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Explaining Benefit Take-up Behavior – The Role of Financial Incentives

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Explaining Benefit Take-up Behavior – The Role of Financial Incentives

Abstract

Take-up of social benefits is a central issue in poverty alleviation and fiscal evaluations of policy reforms. However, it is difficult to find exogenous variation in the benefit level, and little is therefore known about take-up responses to basic financial incentives. We exploit large and plausibly random variation in levels of "flat-rate parental leave benefits", which all Swedish parents are entitled to. There are no financial reasons to leave money on the table, but take-up is nevertheless imperfect. Higher benefits substantially increase claiming across the income distribution. We further detect sizable spillover effects on subsequent take-up of low-income earners.

JEL-Codes: H240, J220.

Keywords: parental benefits, incomplete take-up, after-tax benefits.

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1 Introduction

Families with children do not always claim the social benefits to which they are entitled, and in modern welfare states poor households leave huge amounts of money on the table (Currie, 2004). One way for governments to redistribute income and alleviate poverty is to increase the generosity of existing programs. How do take-up rates respond to such policies? Do those most in need increase their use?¹ And how does the take-up of subsequent benefits respond?

We study take-up responses to higher benefits using a novel source of variation arising from the Swedish parental leave system. All parents to young children are entitled to 90 days of *flat-rate parental leave benefits* per child. In contrast to regular parental leave benefits, take-up does not require work absence. Thus, there is no financial reason to leave the child benefit on the table. Nevertheless, while more than 90% of parents take up some flat rate benefits, only a minority exhaust all benefits before the child turns 8 and entitlement ends. Accordingly, this is an ideal setting to study take-up behavior at the intensive margin and in different segments of the population.

For identification, we exploit a birthday discontinuity in the benefit level that implies a tripling of the benefit level for parents with children born after July 1, 2006. Using Swedish administrative data for the whole population, we first estimate the direct effect of the benefit increase on take-up. We find that non-take-up is reduced by more than a third as a causal effect of the reform. Average take-up jumps from 60 to 71 days out of 90, and there is a response at all income levels. Second, we analyze spillover effects on the subsample of couples

¹The seminal normative analysis of Nichols and Zeckhauser (1982) recommends that transfer policies should impose lower take-up costs on the truly needy.

who had a new child and thus became eligible for an additional 90 days at the post-reform benefit level. We estimate a surprisingly large spillover response of 3.8 days (one-third of the direct response). Parents entitled to low benefits in 2006 take up less benefits also for the next child. Interestingly, the spillover effect is mainly driven by low-income parents. The results suggest that financial incentives can mitigate behavioral biases, by making benefits more salient to parents.

2 Previous research

Compelling causal evidence on how take-up of social benefits reacts to the benefit level is scarce. Measurement problems is one obstacle. Social benefits are often complicated functions of income, wealth and demographics, and eligibility is sometimes measured with error. As discussed by Blundell et al. (1987), incorrectly measured entitlements lead to an error in the dependent variable (take-up) that is directly induced by a measurement error in the independent variable (the benefit level).² Quite often, basic average take-up rates of social programs are uncertain as the denominator, the number of entitled individuals, is unknown. Fortunately, this is not an issue in our setting, because all parents are entitled to the same pre-tax benefits.

Identification, however, is an even more fundamental concern. With some exceptions, which we review below, research on take-up responses to financial incentives has been conducted within models in which identification relies

²See Hernandez and Pudney (2007), Jäntti (2007), and Bargain et al. (2012) for in-depth discussions in the context of microsimulation models.

on cross-sectional variation in benefit levels.³ Ideally, researchers would prefer to randomly assign different benefit levels to different individuals/households and compare their take-up responses. However, it is much easier to conduct field experiments in which information or application assistance varies across individuals. For example, Bhargava and Manoli (2015) and Linos et al. (2022) randomize how the benefit level of the US Earned Income Tax Credit (EITC) is advertised, but not the benefit level itself.

