WORKING PAPER 2023:4

Gender-targeted transfers by default?

Evidence from a child allowance reform in Sweden

Erica Lindahl
Olof Rosenqvist
Håkan Selin

IFAU
INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala.

IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

Gender-targeted transfers by default?

Evidence from a child allowance reform in Sweden[a]

by

Erica Lindahl[b], Olof Rosenqvist[c] and Håkan Selin[d]

March 01, 2023

Abstract

We exploit a sharp birthday discontinuity in a large and universal Swedish cash transfer program, creating plausibly exogenous variation in the default disbursement option, while holding entitlements and other financial incentives constant. When the cash transfer is paid out to the mother by default, instead of a 50/50 default, it has a huge effect on the probability that the transfer is deposited in the mother's bank account also in the long run. Surprisingly, we find that the default policy redistributes resources to separated low-income mothers. We find no indications that the 100%-to-the-mother default induces mothers to work less or to take more responsibility for the children.

Keywords: Gender targeting, family transfers, default, child allowance, gender equality
JEL-codes: J12; H31; D91

[a] We thank Martin Lundin, Sara Roman, two anonymous referees and seminar participants at the Institute for Evaluation of Labour Market and Education Policy (IFAU) in Uppsala, the 2022 EALE conference in Padova, and the 2022 Swedish Conference in Economics for valuable comments. We are grateful for financial support from FORTE, grant number 2019-01202.
[b] Lindahl: IFAU and Uppsala Center for Labour Studies (UCLS)
[c] Rosenqvist: IFAU and UCLS. Corresponding author: olof.rosenqvist@ifau.uu.se
[d] Selin: IFAU, Uppsala Center for Fiscal Studies (UCFS) and UCLS

1 Introduction

Financial support to families with children comes in many appearances: tax credits, deductions, conditional and unconditional cash transfers. The primary aim of child benefits is to promote the well-being of children, but it is also widely acknowledged that the design matters for female empowerment (Duflo 2012; Woolley 2004; Fiszbein et al. 2009). A common policy view is that transfers to families with children should be targeted to mothers, because control over money strengthens the economic independence of mothers, and, additionally, leads to more investments in children.[1] Recently, Almås et al. (2018) showed that poor married mothers indeed are willing to accept a lower total transfer to the family in exchange for controlling the money. But gender targeted transfers can also be criticized from a gender equality perspective. The argument goes that mothers and fathers share responsibility for their children and, therefore, both should receive child benefits as a signaling device.

The latter line of reasoning was central when Sweden in 2014 went from a gender-targeted to a gender-neutral system of *child allowances*, which is the most important cash-transfer to families with children in Sweden.[2] Conditional on the number of children, the allowance is a universal lump-sum transfer (on a monthly basis) independent of the parents' income, wealth, and other characteristics. Public spending on this transfer program is considerable, and especially for low-income single mothers, the monthly allowance is an important source of income.

Before the reform, up to March 1, 2014, mothers (under normal circumstances) were entitled to the entire child allowance. In the new gender-neutral system, parents are equally entitled to the allowance, and the default disbursement option is a 50/50-split between the mother and the father. The transition from the old to the new system created a unique quasi-experimental setting where the *default disbursement option* depended on the child's birth date (before/after March 1, 2014). Mothers of children born prior to March 1 were entitled to the entire child allowance up to this date, and these mothers therefore by default received the first allowance(s). By contrast, mothers of children born after March 1 instead received 50% of the allowance by default. The entitlement structure after March 1 was the same irrespectively of whether the child was born before or after the reform: parents who were living together were entitled to 50% each of the child allowance,

[1] Positive effects on children from gender targeting has some empirical support, see, e.g., Armand et al. (2020) for effects of targeting cash transfers to mothers or fathers on household expenditure on non-durables, Attanasio and Lechene (2014) for effects of the PROGRESA program in MEXICO, Duflo (2003) for effects of the design of the South African social pension programs and (Lundberg, Pollak, and Wales 1997), (Ward-Batts 2008) and Hotchkiss (2005) for effects of the shift in the recipient of the child allowance in UK during the 1970s. However, results from a recent large-scale field experiment of gender targeting of unconditional cash transfers in India suggest that female empowerment is the most relevant margin of response (Somville, Almås, and Vandewalle 2020).

[2] Low-income families with children are also eligible for means-tested benefits (housing allowances, social assistance).

and a lone parent was entitled to 100% of the allowance if the child primarily lived with him/her. Parent couples with a 50/50-entitlement were free to actively change benefit recipient whenever they desired. Other important features of the choice environment, like the total benefit level, were held constant. Thus, after March 1, the only difference between the two groups lies in the default benefit recipient.

The purpose of this paper is to estimate the causal effect of the default transfer recipient on the long-term division of child transfers and other outcomes related to gender equality in a setting where entitlements are held constant. We consider the 100%-to-the-mother default as the treatment or the *nudge*, which we evaluate relative to the 50/50-default.[3] The purpose of the paper is, hence, not to evaluate the reform *per se.*[4] Instead, we evaluate a hypothetical policy that offers a compromise between the traditional gender-targeted system and the completely gender-neutral system that is currently in place in Sweden. We have a special focus on separated and on lone parents (when the child lives with only one of the parents): in those cases, the division of child allowances is more likely a high-stake choice.[5]

Firstly, we study the causal effect of the 100%-to-the-mother default, relative to the 50/50 default, on actual long-run disbursements in *the entire population of parent couples*. We find that the 100%-to-the-mother default had a huge and persistent impact on child allowance disbursements. One year after childbirth, the share of parental couples allocating 100 percent of the transfer to the mother was 55 percentage points higher when exposed to the 100%-to-the-mother default. The "power of default", caused by individuals' tendency to remain passive, has earlier been documented in several other contexts, e.g., retirement savings (Madrian and Shea 2001) and organ donations (Johnson and Goldstein 2003), but never in the context of family cash transfers. Note, however, that a shift in disbursement from the father to the mother does not necessarily imply a change in the two parents' economic conditions, because parents can always transfer money between them, and may hold joint bank accounts. Even though previous studies suggest that Swedish couples do not perfectly pool their incomes (Cesarini et al. 2017; Gelber 2014), it is in general difficult to tell to what extent the reallocation of disbursements represents a real resource transfer to women.

[3] All our results can of course also be interpreted the other way around: as the treatment effect of 50/50-default relative to the 100%-to-the-mother default. However, we consider the 50/50-default with 50/50-entitlement as a more natural benchmark, because in the 100%-to-the-mother group the default option and entitlement differ, and the treatment effect thus illuminates the default effect.

[4] Note that the reform both changed entitlements and default options.

[5] There is probably a low cost of changing payment rule for couples that stay together. However, if they separate, there could be a psychological cost of arguing with your ex-partner. For this reason, we pay extra attention to separated parents.

Secondly, we estimate the effect of the default option on *the population of separated parents*.[6] These parent couples are arguably more interesting than non-separated couples, because they are unlikely to pool their incomes. We find that the default option had a significant effect on divorced parents at all income levels, including low-income parents for whom the child allowance constitutes a significant share of the disposable income.[7] These findings suggest that the 100%-to-the-mother default shifted resources from separated fathers to separated mothers, also when stakes were high. The salient response among separated couples indicates that there are substantial costs (e.g. psychological) of claiming the allowance at the expense of the ex-partner.

Thirdly, we estimate the effect of the default option on a particularly interesting subgroup, namely separated parent couples where the mother receives child support payments from the Swedish Social Insurance Agency (SSIA). In this subsample, where we can be sure that the child solely lives with the mother, mothers are fully entitled to the entire child allowance, and the allowance is important to their living standards. Still, we document a causal effect from the default option on disbursements.[8] The share of mothers receiving 100% of the allowance increases from just above 90% under the 50/50-split default to 100% under the 100%-to-the-mother default. Hence, almost 10% of mothers fail to claim the allowance they are entitled to when they only receive 50% by default. One potential explanation is that eligible mothers are reluctant to upset their ex-partners. Another factor could be that mothers confuse the initial disbursement with their entitlement. In any case, this result highlights that the default option is far from being a curiosity in this context.

Returning to the entire population of parents, we show that a substantial fraction (40 percent) of parents of new-born children in the 50/50-regime choose to actively redirect the entire allowance to the mother. Active parents are on average more gender traditional, and they have lower income and education than parents who follow the default of an equal split. For example, mothers in active couples take the lion's share of the paid parental leave. This evidence is in line with the results presented in Almås et al. (2018) showing that especially low-income mothers prefer controlling the transfer.

Finally, using our birthday discontinuity, we estimate the *causal* impact of the default on outcomes related to gender equality. An explicit intention of the 2014 reform, including the 50/50-default, was to signal shared responsibilities for the children.[9] However, exploring the exogenous

[6] In Swedish registers we can observe if parents who have children together have the same residential address or not. If they do not live at the same address, we interpret them as being separated.
[7] The analysis is simplified by the fact that the reform had no significant impact on the probability of living together.
[8] Once more, we have verified that there is no effect of the default treatment on the probability of appearing in this subpopulation.
[9] That both parents by default are recipients of the child allowance signals their common responsibility for the child according to the bill proposed by the government: Prop. 2013/14:6, p.48.

variation in default disbursement, we find no significant causal effects on the division of earnings, temporary paid parental leave, and regular paid parental leave.[10] The lack of causal effects is consistent with the recent analysis of Armand et al. (2020) who also found zero effects on time allocation outcomes of introducing gender targeting of conditional cash transfers in the Republic of North Macedonia.

The paper unfolds as follows. In the next section, we present the institutional setting and data. In section 3, we outline our empirical strategies and in section 4 we analyze the effects of the default option on the long-term allocation of child allowance disbursements. In section 5, we focus on outcomes related to gender equality, and section 6 concludes the paper.

2 Institutional setting and data

2.1 Child allowances in Sweden

In Sweden, the modern system of child allowances was first implemented in 1947. It is a universal benefit that is not means- or income tested. It is disbursed to all families with children eligible for social insurance. The child allowance currently amounts to about 100 EURO per child and month until the child turns 18 years old (or finish secondary school).[11] Families with two or more children also receive a supplementary multi-child allowance, the size of which varies with the number of children.[12] The child allowance and the supplementary multi-child allowance are administered and paid out by the Swedish Social Insurance Agency.

For the average household with children, the income from child allowances amounts to 4.2% of household disposable income (Försäkringskassan 2020). However, this share varies substantially by deciles of household disposable income. While only around 2% in the top decile, it amounts to almost 16% in the bottom decile. Income from child allowances is generally also more important for single parents.

2.2 The 2014 child allowance reform

Prior to March 1, 2014, Swedish mothers had a strong say in child allowance matters. Mothers were entitled to the entire child allowance when parents were living together. Moreover, mothers were also entitled to the entire allowance if the two parents separated and the child was living at

[10] Temporary parental leave is a benefit parents can use to stay at home and take care of an ill child (that cannot be in school or in daycare) instead of going to work. Regular paid parental leave is a benefit parents can use to stay at home with their children. It is typically used when children are small (before they start going to daycare), but can also be used later on, e.g. when daycare is closed and in similar situations.

[11] When the child turns 16, the benefit is transformed to a study grant and it is only disbursed if the child is registered at secondary school, which most children in Sweden are, although school is only mandatory until the age of 16.

[12] For a two-child family, which is the most common family-type, the supplementary multi-child allowance is around 14 EURO per month.

both parents' homes. The child allowance payments could only be split 50/50 between the parents if they were separated and the mother agreed on the split. If the mother agreed, however, the allowance could be redirected to the father's account (but could at any time be reclaimed by the mother). In 2013, one year before the reform, 93 percent of the child allowances were paid to mothers, 5 percent were paid to fathers, and only 2 percent were split between parents (ISF 2016).

The new rules that came into effect on March 1, 2014, meant that the father became entitled to 50% of the child allowance, unless the child was dwelling solely with the mother. This entitlement change applied to *all* parent couples, regardless of whether the child was born before or after March 1.[13] However, fathers of children born prior to March 1 needed to *actively* apply for their share of the allowance. If they remained passive, the entire allowance continued to be disbursed to the mother. Note that fathers could change the disbursement option from a 100%-to-the-mother allocation to a 50/50 allocation unilaterally, while changes from a 50/50 allocation to a 100% allocation (to the mother or father) required mutual consent. The transition from the old to the new system created a quasi-experimental setting where the default disbursement option depended on the child's birth date (before/after March 1, 2014). Parents who had children just before and just after the cutoff differ only with respect to the default disbursement option (the entire allowance to the mother or a 50/50 split), while the entitlement structure was held constant. In practice, framing and information on the possibility to actively switch transfer recipient are likely determinants of the response to the default option. As we will discuss further in Section 4.2, married parents were provided with more salient information on how to switch compared with unmarried parents.

2.3 Data

The analysis is based on Swedish universal registers on all parents who had children around the reform, i.e., around March 1, 2014. For some analyses, we also use parents of children born around the March cutoff in 2010–2013.

Parents are linked to their children through a multi-generational register including all births in Sweden. We have individual level information on when the child is born (exact birth date) as well as rich background information about the parents. We can observe if the parents were married or unmarried at the time of birth. At the end of each year, we can see if the parents are separated or living together based on information on whether they are registered at the same residential address or not.

[13] For all parents who give birth in the new regime (after March 1, 2014), the 50/50 split is the policy for the new child, irrespectively of the order of the birth. However, payments for older children (born before the reform) will not automatically be split just because the parents have a new child in the new system. In that sense, the rules are child-specific.

The *outcome variables* of interest are how the child allowance is disbursed, and proxies for the division of child-related duties within the family as well as within household labour supply decisions. Regarding the disbursement of the child allowance, we have detailed information on each single payment: exact amount paid to whom and when. As proxies for the within-couple division of child-related duties, we use individual register information about earnings, and the use of paid parental leave and paid temporary parental leave.[14] We have individual spell data on these benefits from which we calculate the number of benefit days used per parent.

3 Empirical strategy

In this section, we present the two empirical models used in the analysis.

