.346)	(0.186)	(0.172)	(0.124)	
Boys	9.577	−1.542	−0.213	−1.359	0.000
	(3.452)	(0.178)	(0.179)	(0.125)	
Short leave unlikely	9.763	−1.323	−0.223	−0.974	0.000
	(3.448)	(0.186)	(0.182)	(0.128)	
Short leave likely	9.385	−1.972	0.259	−1.764	0.000
	(3.343)	(0.176)	(0.171)	(0.121)	
N	22,755	11,692	11,063	22,755	

Notes: This table reports the first stage estimates in the full sample and across various subgroups. The first column displays means and standard deviations of the length of leave. Columns 2 and 3 displays the discontinuities in the length of leave at the cutoff for the treated and control cohort, respectively. Columns 4 and 5 displays the differences in the discontinuities between the two cohorts.

TABLE A.5: Descriptive statistics of main outcomes

	(1) Well-being	(2) Conscientiousness	(3) Agreeableness	(4) Emotional Stability	(5) Absenteeism	(6) 9th grade GPA
Full sample	0.015	0.047	0.017	0.023	0.056	0.036
Maternal Income						
Low income	−0.040	−0.031	−0.008	−0.010	0.061	−0.079
Middle income	0.015	0.011	−0.001	0.021	0.054	−0.026
High income	0.071	0.161	0.060	0.058	0.053	0.206
Maternal Education						
Short education	−0.152	−0.231	−0.119	−0.077	0.072	−0.359
Middle education	0.009	−0.015	−0.004	0.018	0.055	−0.074
Long education	0.084	0.226	0.093	0.066	0.050	0.310
First born	0.074	0.124	0.059	0.046	0.054	0.092
Later born	−0.033	−0.014	−0.017	0.003	0.057	−0.005
Girls	−0.032	−0.005	0.235	−0.143	0.055	0.178
Boys	0.060	0.097	−0.193	0.183	0.056	−0.109
Long leave	0.048	0.102	0.050	0.049	0.053	0.124
Short leave	−0.006	−0.011	−0.022	−0.001	0.058	−0.052

Notes: This table reports means, standard deviations for the main outcomes in the full sample and across various subgroups. All outcomes are standardized to have mean zero and a standard deviation of one in the full population, except for absenteeism, which is reported as a fraction of the total days in the school year.

Table A.6: Marginal effect on probability of taking short leave

Predictor	Level	Child/Mother $\frac{\partial Y}{\partial X}$	Child/Mother Std. Err.	Father $\frac{\partial Y}{\partial X}$	Father Std. Err.
Female		0.002	(0.007)		
Birth weight		0.002	(0.008)		
APGAR score		0.031^{***}	(0.007)		
Gestational age		0.003^{***}	(0.000)		
Birth order	2	0.005	(0.009)		
(Baseline: 1)	3	0.026^{**}	(0.013)		
	4	0.071^{***}	(0.024)		
	5	0.118^{**}	(0.052)		
	6	0.369^{***}	(0.113)		
	7+	0.242^{**}	(0.103)		
Immigrant		-0.061^{***}	(0.021)		
Age at birth		-0.007^{***}	(0.001)	-0.003^{***}	(0.001)
Income		0.00043^{***}	(0.00005)	-0.00013^{***}	(0.00003)
Years of education	8	0.038	(0.069)	0.042	(0.072)
(Baseline: 6)	9	0.111^{*}	(0.060)	0.026	(0.066)
	10	0.074	(0.060)	0.050	(0.066)
	12	0.097^{*}	(0.059)	-0.019	(0.065)
	14	0.157^{***}	(0.061)	-0.049	(0.066)
	15	0.165^{***}	(0.063)	-0.055	(0.071)
	16	-0.002	(0.060)	-0.015	(0.067)
	17	0.190^{***}	(0.062)	-0.023	(0.067)
	20	0.446^{***}	(0.090)	-0.087	(0.082)
Unemployment		-0.013	(0.025)	-0.033	(0.033)
N	16,930				

Notes: This table reports the average marginal effects and standard errors of the various child and maternal/paternal background characteristics on the probability of taking short parental leave. We predict leave taking by running a probit model for mothers giving birth pre-reform (i.e., in Nov/Dec 2001). We regress a dummy for short leave (24 weeks or less) on the full set of background characteristics. We allow for non-linearities by including a full set of dummy variables for educational level and birth order, as well as by including a squared term for age and income. We also include a set of two-digit sector code dummies (not shown).

Table A.7: Descriptive Statistics: Predicted pre-reform leave behavior

	(1) Unlikely to take short leave	(2) Likely to take short leave
Child characteristics		
Female	0.479	0.496
	(0.500)	(0.500)
Birth weight	3353.578	3587.849
	(808.598)	(596.279)
APGAR score	9.633	9.832
	(1.501)	(1.073)
Gestational age	268.874	280.137
	(39.091)	(20.775)
Birth order	1.747	1.790
	(0.819)	(0.958)
Immigrant	0.041	0.029
	(0.199)	(0.168)
Maternal characteristics		
Age at birth	31.312	29.326
	(3.808)	(4.948)
Years of education	13.613	12.507
	(2.614)	(2.526)
Income	221.057	226.170
	(79.263)	(114.226)
Unemployment	0.052	0.061
	(0.140)	(0.151)
Paternal characteristics		
Age at birth	33.804	31.240
	(5.261)	(6.048)
Years of education	13.183	11.815
	(2.709)	(3.016)
Income	351.413	274.601
	(273.733)	(580.089)
Unemployment	0.027	0.033
	(0.110)	(0.115)
N	11390	11365

Notes: This table reports covariate means and standard deviations separately by whether the mothers are predicted to have a likelihood of taking short leave above or below the median.

TABLE A.8: Heterogeneous effects: Gender

Outcome:	(1) Well-being	(2) Conscientiousness	(3) Agreeableness	(4) Emotional stability
Months of leave (Girls)	0.043^{***}	0.031^{*}	0.027	0.012
	(0.017)	(0.017)	(0.017)	(0.017)
Months of leave (Boys)	0.051^{***}	0.040^{**}	-0.016	0.044^{**}
	(0.017)	(0.017)	(0.383)	(0.017)
N	40,236	40,082	40,167	39,340

Notes: This table reports parameter estimates from 2SLS regressions estimating the effect of the length of maternity leave on non-cognitive skills in grades 7 and 8. Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors are reported in parentheses and clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.9: Heterogeneous effects: Birth order

Outcome:	(1) Well-being	(2) Conscientiousness	(3) Agreeableness	(4) Emotional stability
Months of leave (First born)	0.059***	0.017	-0.012	0.045**
	(0.021)	(0.022)	(0.021)	(0.021)
Months of leave (Later born)	0.026	0.018	-0.001	0.011
	(0.020)	(0.020)	(0.019)	(0.020)
N	40,236	40,082	40,167	39,340

Notes: This table reports parameter estimates from 2SLS regressions estimating the effect of the length of maternity leave on non-cognitive skills in grades 7 and 8. Control variables at child level: birth weight, APGAR score, gestational age, birth order, immigrant status, municipality fixed effects; and for mother and father: age at child birth, years of education, income and unemployment in the year before child birth. Standard errors are reported in parentheses and clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.