3.1 A regression discontinuity model

Since there was a sharp change in the default disbursement option of the child allowance on March 1, 2014, the setting is suitable for estimating the causal effect of letting the mother be the default recipient, relative to a 50/50 split, on the long-term division of the child allowance (and other outcomes) by means of a regression discontinuity design. We estimate the model in Equation (1) on a sample of children born in January–April 2014.

$$Y_i = \beta_0 + \beta_1 1(Def. = mother)_i + \beta_2 BD_i + \beta_3 BD_i * 1(Def. = mother)_i + \varepsilon_i \qquad (1)$$

We regress the chosen disbursement option of the child allowance (or some other outcome) for child i on a dummy that equals 1 if the default is 100%-to-the-mother, i.e., the child was born before March 1, 2014, zero otherwise. The model also includes a linear control for the exact birth date (BD) of the child, normalized by the cutoff date, and the linear relation to the outcome is allowed to differ by default status. We use a uniform kernel and robust standard errors in the main specification.[15]

The effect of the 100%-to-the-mother default on the long-run division of the child allowance disbursement (or other outcomes) is causally identified using Equation (1) under the assumption that births occurred randomly around March 1, 2014. While it has been found that parents, in some contexts, are timing their births depending on access to more generous child-related benefits

[14] Temporary parental leave is a benefit parents can use to stay at home and take care of an ill child (that cannot be in school or in daycare) instead of going to work. Regular paid parental leave is a benefit parents can use to stay at home with their children. It is typically used when children are small (before they start going to daycare), but can also be used later on, e.g. when daycare is closed and in similar situations. Eriksson and Nermo (2010) show that the use of paid temporary parental leave is a good proxy for the gender division of household work.

[15] We follow Kolesár and Rothe (2018) for the choice of standard errors. Our results are not sensitive to varying the bandwidth, using a triangular instead of a uniform kernel or using standard errors clustered at the birth date level (see Table 1).

(Neugart and Ohlsson 2013; Borra, González, and Sevilla 2016), we are not worried about such incentives in this case since entitlements and benefit levels were the same on both sides of the cutoff. Still, a careful investigation of the assumption is of course warranted. We investigate the matter by checking for discontinuities in predetermined parental characteristics and number of births at the cutoff. The results are presented in Appendix B. The most important takeaways are: March 1, 2014, was a Saturday and planned birth-interventions (e.g., C-sections) are unlikely to be scheduled for weekends. Therefore, it is not surprising to see a drop in the number of births at the cutoff in panel A of Figure B1. The drop causes the McCrary density test (McCrary 2008) to be significant, but this result does not suggest manipulation considering the sluggish appearance of the frequency distribution. Frandsen (2017) suggests another test when the running variable is discrete, which essentially compares the jump at the cutoff with other jumps in the distribution. When applying this test, we get an insignificant result with a p-value of 0.262.

In Table B1 in the appendix, we present estimates of β_1 from Equation (1) using predetermined parental characteristics as outcome variables. We consider average parental characteristics in the household in terms of age at birth, prebirth education and income, and foreign vs native background. While the effect on income has a p-value of just below 0.10, the overall results suggest that we cannot reject the null hypothesis of no discontinuities at the cutoff.

3.2 Robustness: A difference-in-differences model

To improve statistical precision (which is important for heterogeneity analyses), and to address the potential concern of general turn-of-the-month effects, we use a difference-in-differences (DiD) approach as an alternative empirical strategy.[16] By comparing differences in outcomes between parents of children born in February and March 2014 to corresponding differences in the preceding years, we net out permanent differences between parents of children born in February and March. We estimate the following model on a sample of children born in February–March in the years 2010–2014:

$$Y_{iym} = \theta_y + \rho_m + \sum_{j=2010, j\neq 2013}^{2014} \alpha_{j-2010} 1(y = j)_i * 1(m = February)_i + v_{iym} \quad (2)$$

Using 2013 as the reference year, the specification includes year (y) fixed effects, month (m) fixed effects and interactions between the year dummies and the child being born in February. The parameter of interest is α_4, which captures the extent to which differences in outcomes between

[16] Possible reasons for turn-of-the-month effects could be admission policies to childcare centers, and/or informal policies regarding when parents return to work after parental leave.

parents of children born in February and March 2014 are different from corresponding differences in 2013. While α_4 captures the effect of the 100%-to-the-mother default, the "placebo coefficients" α_0–α_2 are informative about the stability of the February-March differences across years in the pre-reform period.

In figures C1–C7 in the appendix, we show that estimates of α_4 consistently are insignificant when Equation (2) is applied to the predetermined parental characteristics that we previously used (presented in Table B1 in the appendix) to examine the validity of the regression discontinuity design (i.e., parents' average prebirth schooling and income, age and likelihood of being Swedish-born). The pre-reform estimates are also largely insignificant. Thus, the tests support the validity of the DiD design.

4 Default effects on the long-run allocation of child allowances

In this section, we present results on how the 100%-to-the-mother default, relative to the 50/50 default, affected the long-run disbursement of the child allowance in the households. We show effects using two empirical models: regression discontinuity and difference-in-differences.

4.1 All parents

Figure 1 plots the share of mothers receiving the entire child allowance at the 12th payment by birth date for all children born in January–April 2014. The sharp shift in the share of parents allocating 100% to the mother at March 1 simply corresponds to β_1 in Equation (1), i.e. the effect of the 100%-to-the-mother default relative to the 50/50 default.[17] The RD-estimate and the corresponding DiD-estimate are printed in the figure and both amount to around 55 percentage points. As can be seen in Table 1, where we vary the specification of the RD-model and perform a donut exercise (excluding children born within one week of the cutoff), the RD-estimate is very robust. Moreover, the DiD-model displays insignificant placebo estimates (see Table A1 in the Appendix).

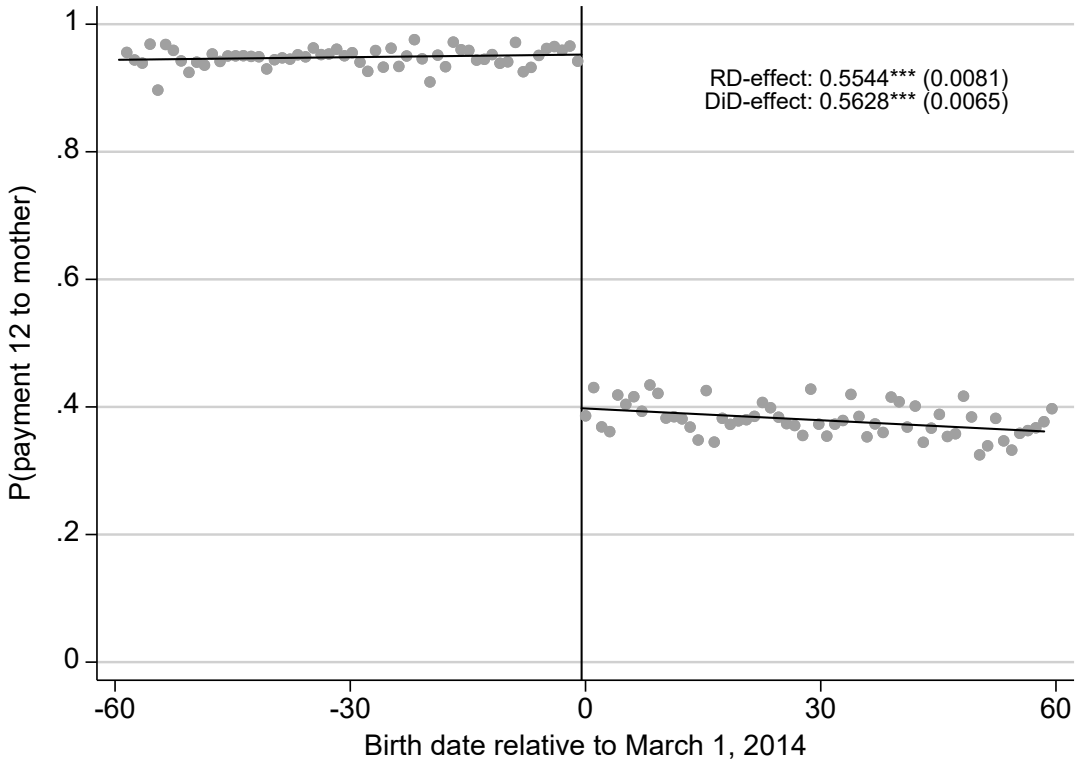
The interpretation is that letting the mother be the default recipient, rather than sharing the child allowance by default, increases the long-run probability of the mother being the sole recipient by about 55 percentage points. The outcome is measured approximately 1 year after the birth of the child, which means that parents had time to change the division of the child allowance.[18] Columns 1–2 in Table A2 in the Appendix further shows that the increase in the

[17] Note that the 100%-to-the-mother default treatment is turned on when the vertical line is crossed from the right to the left.

[18] When the corresponding analysis is done for payment 72, when the child is six years old, the effect is slightly smaller at approximately 49 percentage points (see column 7 of Table 1).

share of mothers receiving the entire allowance is accompanied by a corresponding decrease in the share of couples splitting the allowance in the long run ("Mother 50 %"), while the share of fathers receiving the entire allowance remains largely unaffected ("Mother 0 %"). Consequently, the effect of the 100%-to-the-mother default on the mother's share of the child allowance disbursement becomes roughly half of the effect on the probability that the mother is the sole recipient.

Figure 1 Effect of the 100%-to-the-mother default on child allowance division among all parent couples



Note: The figure is based on all parents of children born between January and April 2014. The outcome takes the value 1 if the mother received the entire child allowance at the 12th payment. The printed RD-effect is an estimate of β_1 from Equation (1) and represents the difference between the left- and the right-hand side intercepts. The printed DiD-effect is an estimate of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level.

Table 1 RD-effects on the child allowance going to the mother at payment 12 – model variations

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample:				All			
Outcome:				Mother receives 100% of the allowance			
Model:	Baseline	Triangular kernel	SE clustered on birthdate	Bandwidth 30 days	Bandwidth 90 days	1-week donut	Payment 72
RD-effect	0.5544***	0.5556***	0.5544***	0.5565***	0.5642***	0.5528***	0.4864***
	(0.0081)	(0.0089)	(0.0074)	(0.0115)	(0.0066)	(0.0105)	(0.0088)
Mean	0.3874	0.3874	0.3874	0.3874	0.3874	0.3874	0.4097
Obs.	33,981	33,981	33,981	17,349	50,393	29,856	33,943

Note: The RD-effect represents estimates of β_1 from Equation (1). Robust standard errors are in parentheses (except for column 3) and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. In the baseline specification we use a uniform kernel, robust standard errors and a bandwidth of 60 days. In columns 1–6, the outcome takes the value 1 if the mother received the entire child allowance at the 12th payment. In column 7, the outcome takes the value 1 if the mother received the entire child allowance at the 72nd payment.

4.2 Married vs. unmarried couples

In a secularized country liked Sweden, it is common that couples with children live together without being married. About 55% of the couples in our data were unmarried at birth. From a legal viewpoint, the most relevant difference between being married or unmarried is that the former couples, at least in the absence of prenuptial agreements, split their assets equally after a divorce. Moreover, married spouses inherit each other by default. By contrast, cohabiting unmarried couples only split household goods and furniture after a separation, and bequests to the partner must be regulated in wills. Accordingly, married couples are likely to have more integrated economies. In Figure 2, we visualize the responses of married and unmarried parents, respectively. Among married parents, the 100%-to-the-mother default increased the likelihood that the mother is the sole recipient of the child allowance at the 12th payment by about 36 percentage points. The corresponding effect among unmarried parents is about twice as large.[19]

Important to note, however, is that we cannot attribute the differential responses of married and unmarried parents to economic fundamentals, because the information about the possibility of actively changing the default disbursement varied across the two groups. Married parents have joint custody from birth, and they therefore received letters shortly after the birth with information on the possibility of choosing child allowance recipient as well as an enclosed form for that purpose. Married parents got the same information on both sides of the March 1 cutoff. Unmarried parents who had their child just before March 1 received the same type of information, while unmarried parents of children born just after March 1 received a letter in which information on how to switch recipient was less salient.[20]

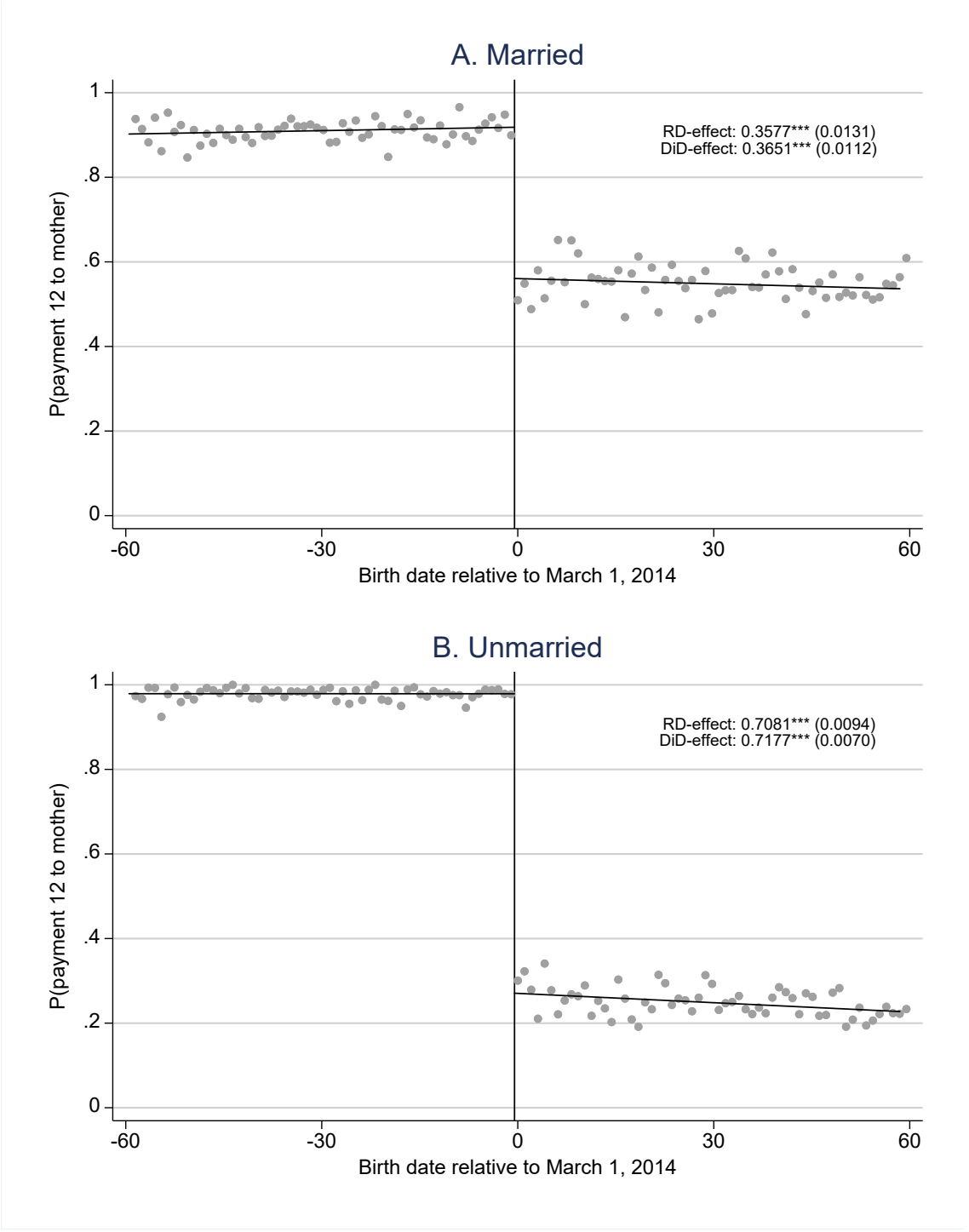
[19] See also columns 3–6 in Table A2 in the Appendix for the outcomes "Mother 50%", "Mother 0%" and "Mother's share" at the 12th payment.

[20] The letter received by unmarried parents to children born just after March 1 simply stated that 50% of the allowance was going to be paid to each parent from the next payment, and there was only a small second-page note about the possibility of actively changing disbursement option. In contrast, the letters sent to married and unmarried couples to

These differences in the choice architecture, originating from new and premature administrative routines at the Swedish Social Insurance Agency, are likely to explain substantial parts of the heterogeneity across married and unmarried parents. In the rest of the paper, we will pool married and unmarried parents, to gain statistical precision, and focus on other subgroups of the population. We will distinguish between couples who are living together (regardless of marital status), and separated couples (also regardless of initial marital status).

the left of the cutoff contained an enclosed form for choosing recipient. This form could, however, easily be downloaded and printed from the webpage of the Swedish Social Insurance Agency.

Figure 2 Effects of the 100%-to-the-mother default on child allowance division among married and unmarried parent couples



Note: Panel A (B) is based on married (unmarried) parents of children born in the period January–April 2014. The outcome takes the value 1 if the mother received the entire child allowance at the 12th payment. The printed RD-effect is an estimate of β_1 from Equation (1) and represents the difference between the left- and the right-hand side intercepts. The printed DiD-effect is an estimate of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level.

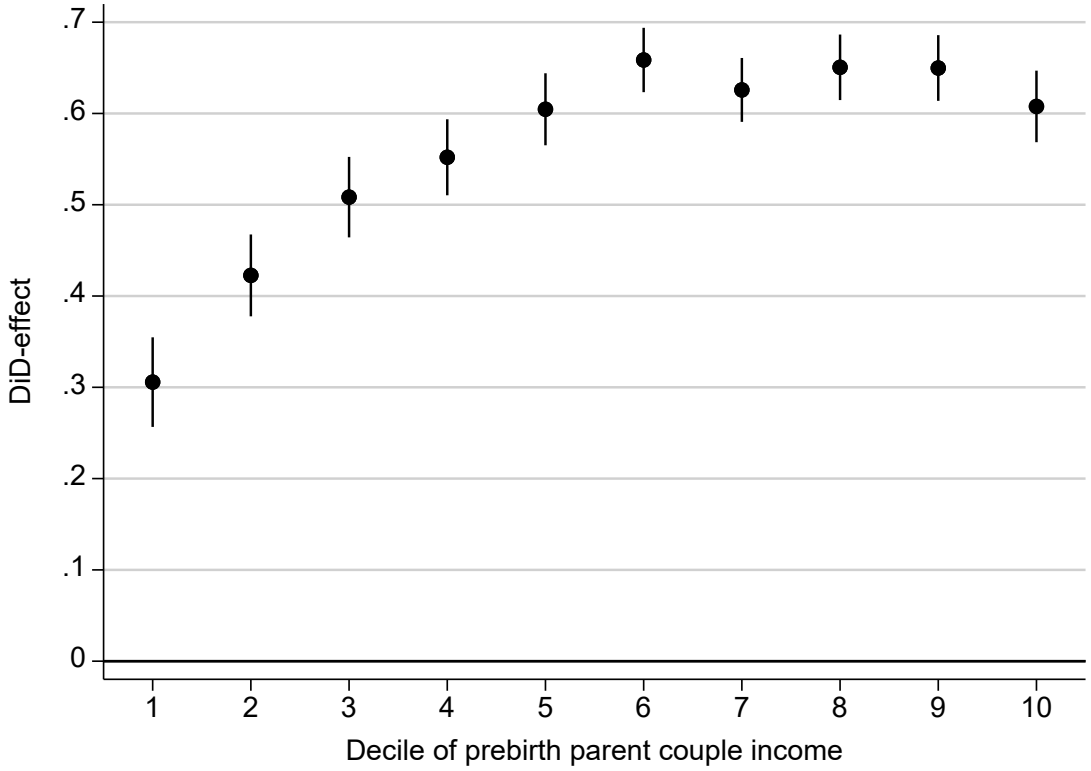
4.3 Responses across the income distribution

The child allowance, of course, constitutes a larger share of the household income among lower-income parents. For these parents, the disbursement of the benefit may be more important, and the 100%-to-the-mother default thus less powerful. Figure 3 presents the 100%-to-the-mother default effect on the probability that the mother is the sole recipient of the allowance at the 12th payment along the deciles of the prebirth parent-couple income distribution.[21] To improve precision, we use the difference-in-differences model in Equation (2) on the pooled sample of married and unmarried parents in this analysis.

As predicted, the effect of the 100%-to-the-mother default on the probability that the mother is the sole recipient of the allowance at the 12th payment is smaller among low-income parents. The effect in the lowest decile is around 30 percentage points, which can be compared to the average effect of just above 55 percentage points. The effect gradually grows over the deciles below the median reaching 60 percentage points at decile 5, and then remains at about that level for all deciles above the median.[22] The smaller effect of the 100%-to-the-mother default in the lower deciles probably reflects that mothers in these couples have strong preferences for controlling the benefit, in line with the results in Almås et al. (2018).

[21] Prebirth parent-couple income is here the sum of the parents' income the calendar year before the birth.
[22] See Table A3 in the appendix for the printed DiD-estimates as well as the corresponding RD-estimates.

Figure 3 Effects of the 100%-to-the-mother default on child allowance division by prebirth parent couple income decile (DiD-estimates)



Note: The figure shows estimates of α_4 from Equation (2) with 95 % confidence intervals for different deciles of the prebirth parent couple income distribution. The outcome takes the value 1 if the mother received the entire child allowance at the 12th payment. All parents of children born in February–March in the years 2010–2014 are included in the analysis.

4.4 Parents living together vs. separated couples

The disbursement of the child allowance may play a minor role as long as a couple stays together. We cannot observe how parents transfer resources within the household, but after a separation it is reasonable to assume that parents do not pool their incomes. Thus, any effect of the 100%-to-the-mother default on child allowance disbursements after separation could more convincingly be interpreted as being a real effect of the policy. In this section, we therefore estimate our empirical models separately for parents who are living together or not at the end of the child's third calendar year.[23] This implies that we condition on a potential outcome (separation could be affected by the default recipient).[24] Thus, we start by carefully investigating if the 100%-to-the-mother default has an impact on the probability of living together. Table 2 reports effects of the 100%-to-the-

[23] Note, the distinction is between living together or not, irrespectively of marital status.
[24] Policies for increased gender equality, in the form of reserved paid parental leave for the father, have previously been found to affect household dynamics in terms of separations rates (Avdic and Karimi 2018; Olafsson and Steingrimsdottir 2020). Using cross-country variation, González (2007) shows there is a small but significant effect of benefit generosity (defined as social assistance payments and/or family-related allowances) on the incidence of single motherhood.

mother default on the probability that the parents are living together at different points in time after childbirth. Overall, the results suggest that the separation margin is unaffected by the default. We have also estimated effects on the probability of living together at the end of the child's third calendar year by deciles of the mother's share of the prebirth parent couple income, motivated by the notion that the default recipient might matter more for separation choices in the lower deciles, but there are no indications of important heterogeneity across the different deciles (see Figure A1).

Table 2 Effects of the 100%-to-the-mother default on the probability of living together at year 0–5

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample:	All	All	All	All	All	All	All
Outcome:	Together year 0	Together year 1	Together year 2	Together year 3	Together year 4	Together year 5	Support sample
RD-effect	-0.0020	-0.0047	-0.0114*	-0.0058	-0.0010	0.0026	0.0003
	(0.0057)	(0.0062)	(0.0068)	(0.0074)	(0.0079)	(0.0084)	(0.0025)
DiD-effect	-0.0038	-0.0020	-0.0085	-0.0042	0.0008	0.0041	0.0001
	(0.0057)	(0.0063)	(0.0068)	(0.0074)	(0.0079)	(0.0084)	(0.0025)
Mean	0.9255	0.9120	0.8931	0.8701	0.8419	0.8185	0.0121
Observations (RD)	34,106	34,106	34,106	34,106	34,106	34,106	34,106
Observations (DiD)	85,553	85,553	85,553	85,553	85,553	85,553	85,553

Note: The RD-effect represents estimates of β_1 from Equation (1). The DiD-effect represents estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. In section 4.5, we elaborate on the purpose and construction of the variable "Support sample", which is a subset of the sample of separated couples, for which we observe entitlements to child allowances.

Before starting the analysis, we note that the 50/50 entitlement rule does not always apply to separated parents. The parent who provides the child's primary home is always entitled to the entire child allowance. If the child stays with the mother and the father to roughly the same extent, the parents are entitled to 50% each (the same as when they live together).[25] Unfortunately, we cannot observe the child's living arrangement in our data. Thus, child allowance entitlements among separated parents are generally unknown to us at the individual level.[26] However, from public statistics we know that after a separation the mother is the primary home provider in about 60% of cases, the father in about 15% and in the remaining part there is a 50/50 living arrangement (SOU 2011). We also know that the 50/50 living arrangement is less common among parents with lower socioeconomic status (SOU 2011; Fransson, Bergström, and Hjern 2015). Thus, while all parents who are living together have a 50/50 entitlement, it is relatively common that the mother is entitled to the entire child allowance in the sample of separated parents. However, we have no

[25] The parents do not always agree on the characterization of the living arrangement of the child. When there are disputes, the SSIA investigates the matter and decides who is entitled to the child allowance. Still, the entitled parent needs to actively claim the allowance if he or she does not already receive it.

[26] In specific cases, we can deduce the living arrangement of the child from other benefits paid to the parents. We return to this issue in subsection 4.5.

reason to believe that the 100%-to-the-mother default affected the living arrangements of children of separated parents, i.e., we expect entitlements to be balanced at the default cutoff for separated parents.

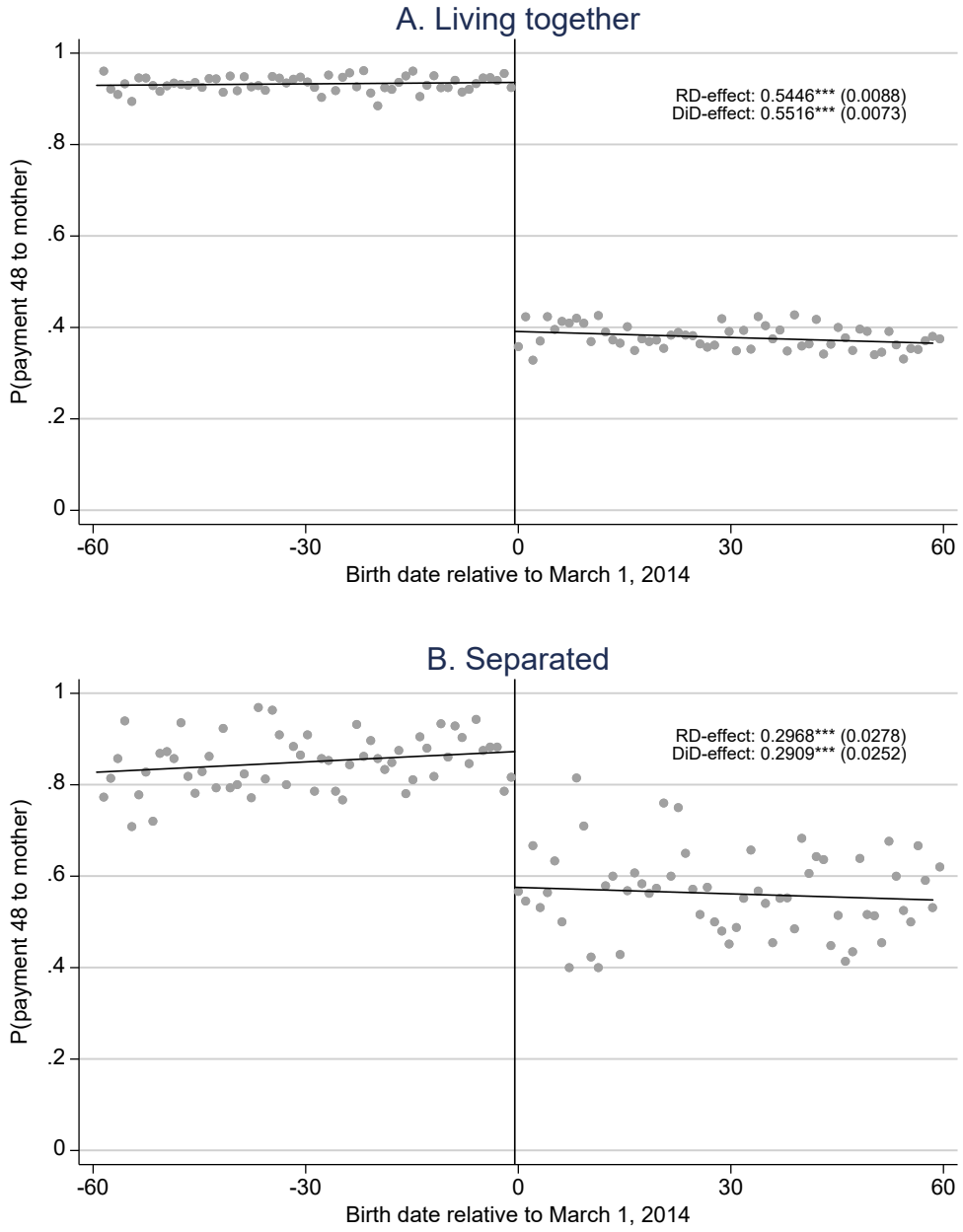
In Figure 4, we present results from estimating the regression discontinuity model of Equation (1) and the difference-in-differences model of Equation (2) on an outcome that takes the value 1 if the mother received the entire child allowance at the 48th payment, i.e., four years after childbirth. The outcome is evaluated at a later point (48th instead of the 12th payment), to make sure that the observation of the child allowance allocation takes place well after the observation of the separation status (which we observe around payment 34–35).

We find about the same estimates from both empirical models.[27] For parents who are living together, the 100%-to-the-mother default causes an increase in the probability that the mother is the sole recipient of the 48th payment of 55 percentage points, while the corresponding effect among separated parent couples is around 30 percentage points.[28] The smaller default effect for separated parents is expected since they have stronger incentives to claim their entitlements now that they are in separate households, but the effect is still surprisingly large, suggesting that passive behavior is important also in this group. One potential explanation for this passive behavior could be that individuals feel uneasy about making claims at the expense of the ex-partner.

[27] See also Table A4 in the Appendix for the outcomes "Mother 50%", "Mother 0%" and "Mother's share" at the 48th payment.

[28] In Appendix D, we present suggestive evidence on the entitlement effect, which relies on much stronger identifying assumptions than the default analysis. Interestingly, the huge default effect of parents who are living together can be contrasted with an entitlement effect that appears to be close to zero in the same group. Hence, for a given default option, it does not matter for the division of child allowances if the two parents are equally entitled or not.

Figure 4 Effects of the 100%-to-the-mother default on child allowance division among non-separated and separated parent couples



Note: Panel A (B) is based on parents of children born between January–April 2014 and who were living together (separated) at the end of 2016. The outcome takes the value 1 if the mother received the entire child allowance at the 48th payment (paid out in the beginning of 2018). The printed RD-effect is an estimate of β_1 from Equation (1) and represents the difference between the left- and right-hand side intercepts. The printed DiD-effect is an estimate of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level.

4.5 Subgroups of particular interest

Does the 100%-to-the-mother default effect survive among low-income separated parents where stakes are even higher and where parents have strong incentives to claim all benefits that they are entitled to? To investigate this, we have estimated default effects along the prebirth parent couple income distribution of separated parents. To gain precision, we estimate the effects by income quintile group using the difference-in-differences model of Equation (2). Again, the outcome takes the value 1 if the mother received the entire child allowance at the 48th payment and zero otherwise. Panel A in Figure 5 presents the results. We first note that the estimates are significantly positive for all quintiles. We also see that the 100%-to-the-mother default matters less for low-income separated parents. Still, there is a clear and significant effect in the bottom quintile group: 13 percentage points in the lowest quintile, and just above 20 percentage points for quintiles 2–3. In quintiles 4–5, the effect averages 40 percentage points.[29] We conclude that the 100%-to-the-mother default has power to redistribute resources from fathers to mothers even among economically vulnerable parents with separate household budgets.

Since the child's living arrangement after separation is related to the socioeconomic status of the parents (living primarily with the mother is more common among parents with low income), the pattern in panel A of Figure 5 might reflect heterogeneous effects of the default across different living arrangements. While the child's living arrangement generally is unobserved when parents have separated, we can deduce it for some parents as they receive other child-related benefits. If the child primarily stays with one of the parents (typically the mother), the other parent (typically the father) is obliged to pay child support (alimony). If the other parent is unable (or unwilling) to pay child support payments, the parent who cares for the child can claim child support from the SSIA. The SSIA then, in turn, seeks reimbursement from the other parent. Using register information on child support transfers, we constructed a smaller sample of separated parent couples where we can be fairly sure that the mother is the primary home provider for the child and therefore entitled to 100% of the allowance.[30] Note, however, that the entire child allowance is not automatically redirected to the parent who receives SSIA child support; the parent must actively claim it.

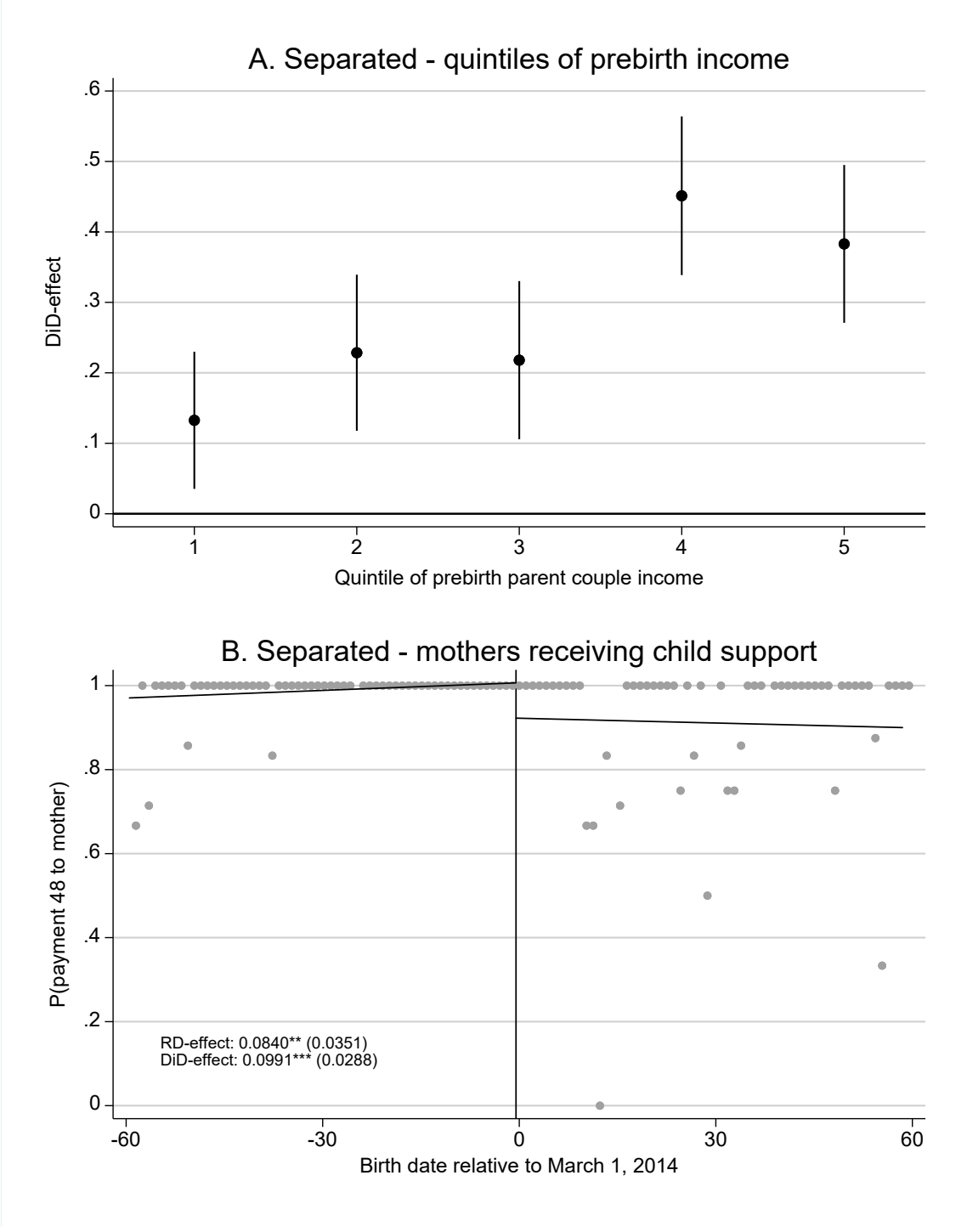
[29] See Table A5 in the Appendix for the printed DiD-estimates as well as the corresponding RD-estimates.

[30] In our data, we observe how much child support from SSIA a parent has received in a given year. However, we cannot link the child support payments to a specific child, and parents may have more than one child. Starting from the sample of parents who were separated at the end of the child's third calendar year, we therefore impose two additional restrictions. First, the mother should receive an amount corresponding to full child support from the SSIA during the child's fourth calendar year. Second, the child born around March 1, 2014, should be the mother's first child to avoid picking up child support related to older children from previous relationships. In summary, we are looking at parents who had their first child (the mother's first child) around March 1, 2014, and who were separated on December 31, 2016. The mother received full child support from the SSIA in 2017 and then we observe the child allowance recipient in the beginning of 2018 (payment 48).

Remarkably, the share of mothers receiving the entire child allowance on the 100%-to-the-mother default side of the cutoff is virtually 100%, while it drops to a little more than 90% on the other side of the default cutoff (see panel B of Figure 5).[31] Apparently, the default-based gender targeting of transfers can be powerful also in situations where individuals have strong incentives to claim the benefits they are entitled to. In similarity with the analysis of separated couples, we cannot *ex ante* exclude that there is a causal effect of the policy discontinuity on the probability of appearing in the sample. However, as can be seen from column 7 in Table 2 there are no such indications, and we are confident that the effect we illustrate in panel B of Figure 5 indeed reflects a causal effect of the default policy.

[31] In this particular RD-specification, we weight observations with a triangular kernel. When using a uniform kernel, we obtain a larger effect, but then the left-hand side intercept is well off the feasible probability range.

Figure 5 Effects of the 100%-to-the-mother default on chosen disbursement option at payment 48 for *separated parents* by prebirth household income quintile (difference-in-differences estimates)



Note: The outcome in both panels takes the value 1 if the mother received the entire child allowance at the 48th payment. Panel A is based on parents of children born in February–March in years 2010–2014 who were separated at the end of the year when the child turned 2. It presents estimates of α_4 from Equation (2) with 95 % confidence intervals for different quintiles of the prebirth parent-couple income distribution among the separated. Panel B is based on parents of children born in the period January–April in 2014 and who belong to the "child support sample", which is explained in section 4.5. The printed RD-effect is an estimate of β_1 from Equation (1) and represents the difference between the left- and the right-hand side intercepts. The printed DiD-effect is an estimate of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level.

5 Child allowances and gender equality: correlations and causality

In this section, we consider the relation between the parents' division of the child allowance and measures of gender equality in the parent couple. We first investigate if there is a relationship at all and then to what extent the relation is causal.

5.1 Active and passive parents

A substantial fraction (40 percent) of parents of new-born children in the 50/50-regime do indeed choose to actively redirect the entire allowance to the mother. In this subsection, we present a descriptive analysis of the characteristics of active and passive parents and how they differ in revealed behaviour when it comes to the division of childcare responsibilities.

First, in Table 3, we show characteristics of parents by chosen disbursement option at payment 12 for children born in March–December 2014. We note that very few parents switch from the 50/50 default to the father as the sole recipient (column 1), and foreign-born parents are strongly overrepresented in this small group. About 40% of the parents switch to the mother as the sole recipient (column 3) and foreign-born parents are overrepresented also in this group. Parents who passively follow the 50/50 default option are more likely to be first-time parents and they have higher education and income. The passive parents (column 2) are significantly different (on the 1% level) from the other parents (columns 1+3) in all dimensions listed in the table.

Table 3 Characteristics by chosen disbursement option (50/50 is the default)

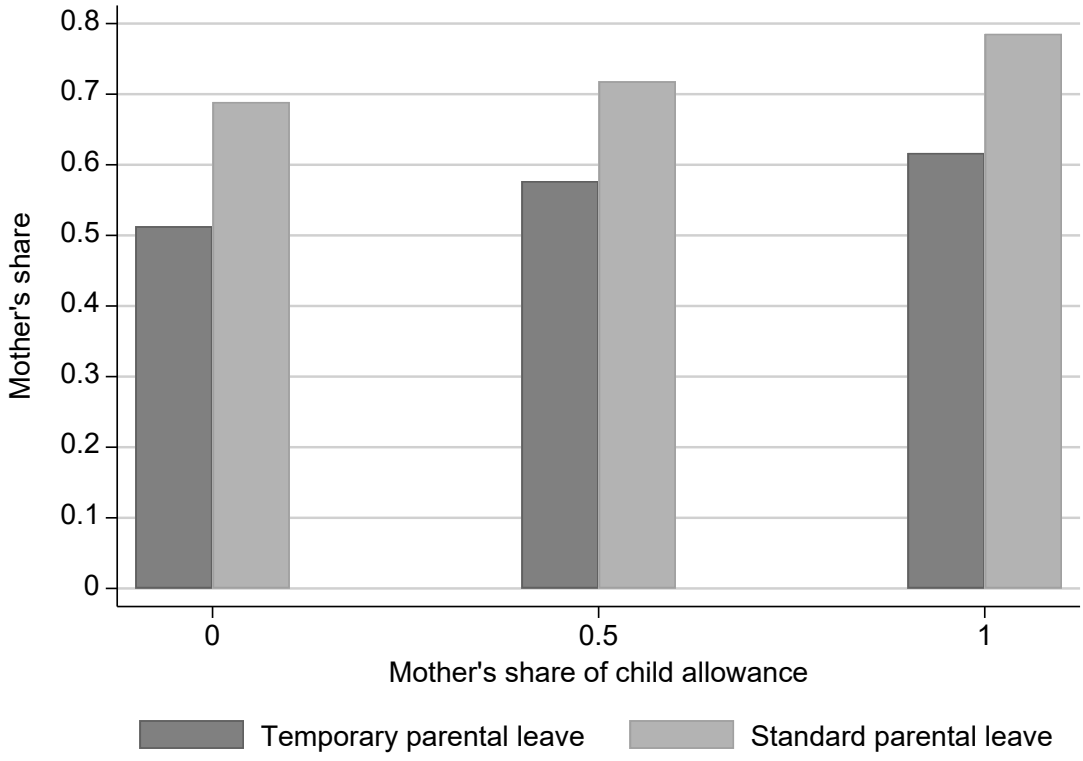
Column:	(1)	(2)	(3)
Share to mother:	0% to mother	50% to mother	100% to mother
Age at birth (mother)	30.72	30.64	30.95
Age at birth (father)	34.94	33.20	34.21
Years of schooling in t-1 (mother)	12.91	13.37	12.83
Years of schooling in t-1 (father)	12.63	12.87	12.29
Mother born in Sweden	0.448	0.857	0.685
Father born in Sweden	0.496	0.856	0.664
Mother's income in t-1 (1,000 2018 SEK)	145.98	229.66	186.80
Father's income in t-1 (1,000 2018 SEK)	287.54	336.19	297.32
First-born child (mother)	0.374	0.485	0.346
First-born child (father)	0.345	0.483	0.349
Number of children	4,000	46,878	35,602

Note: The table is based on children born in the period March–December 2014. The mother's share of the child allowance is measured at the 12th payment when the child is aged 1.

Second, we study the correlation between child allowance disbursement and family responsibilities in a sample of parents of new-born children in the 50/50-regime (children born March–December 2014). To get a proxy for the division of family responsibilities, we follow earlier literature (Evertsson and Boye, 2018; Boye, 2019; Ichino et al., 2019, and Ekberg,

Eriksson, and Friebel, 2013) suggesting that the division of paid parental and paid temporary parental leave mirrors the division of overall family responsibilities.[32] We use the division of these two types of benefits as a proxy for the division of responsibilities in the household and relate those to how the child allowance is disbursed at payment 12. The results are presented in Figure 6. In couples who actively redirect the entire allowance to the mother (bars to the right), the mother also takes most of the paid parental leave: 62 percent of the paid temporary leave and 79 percent of the paid parental leave. The corresponding shares for couples who passively follow the 50/50 default are 58 percent and 72 percent (bars in the middle). In the very small minority of parents who actively redirect the entire allowance to the father, the mother's share is even lower (bars to the left). These positive correlations open the possibility that the division of the child allowance causally affects the division of family responsibilities in line with the intentions of the reform. To answer this question, exogenous variation in disbursements is needed.

Figure 6 The correlation between division of child allowance and division of paid parental leave



Note: The figure is based on children born in the period March–December 2014. The mother's share of the child allowance is measured at the 12th payment when the child is aged 1. The parental leave measures capture the mother's share of the total days used for the child by the 6th birthday. Paid parental leave is a benefit for taking care of a child in the home instead of working, while temporary paid leave is a benefit for taking care of an ill child.

[32] Temporary parental leave is a benefit parents can use to stay at home and take care of an ill child (that cannot be in school or in daycare) instead of going to work. Regular paid parental leave is a benefit parents can use to stay at home with their children. It is typically used when children are small (before they start going to daycare), but can also be used later on, e.g. when daycare is closed and in similar situations.

5.2 Causal effect of the default recipient on gender equality

The observation that people tend to treat money differently depending on its origin and intended use (see for example Thaler 1999), might imply that the recipient of the child allowance is expected to take greater responsibility for child-related consumption/saving and overall childcare. Following that logic, it might matter for gender equality in the household if the entire allowance is deposited in the mother's bank account, or if the parents receive 50% each. An explicit idea behind the introduction of the 50/50 regime was that payments to the fathers' accounts would serve as monthly reminders of the fathers' childcare responsibilities.[33]

One may also hypothesize that, under imperfect income pooling, an increase (decrease) in one parent's child allowance induces a decrease (increase) in his/her labour supply due to an income effect. From this perspective, the 100%-to-the-mother default policy is likely to reduce mothers' labour supply.

We estimate the regression discontinuity model in Equation (1) and the difference-in-differences model in Equation (2) with outcomes capturing division of market and home production.[34] The results for the overall population are presented in panel A of Table 4. Evidently, there are no quantitively important effects on the studied outcomes.

Finally, we also present estimates of the 100%-to-the-mother default effect on the outcomes capturing the division of market and home production by separation status of the parents at the end of the child's third calendar year: see panel B and C of Table 4. Since separated mothers (fathers) in parent couples with a 100%-to-the-mother default receive more (less) child allowance than separated mothers (fathers) in parent couples with a 50/50 default, there could be labour supply differences between the groups driven by income effects (González 2013; Schirle 2015; Magda, Kiełczewska, and Brandt 2020). Once more, the estimates for the mother's share of income (column 1) and temporary parental leave (column 3) are insignificant. However, for parents living together, we do find some evidence of a *negative* response in the mother's share of the parental leave days (column 2). However, in the DiD-specification this estimate is only borderline significant.

From a gender equality perspective, an equal division of parental leave is typically considered as being a desirable outcome. An important lesson from Table 4 is that we do not find any evidence of negative effects on gender equality from the 100%-to-the-mother default.

[33] That both parents by default are recipients of the child allowance signals their common responsibility for the child according to the bill proposed by the government: Prop. 2013/14:6, p.48.

[34] We estimate reduced form effects of the 100%-to-the-mother default on the outcomes rather than using the default as an instrument for how the child allowance is received. This choice is motivated by the concern that the exclusion restriction would be violated in an instrumental variable approach. The default option could influence the behaviour of the parents without going through the payment division mechanism, e.g., through a signaling value of being a default recipient.

Table 4 Effects of the 100%-to-the-mother default on outcomes related to household gender equality

Column:	(1)	(2)	(3)
Outcome:	Mother's share of labour income	Mother's share of paid parental leave	Mother's share of paid temporary parental leave
A. All			
RD-effect	0.0047	-0.0067	0.0072
	(0.0042)	(0.0041)	(0.0071)
DiD-effect	0.0020	-0.0068	-0.0034
	(0.0042)	(0.0042)	(0.0072)
Mean	0.3502	0.7443	0.5916
Observations (RD)	33,906	34,039	30,769
Observations (DiD)	84,772	85,298	75,970
B. Living together at end of year 2			
RD-effect	0.0059	-0.0086**	0.0052
	(0.0042)	(0.0043)	(0.0074)
DiD-effect	0.0039	-0.0075*	-0.0032
	(0.0043)	(0.0044)	(0.0075)
Mean	0.3470	0.7358	0.5864
Observations (RD)	30,137	30,220	27,608
Observations (DiD)	75,562	75,938	68,497
C. Separated at end of year 2			
RD-effect	-0.0081	0.0004	0.0194
	(0.0164)	(0.0127)	(0.0253)
DiD-effect	-0.0154	-0.0073	-0.0073
	(0.0166)	(0.0127)	(0.0258)
Mean	0.3771	0.8152	0.6386
Observations (RD)	3,769	3,819	3,161
Observations (DiD)	9,210	9,360	7,473

Note: The RD-effect represents estimates of β_1 from Equation (1). The DiD-effect represents estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. The outcomes measure the mother's share of the parent couple's total labour income and uptake of standard and temporary paid parental leave days when the child is aged 0–6.

6 Concluding discussion

It is well known that the design of child transfers matters both for female empowerment and children's well-being. In this paper we highlighted a distinction between benefit *entitlement* and benefit *receipt*, and we analyzed the effect of varying the default recipient. To the best of our knowledge, this distinction has been neglected in the previous research literature on child benefits. A common view is that transfers to families with children should be targeted to mothers. Gender targeting may, however, violate fundamental legal principles of equal treatment and non-discrimination. An alternative policy, which would be a compromise between pure gender neutrality and conventional gender targeting, is that mothers and fathers are equally entitled to child benefits, while the default option is that money are disbursed to mothers. It would be practically feasible for the government to design a cash-transfer program to families with children

in which mothers by default receive 100% of the transfer after childbirth but only 50% of the entitlement. In such a system, a father can always claim his share of the benefit, but if he is passive the entire transfer will be deposited in the mother's bank account. One could argue that such a program better satisfies principles of equal treatment of men and women, while still benefiting mothers and children with a nudging mechanism.

By exploiting a sharp birthday discontinuity in the Swedish system for child allowances we, in this paper, demonstrate that the 100%-to-the-mother default has substantial effects on the gender-division of long-run child allowance disbursements. Most importantly, the 100%-to-the-mother default, relative to the 50/50 default, improves the economic conditions of separated low-income mothers, and of lone mothers who are fully entitled to the benefit but fail to actively claim it under the 50/50-default. We do not find any evidence of negative effects on gender equality when examining outcomes capturing the division of childcare responsibility and market work. We therefore believe that our findings have clear policy relevance.

Our study opens interesting avenues for future empirical and theoretical research, e.g., with respect to consumption responses and bank transactions. As shown by Andersen, Johannesen, and Sheridan (2020) for Denmark, such data can in principle be collected from retail banks, but this is clearly beyond the scope of this paper. Additionally, it would be interesting to estimate long-run effects on the *children's* labour market, educational, and health outcomes. For the time being, such an evaluation cannot be done as too little time has elapsed from the 2014 reform.

More generally, our paper is relevant to the normative literature on the optimal design of tax- and transfer systems. While abstracting from nudges, Alesina, Ichino, and Karabarbounis (2011) and Bastani (2013) have shown that gender-specific tax- and transfer systems give rise to social welfare gains, relative to gender-neutral systems. But gender-differentiated tax- and transfer systems are often deemed controversial. Our study highlights that other policy instruments than traditional gender-targeted transfers are available if the objective is to support mothers.

References

Alesina, Alberto, Andrea Ichino, and Loukas Karabarbounis. 2011. 'Gender-Based Taxation and the Division of Family Chores'. *American Economic Journal: Economic Policy* 3 (2): 1–40. https://doi.org/10.1257/pol.3.2.1.

Almås, Ingvild, Alex Armand, Orazio Attanasio, and Pedro Carneiro. 2018. 'Measuring and Changing Control: Women's Empowerment and Targeted Transfers'. *The Economic Journal* 128 (612): F609–39. https://doi.org/10.1111/ecoj.12517.

Andersen, Asger Lau, Niels Johannesen, and Adam Sheridan. 2020. 'Bailing out the Kids: New Evidence on Informal Insurance from One Billion Bank Transfers'. *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3624022.

Armand, Alex, Orazio Attanasio, Pedro Carneiro, and Valérie Lechene. 2020. 'The Effect of Gender-Targeted Conditional Cash Transfers on Household Expenditures: Evidence from a Randomized Experiment'. *Economic Journal* 130 (631): 1875–97. https://econpapers.repec.org/article/oupeconjl/v_3a130_3ay_3a2020_3ai_3a631_3ap_3a1875-1897..htm.

Attanasio, Orazio P., and Valérie Lechene. 2014. 'Efficient Responses to Targeted Cash Transfers'. *Journal of Political Economy* 122 (1): 178–222. https://doi.org/10.1086/674968.

Avdic, Daniel, and Arizo Karimi. 2018. 'Modern Family? Paternity Leave and Marital Stability'. *American Economic Journal: Applied Economics* 10 (4): 283–307. https://doi.org/10.1257/app.20160426.

Bastani, Spencer. 2013. 'Gender-Based and Couple-Based Taxation'. *International Tax and Public Finance* 20 (4): 653–86. https://doi.org/10.1007/s10797-013-9285-9.

Borra, Cristina, Libertad González, and Almudena Sevilla. 2016. 'Birth Timing and Neonatal Health'. *American Economic Review* 106 (5): 329–32. https://doi.org/10.1257/aer.p20161123.

Boye, Katarina. 2019. 'Care More, Earn Less? The Association between Taking Paid Leave to Care for Sick Children and Wages among Swedish Parents'. *Work, Employment and Society* 33 (6): 983–1001. https://doi.org/10.1177/0950017019868138.

Cesarini, David, Erik Lindqvist, Matthew J. Notowidigdo, and Robert Östling. 2017. 'The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries'. *American Economic Review* 107 (12): 3917–46. https://doi.org/10.1257/aer.20151589.

Duflo, Esther. 2003. 'Grandmothers and Granddaughters : Old-Age Pensions and Intrahousehold Allocation in South Africa', January. https://doi.org/10.1093/wber/lhg013.

———. 2012. 'Women Empowerment and Economic Development'. *Journal of Economic Literature* 50 (4): 1051–79. https://doi.org/10.1257/jel.50.4.1051.

Ekberg, John, Rickard Eriksson, and Guido Friebel. 2013. 'Parental Leave — A Policy Evaluation of the Swedish "Daddy-Month" Reform'. *Journal of Public Economics* 97 (January): 131–43. https://doi.org/10.1016/j.jpubeco.2012.09.001.

Eriksson, Rickard, and Magnus Nermo. 2010. 'Care for Sick Children as a Proxy for Gender Equality in the Family'. *Social Indicators Research* 97 (3): 341–56. https://doi.org/10.1007/s11205-009-9505-y.

Evertsson, Marie, and Katarina Boye. 2018. 'The Transition to Parenthood and the Division of Parental Leave in Different-Sex and Female Same-Sex Couples in Sweden'. *European Sociological Review* 34 (5): 471–85. https://doi.org/10.1093/esr/jcy027.

Fiszbein, Ariel, Norbert Schady, Francisco H. G. Ferreira, Margaret Grosh, Niall Keleher, Pedro Olinto, and Emmanuel Skoufias. 2009. *Conditional Cash Transfers : Reducing Present and Future Poverty*. Washington, DC: World Bank. https://openknowledge.worldbank.org/handle/10986/2597.

Försäkringskassan. 2020. 'Barn- och familje-ersättningar har stor betydelse för ekonomin i låginkomstfamiljer'. *Korta analyser 2020:1* 2020.

Gelber, Alexander M. 2014. 'Taxation and the Earnings of Husbands and Wives: Evidence From Sweden'. *The Review of Economics and Statistics* 96 (2): 287. https://doi.org/10.1162/REST_a_00425.

González, Libertad. 2007. 'The Effect of Benefits on Single Motherhood in Europe'. *Labour Economics* 14 (3): 393–412. https://doi.org/10.1016/j.labeco.2006.03.001.

———. 2013. 'The Effect of a Universal Child Benefit on Conceptions, Abortions, and Early Maternal Labor Supply'. *American Economic Journal: Economic Policy* 5 (3): 160–88. https://doi.org/10.1257/pol.5.3.160.

Hotchkiss, Julie L. 2005. 'Do Husbands and Wives Pool Their Resources?: Further Evidence'. *The Journal of Human Resources* 40 (2): 519–31. https://www.jstor.org/stable/4129536.

Ichino, Andrea, Martin Olsson, Barbara Petrongolo, and Peter Skogman Thoursie. 2019. 'Economic Incentives, Home Production and Gender Identity Norms'. *Economic Incentives*, 58.

ISF. 2016. 'Delat Barnbidrag Ett Stegpå Väg Mot Ökad Jämställdhet?' *Inspektionen För Socialförsäkringen 2016:15* 2016 (15).

Johnson, Eric J., and Daniel Goldstein. 2003. 'Do Defaults Save Lives?' *Science*, November. https://doi.org/10.1126/science.1091721.

Kolesár, Michal, and Christoph Rothe. 2018. 'Inference in Regression Discontinuity Designs with a Discrete Running Variable'. *American Economic Review* 108 (8): 2277–2304. https://doi.org/10.1257/aer.20160945.

Lundberg, Shelly, Robert Pollak, and Terence J. Wales. 1997. 'Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit'. *Journal of Human Resources* 32 (3): 463–80. https://econpapers.repec.org/article/uwpjhriss/v_3a32_3ay_3a1997_3ai_3a3_3ap_3a463-480.htm.

Madrian, Brigitte C., and Dennis F. Shea. 2001. 'The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior'. *The Quarterly Journal of Economics* 116 (4): 1149–87. https://www.jstor.org/stable/2696456.

Magda, Iga, Aneta Kiełczewska, and Nicola Brandt. 2020. 'The Effect of Child Benefit on Female Labor Supply'. *IZA Journal of Labor Policy* 10 (1). https://doi.org/10.2478/izajolp-2020-0017.

Neugart, Michael, and Henry Ohlsson. 2013. 'Economic Incentives and the Timing of Births : Evidence from the German Parental Benefit Reform of 2007'. *Journal of Population Economics* 26 (1): 87–108. http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-186053.

Olafsson, Arna, and Herdis Steingrimsdottir. 2020. 'How Does Daddy at Home Affect Marital Stability?' *The Economic Journal* 130 (629): 1471–1500. https://doi.org/10.1093/ej/ueaa009.

Schirle, Tammy. 2015. 'The Effect of Universal Child Benefits on Labour Supply'. *The Canadian Journal of Economics / Revue Canadienne d'Economique* 48 (2): 437–63. https://www.jstor.org/stable/43818218.

Somville, Vincent, Ingvild Almås, and Lore Vandewalle. 2020. 'The Effect of Gender-Targeted Transfers: Experimental Evidence From India', September. https://doi.org/10.2139/ssrn.3686461.

SOU. 2011. 'Fortsatt föräldrar --om ansvar, ekonomi och samarbete för barnets skull' 2011.

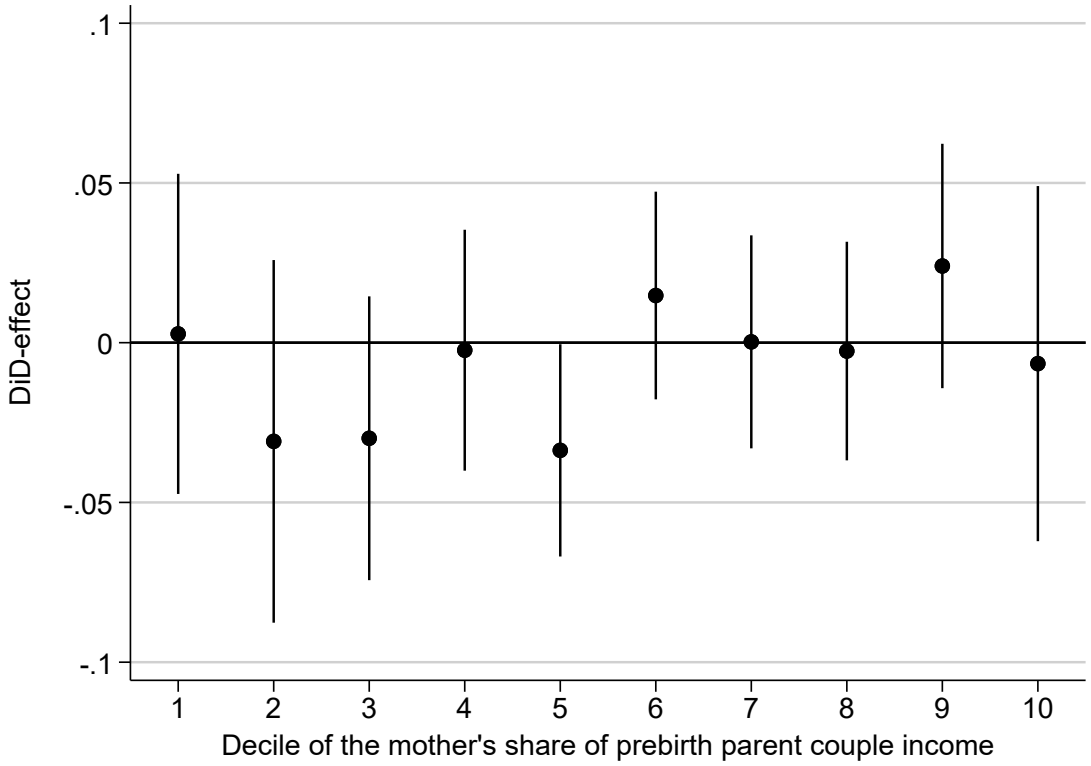
Thaler, Richard H. 1999. 'Mental Accounting Matters'. *Journal of Behavioral Decision Making* 12 (3): 183–206. https://doi.org/10.1002/(SICI)1099-0771(199909)12:3<183::AID-BDM318>3.0.CO;2-F.

Ward-Batts, Jennifer. 2008. 'Out of the Wallet and into the Purse: Using Micro Data to Test Income Pooling'. *The Journal of Human Resources* 43 (2): 325–51. https://www.jstor.org/stable/40057349.

Woolley, Frances. 2004. 'Why Pay Child Benefits to Mothers?' 02–08. *Carleton Economic Papers*. Carleton Economic Papers. Carleton University, Department of Economics. https://ideas.repec.org/p/car/carecp/02-08.html.

Appendix A Additional results

Figure A1 Effects of the 100%-to-the-mother default on the probability of living together at age 2 by deciles of the mother's share of prebirth parent couple income (difference-in-differences estimates)



Note: The figure shows estimates of α_4 from Equation (2) with 95 % confidence intervals for different deciles of the mother's share of prebirth parent couple income. The outcome takes the value 1 if the parents were living together by the end of the year when the child turned 2. All parents of children born in February–March 2010–2014 are included in the analysis.

Table A1 Real and placebo DiD-estimates (outcomes relate to payment 12)

Column:	(1)	(2)	(3)	(4)
Sample:	All	All	All	All
Outcome:	Mother 100%	Mother 50%	Mother 0%	Mother's share
Real DiD (α_4)	0.5628***	-0.5632***	0.0005	0.2811***
	(0.0065)	(0.0053)	(0.0045)	(0.0050)
Placebo DiD (α_0)	0.0019	0.0003	-0.0021	0.0020
	(0.0044)	(0.0004)	(0.0044)	(0.0044)
Placebo DiD (α_1)	-0.0024	0.0007*	0.0017	-0.0021
	(0.0046)	(0.0004)	(0.0045)	(0.0045)
Placebo DiD (α_2)	0.0002	0.0004	-0.0007	0.0005
	(0.0045)	(0.0004)	(0.0045)	(0.0045)
Mean	0.3874	0.5684	0.0441	0.6716
Observations	85,140	85,140	85,140	85,140

Note: The table shows estimates of α_0–α_2 and α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014.

Table A2 Effects of the 100%-to-the-mother default on chosen disbursement option at payment 12

Column:	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	All	All	Married	Married	Unmarried	Unmarried
Model:	RD	DiD	RD	DiD	RD	DiD
Mother 100 %	0.5544***	0.5628***	0.3577***	0.3651***	0.7081***	0.7177***
	(0.0081)	(0.0065)	(0.0131)	(0.0112)	(0.0094)	(0.0070)
Mean	0.3874	0.3874	0.5514	0.5514	0.2592	0.2592
Mother 50 %	-0.5501***	-0.5632***	-0.3413***	-0.3554***	-0.7133***	-0.7257***
	(0.0076)	(0.0053)	(0.0110)	(0.0076)	(0.0091)	(0.0063)
Mean	0.5684	0.5684	0.3582	0.3582	0.7327	0.7327
Mother 0 %	-0.0043	0.0005	-0.0164*	-0.0097	0.0052*	0.0080**
	(0.0045)	(0.0045)	(0.0092)	(0.0091)	(0.0031)	(0.0035)
Mean	0.0441	0.0441	0.0904	0.0904	0.0080	0.0080
Mother's share	0.2794***	0.2811***	0.1870***	0.1874***	0.3514***	0.3549***
	(0.0053)	(0.0050)	(0.0099)	(0.0094)	(0.0053)	(0.0046)
Mean	0.6716	0.6716	0.7305	0.7305	0.6256	0.6256
Observations	33,981	85,140	14,988	37,756	18,993	47,384

Note: Columns 1, 3 and 5 present estimates of β_1 from Equation (1). Columns 2, 4 and 6 present estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. The outcomes "Mother X %" take the value 1 if the mother received X % of the 12th payment. The outcome "Mother's share" is the mother's share of the 12th payment, i.e. 0, 0.5 or 1.

Table A3 Effects of the 100%-to-the-mother default by deciles of the prebirth parent couple income distribution

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Decile:	1	2	3	4	5	6	7	8	9	10
	Outcome: mother receives 100% at payment 12									
RD-effect	0.328***	0.399***	0.491***	0.551***	0.581***	0.647***	0.634***	0.636***	0.637***	0.598***
	(0.030)	(0.028)	(0.027)	(0.026)	(0.026)	(0.023)	(0.024)	(0.024)	(0.024)	(0.024)
DiD-effect	0.306***	0.423***	0.508***	0.552***	0.605***	0.659***	0.626***	0.651***	0.650***	0.608***
	(0.025)	(0.023)	(0.022)	(0.021)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)
Mean	0.571	0.498	0.437	0.412	0.358	0.318	0.339	0.316	0.318	0.342
Obs. (RD)	3,177	3,229	3,174	3,207	3,334	3,480	3,523	3,552	3,609	3,696
Obs. (DiD)	8,036	7,989	7,989	7,923	8,131	8,683	8,777	9,160	9,188	9,264

Note: The RD-effect represents estimates of β_1 from Equation (1). The DiD-effect represents estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. The outcome takes the value 1 if the mother received the entire child allowance at the 12th payment.

Table A4 Effects of the 100%-to-the-mother default, living together vs. separated couples (payment 48)

Column:	(1)	(2)	(3)	(4)
Sample:	Together	Together	Separated	Separated
Model:	RD	DiD	RD	DiD
Mother 100 %	0.5446***	0.5516***	0.2968***	0.2909***
	(0.0088)	(0.0073)	(0.0278)	(0.0252)
Mean	0.3832	0.3832	0.5703	0.5703
Mother 50 %	-0.5424***	-0.5556***	-0.3111***	-0.3030***
	(0.0082)	(0.0060)	(0.0260)	(0.0229)
Mean	0.5694	0.5694	0.3877	0.3877
Mother 0 %	-0.0022	0.0040	0.0143	0.0121
	(0.0050)	(0.0050)	(0.0137)	(0.0132)
Mean	0.0474	0.0474	0.0421	0.0421
Mother's share	0.2734***	0.2738***	0.1413***	0.1394***
	(0.0059)	(0.0055)	(0.0176)	(0.0166)
Mean	0.6679	0.6679	0.7641	0.7641
Observations	30,156	75,824	3,826	9,366

Note: Columns 1 and 3 present estimates of β_1 from Equation (1). Columns 2 and 4 present estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. The outcomes "Mother X %" take the value 1 if the mother received X % of the 12th payment. The outcome "Mother's share" is the mother's share of the 12th payment, i.e. 0, 0.5 or 1.

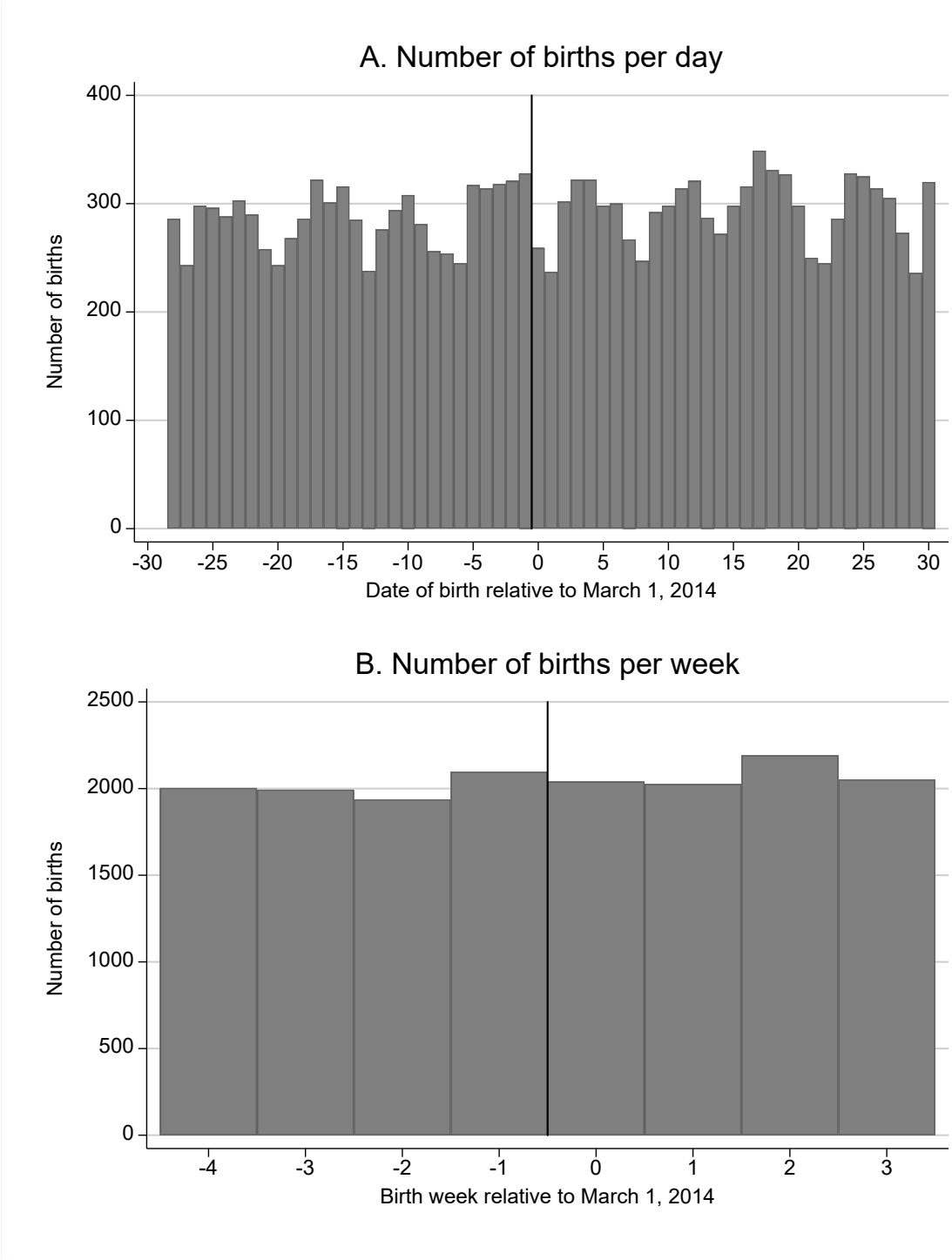
Table A5 Effects of the 100%-to-the-mother default for subgroups of separated couples (payment 48)

Column:	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	Child support
	Outcome: mother receives 100% at payment 48					
RD-effect	0.1423***	0.2241***	0.2525***	0.4565***	0.3755***	0.0840**
	(0.0542)	(0.0625)	(0.0627)	(0.0615)	(0.0611)	(0.0351)
DiD-effect	0.1326***	0.2285***	0.2180***	0.4512***	0.3830***	0.0991***
	(0.0496)	(0.0565)	(0.0572)	(0.0574)	(0.0571)	(0.0288)
Mean	0.7680	0.6195	0.6136	0.4341	0.4471	0.9091
Obs. (RD)	746	762	715	816	787	443
Obs. (DiD)	1,814	1,809	1,839	1,952	1,952	867

Note: The RD-effect represents estimates of β_1 from Equation (1). The DiD-effect represents estimates of α_4 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The reported mean is for children born in March 2014. The outcome takes the value 1 if the mother received the entire child allowance at the 48th payment.

Appendix B Validity of the regression discontinuity model

Figure B1 Distribution of births around March 1, 2014



Note: The figure shows the number of births in our sample per day/week during the period February–March 2014. March 1, 2014, was a Saturday.

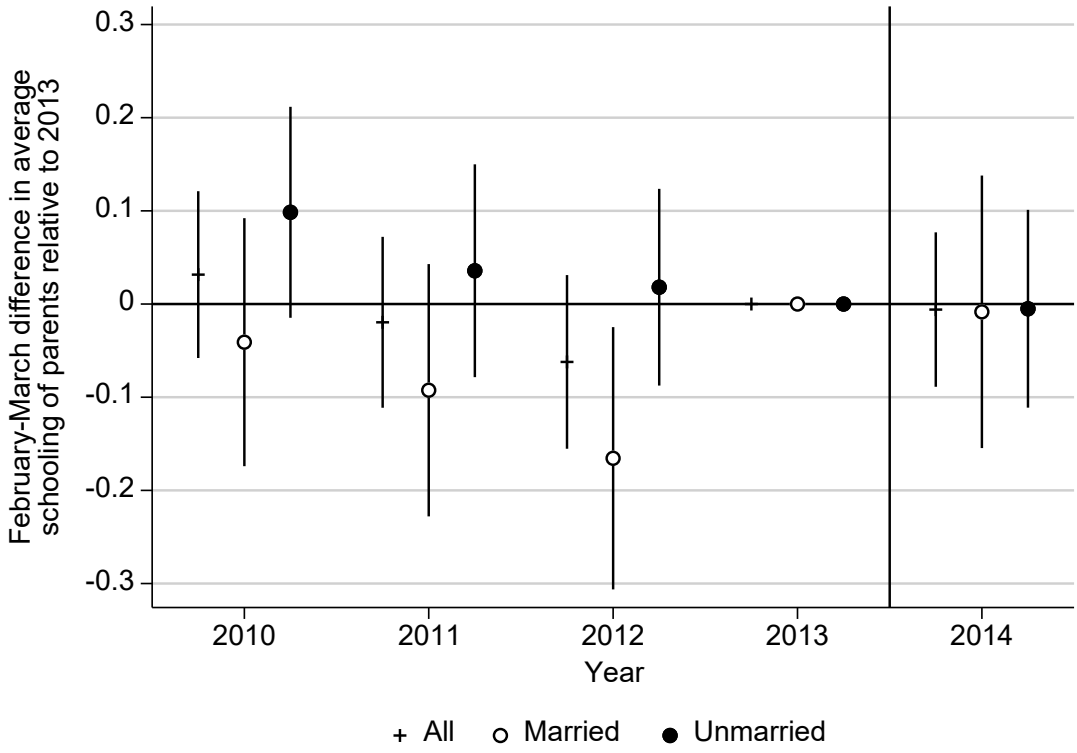
Table B1 Balance of predetermined parental characteristics

Column:	(1)	(2)	(3)	(4)
Outcome (parental average):	Schooling	Income	Age	Born in Sweden
100%-to-the-mother default	0.0248	5,748*	0.143	-0.0087
	(0.0464)	(3,492)	(0.111)	(0.0082)
Mean	12.94	269,002	32.48	0.77
Observations	33,638	34,770	34,770	34,770

Note: The table shows estimates of β_1 from Equation (1). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The sample is children born in January–April 2014 and the reported mean is for children born in March 2014.

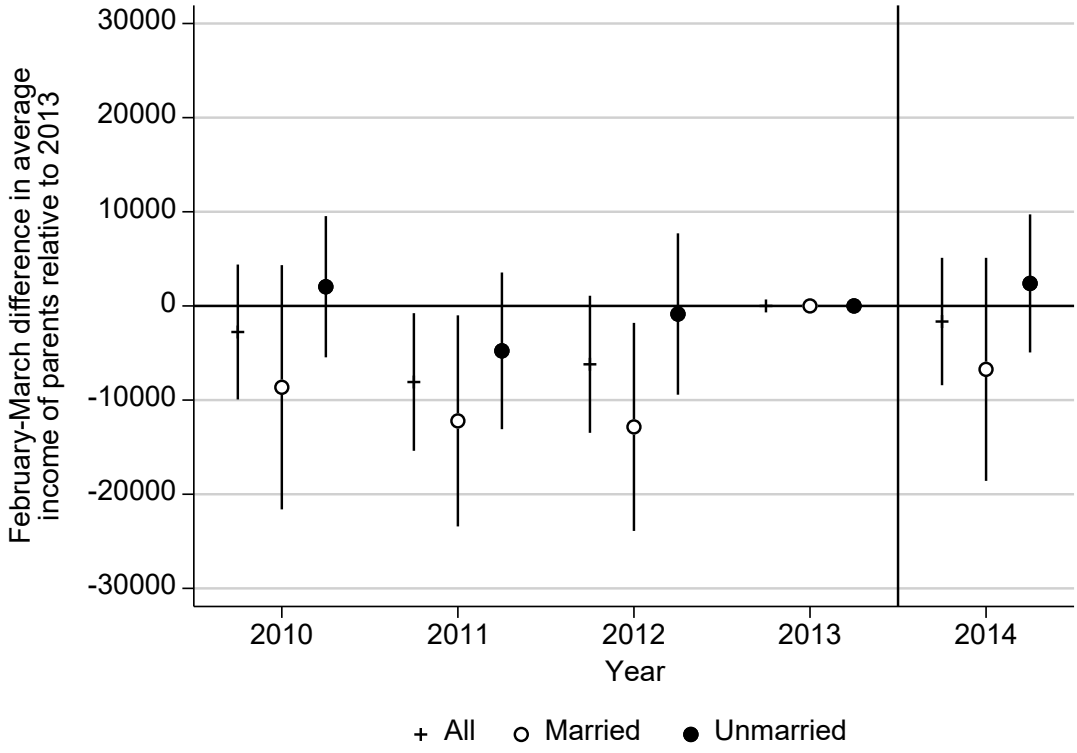
Appendix C Validity of the difference-in-differences model

Figure C1 DiD-"effects" on average prebirth schooling of parents



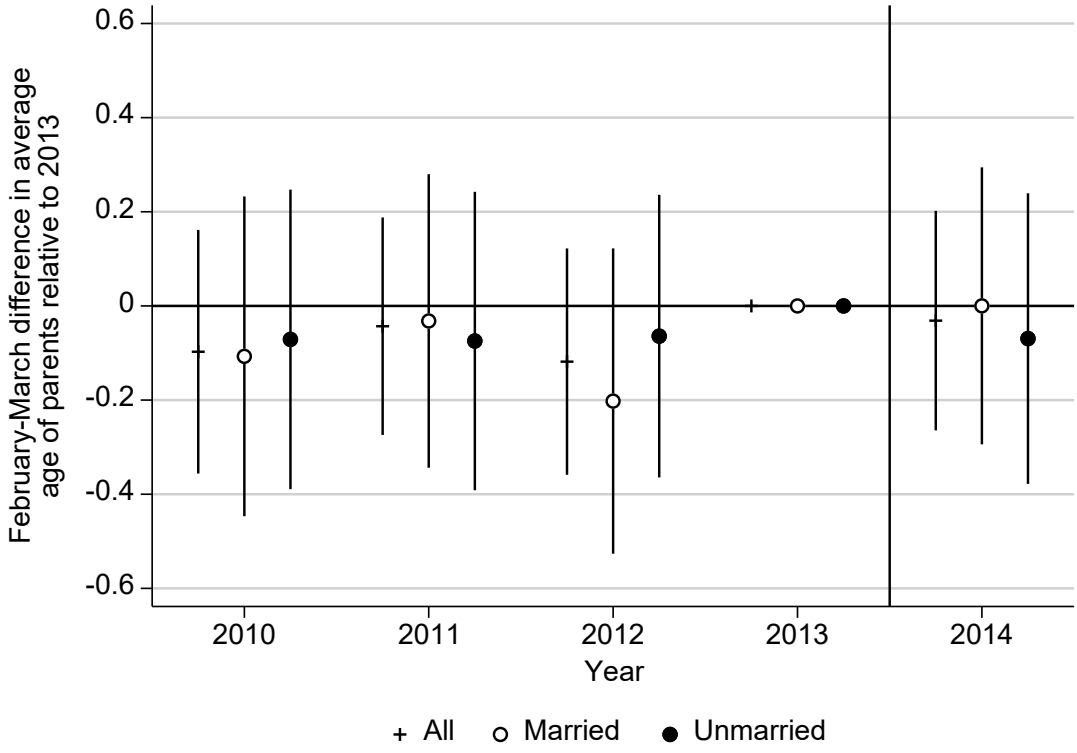
Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C2 DiD-"effects" on average prebirth income of parents



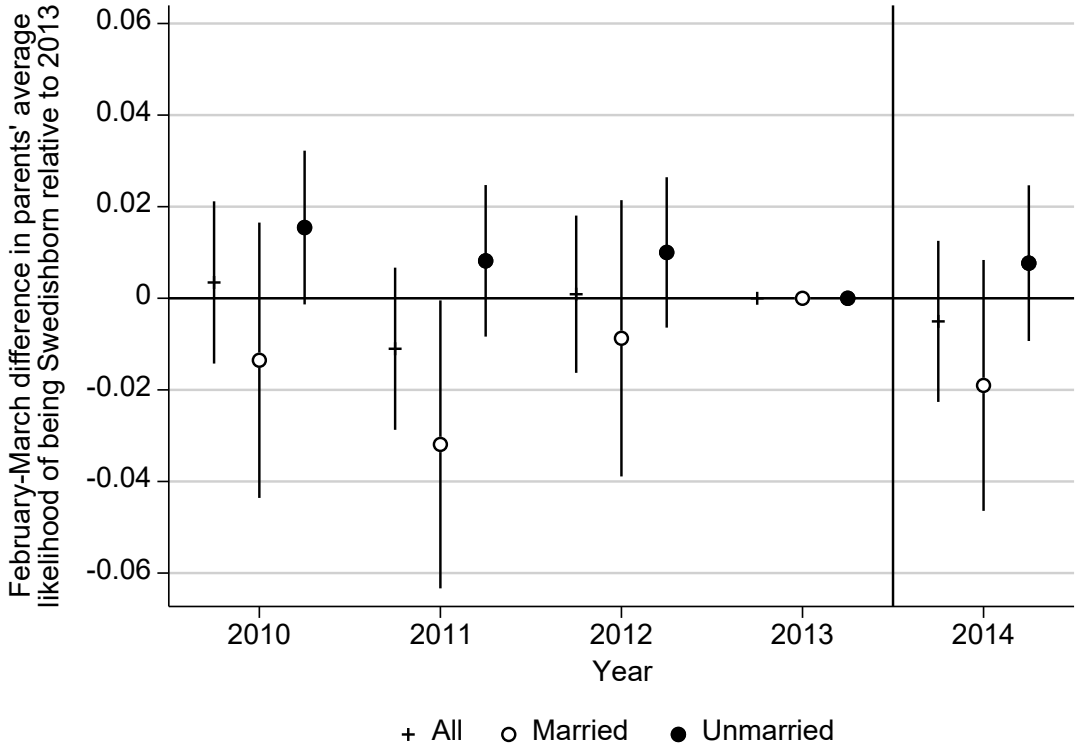
Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C3 DiD-"effects" on average age at birth of parents



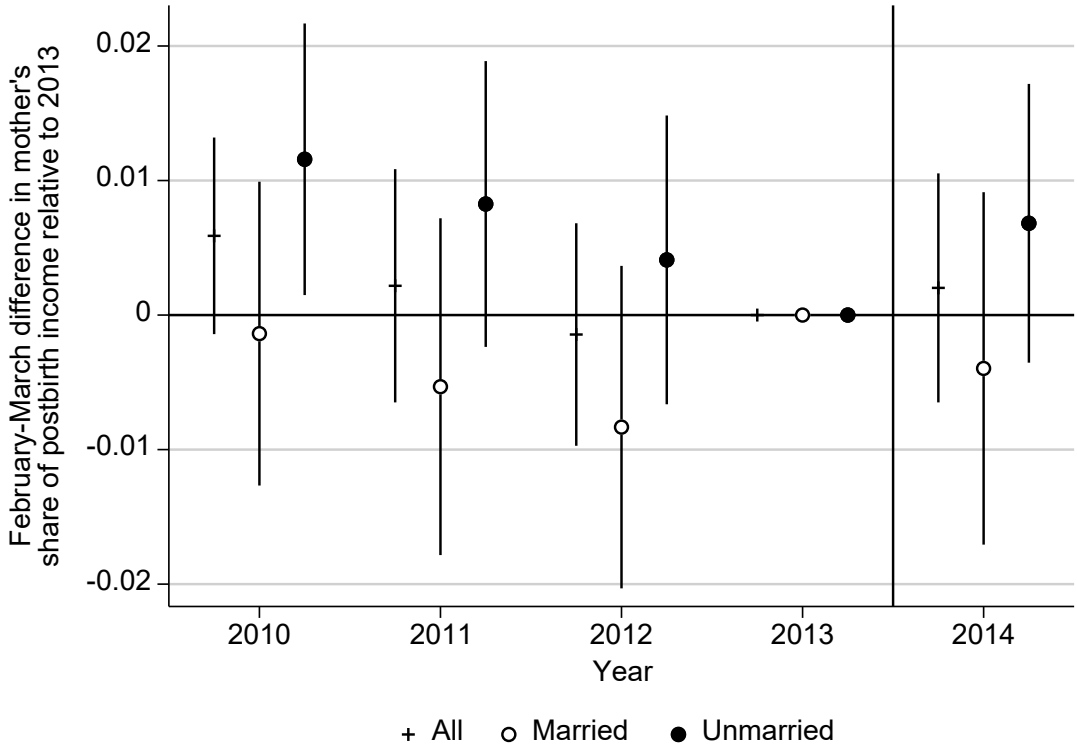
Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C4 DiD-"effects" on parents' average likelihood of being Swedish born



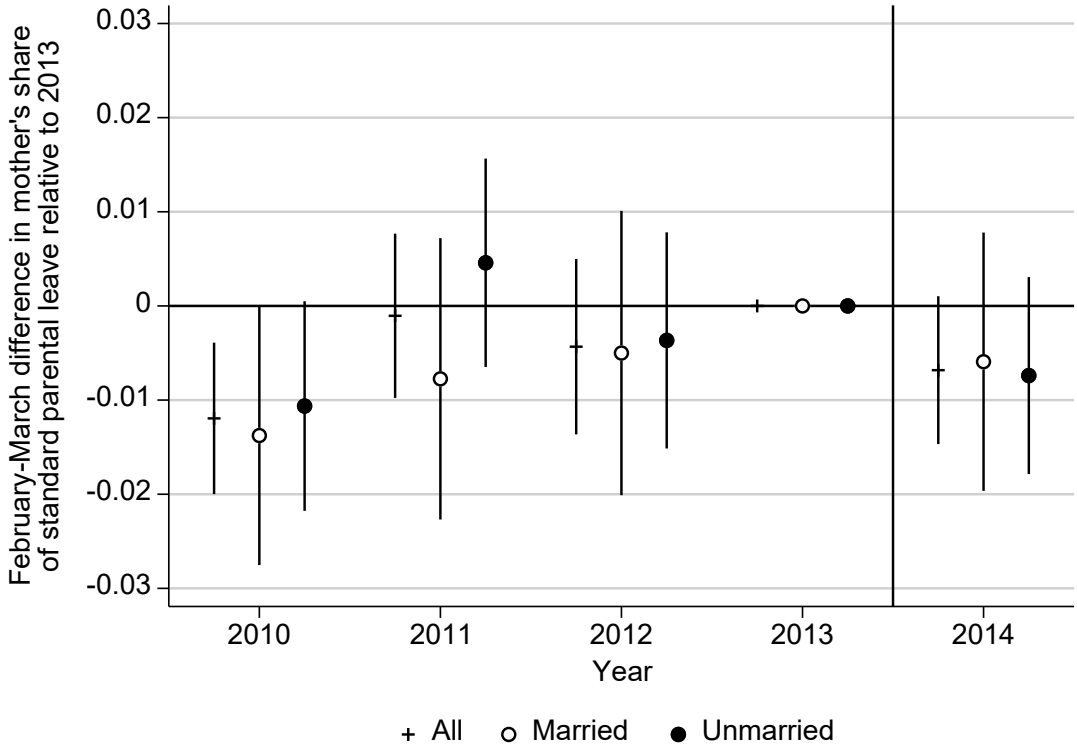
Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C5 DiD-effects on mother's share of post-birth income



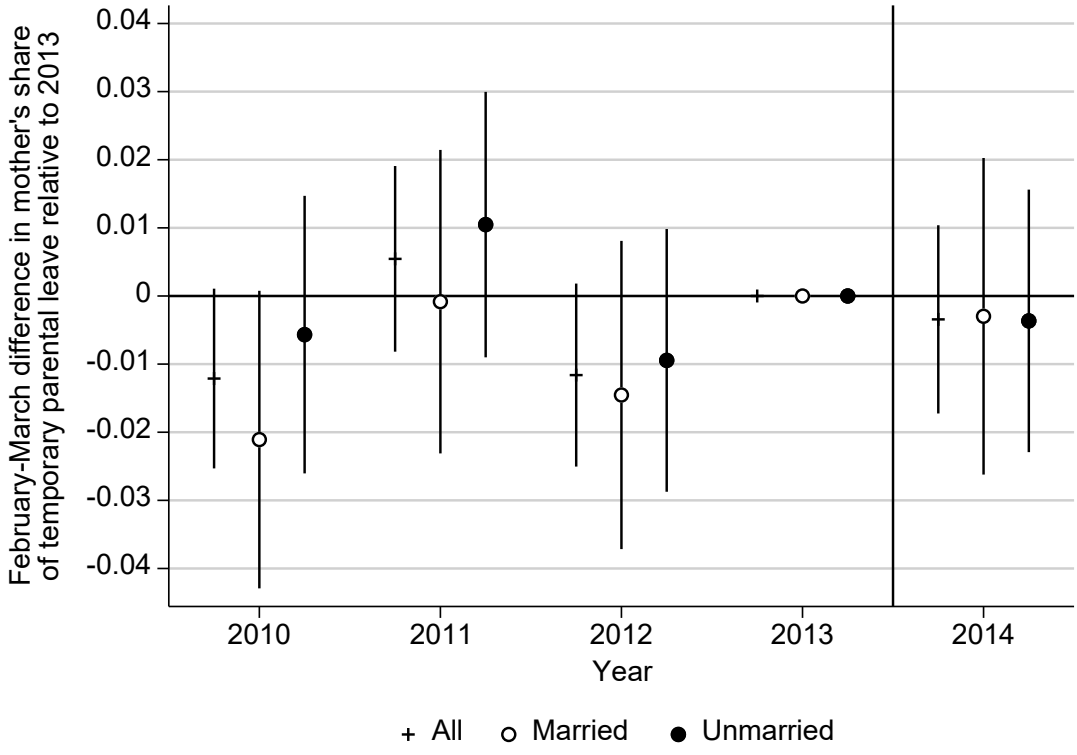
Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C6 DiD-effects on mother's share of standard parental leave



Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Figure C7 DiD-effects on mother's share of temporary parental leave



Note: The figure plots estimates of α_0–α_2 and α_4 from Equation (2).

Appendix D Suggestive evidence on the entitlement effect

In section 4, we established that the 100%-to-the-mother default, in a 50/50 entitlement regime, had a huge effect on the probability that the entire child allowance was deposited in the mother's bank account. The purpose of this Appendix section is to instead provide suggestive evidence on the entitlement effect. We now pose the reverse question: What happens to disbursements if the entitlement changes from 100%-to-the-mother to 50% to each parent *while holding the default option constant*? Unfortunately, there is no birthdate discontinuity in the entitlement, because all fathers living with their children became entitled to 50% of the child allowance from March 1, 2014, regardless of the birthdate of the child. This prevents solid identification of the entitlement effect, but tentative conclusions about its importance might still be drawn.

To study the effect of the 100%-to-the-mother entitlement, relative to the 50/50 entitlement, on long-run child allowance allocation, we need to focus on parents of children born at different points in time before March 1, 2014 onwards. These parents were all exposed to the 100%-to-the-mother entitlement (and consequently 100%-to-the-mother default) *at childbirth*, but while parents of children born just before March 1 quickly were exposed to the new 50/50 entitlement regime, parents of children born in earlier years lived under the old entitlement regime for several years before becoming subject to the new rules. We use this variation in timing to investigate the importance of the entitlement.

In Figure D1, we plot the probability that the entire allowance is paid out to the mother over elapsed time since birth (payment 1 is made in the first month after the birth month and so on). Since we expect action to occur when couples separate, we report this probability separately by separation timing. The sample is restricted to parents who live together at the end of the child's first calendar year (i.e., at payment 11), and then we look separately at parents who separate at some point during the child's second, third, fourth and fifth calendar year. Note that separations are observed only by the end of each calendar year, while child allowance disbursements are measured at a monthly frequency. The separated parents are also contrasted to a group of parents who still live together at the end of the child's fifth calendar year.

In panel A, we focus on parents of children born in January 2010. These parents were subject to the 100%-to-the-mother entitlement between payments 1–50 and then became subject to the 50/50 entitlement.[35] In some cases, when the child exclusively was living together with the father, the mother lost her entitlement also under the old rules. In panel B, we show the corresponding figure for parents of children born in January 2014. These parents became subject to the new 50/50 entitlement regime just two months after childbirth, meaning that separated fathers in this

[35] We only observe child allowance disbursements from August 2010, and therefore information on payments 1–6 is missing for this group.

sample could claim 50% of the child allowance without the mother's consent if the child was living at both separated parents' homes. However, if the child was solely dwelling with the mother, she was entitled to 100% of the allowance. In panel C, finally, we take the difference between the old and new entitlement regimes.

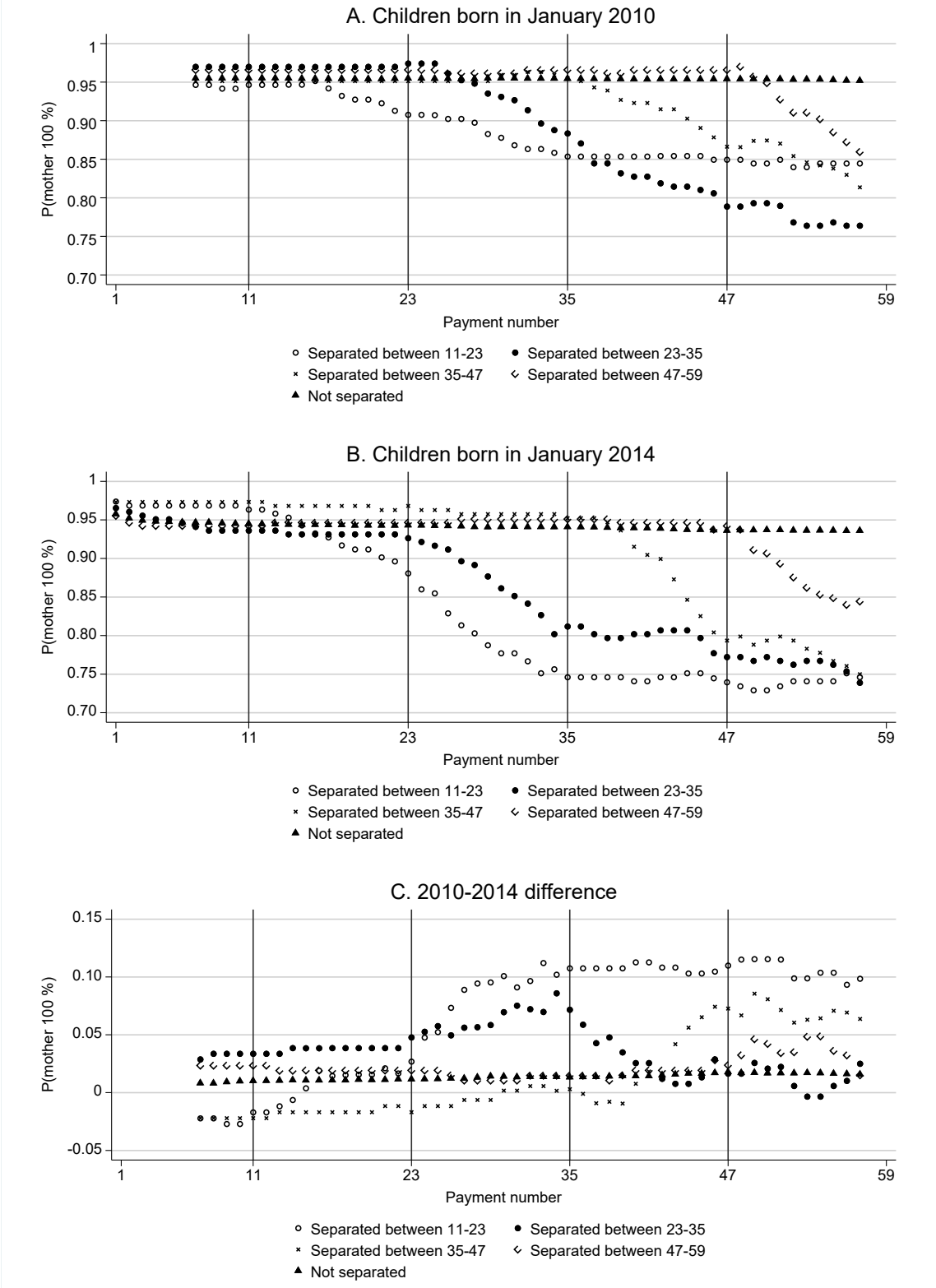
From Figure D1 we can draw the following conclusions. Firstly, the probability that the mother receives 100% of the allowance is approximately 95% under the old regime, and it is only slightly lower under the new rules that imply equal entitlement. We thus conclude that, among parents who are living together, child allowance allocation appears highly insensitive to the entitlement structure. These findings stand in striking contrast to the huge default effects for couples living together reported in Section 4.4.

Secondly, the figure clearly shows that, already under the old rules, separation causes parents to adjust the child allowance allocation. Even though a huge majority of mothers in panel A sample are entitled to 100% of the child allowance, some mothers still agree to splitting the allowance after a separation. The share of mothers receiving the entire child allowance goes down by around 10–15 percentage points after separation.

Thirdly, panel C, which displays the difference between panels A and B, suggests that mothers in the 2010 sample, on average, are about 5 percentage points more likely to remain sole recipients of the child allowance.

From the analysis of this Appendix section, we are fairly confident that the entitlement effect is substantially smaller than the default effect for *couples that are living together*. However, for separated couples, we are more reluctant to draw firm conclusions. One should keep in mind that "identification" comes from pure time series variation – we cannot control for spurious time trends determining child allowance division. Another caveat is that we do not observe the true entitlement structure. As we discuss in Section 4.5, we only observe living arrangements in some very special cases. Accordingly, the difference reported in panel C can be loosely interpreted as an "intent-to-treat" effect.

Figure D1 Entitlement effects in separated and non-separated parent couples



Note: This figure is based on parents of children born in January of 2010 and 2014.