In the US, social policies such as the EITC and the Child Tax Credit (CTC) are administered through the tax code, and take-up of such benefits therefore requires tax filing, see e.g. Goldin et al. (2022) and Kopczuk and Pop-Eleches (2007). Using a regression discontinuity design, Ramnath and Tong (2017) found that filing rose among low-income households qualified for a stimulus check of USD 300 in 2008. The stimulus check also had a significant causal effect on filing in subsequent years, which, in turn, resulted in increased workforce attachment, earnings, and EITC claiming. Although Ramnath and Tong (2017) provides valuable indirect evidence of a financial incentive effect on benefit take-up (operating through filing), we instead analyze *direct* effects of the benefit level on take-up.

Only a few papers study the causal effect of the benefit level on take-up using quasi-experimental variation (defined in a broad sense). To date, no study on the direct effects of financial incentives on benefit take-up is close to being

³The literature has recently been reviewed by Ko and Moffitt (2024).

a randomized experiment.⁴ Anderson and Meyer (1997) exploit changes to the US income tax system to estimate the take-up elasticity of unemployment insurance benefits with respect to the after-tax benefit level. Zantomio et al. (2010) estimate take-up responses to the 2001 extension to the Minimum Income Guarantee for UK pensioners, and Zantomio (2015) utilized a subsequent reform in the UK pension system for a partially similar purpose. Dahan and Nisan (2010) estimate take-up responses to the benefit level of water consumption bills in the city of Jerusalem, comparing households that had twins (entitled to double benefits) and households that had singletons. A common finding in these studies is that financial incentives matter. Still, it is hard of course to compare magnitudes (e.g. elasticities) across different contexts.

We contribute to the small existing literature on take-up responses to financial incentives in several ways. First, our identification strategy is clean and transparent. In fact, we believe that our setting has a flavor of being a randomized experiment, because the identifying assumptions are straightforward to validate both graphically and statistically. Second, we access rich administrative data on the full population of Swedish parents. We observe benefit entitlement, which is a deterministic function of the children’s birth date, in a precise way. Moreover, our administrative data allow us to examine treatment effect heterogeneity and spillover effects.

⁴By contrast, other aspects of the take-up decision have been analyzed in well-designed randomized field experiments. Several recent papers have studied the effect of information letters or application assistance on take-up. These include Bhargava and Manoli (2015, EITC conditional on filing), Engström et al. (2019, Swedish housing allowances), Finkelstein and Notowidigdo (2019, food stamps in the US), and Matikka and Paukkeri (2022, guarantee pensions in Finland).

3 Institutional setting and data

3.1 Parental leave benefits in Sweden

In the Swedish parental leave system, parents claim benefits for specific dates when they care for their children. The application process is flexible: Parents can apply for all days at once, but they usually apply for periods of benefit days several times. It is possible to claim benefits retroactively for specific dates.

Parents with custody are automatically entitled to parental leave benefits. During the period of study, the two parents were together entitled to 480 days of parental leave benefits for a child: 390 so-called SGI-days and 90 flat rate days.⁵ The SGI benefits depended on the parent's income (80% of previous income up to a ceiling), whereas all parents received the same pre-tax flat rate benefit. Flat-rate days were fully transferable between parents, and one parent did not need permission from the other parent to take up more than 50% of the flat rate benefits.⁶ There were several similarities between the SGI days and flat rate days. Both types of benefits must be taken up before the child turns 8 (or ends first grade). Moreover, both flat rate benefits and SGI benefits were taxed with individual earnings and other taxable social benefits of the recipient, and both benefits were part of the same application procedure.⁷

Parents were allowed to claim flat-rate days on Saturdays and Sundays with-

⁵The number of benefit days does not necessarily translate into the number of actual parental leave days, because parents' job protected leave exceeds the number of benefit days.

⁶By contrast, during the period of study 60 SGI days were earmarked for either parent, as an incentive for fathers to go on parental leave.

⁷Tax filing is very simple in Sweden. The declaration form is pre-filled with third-party reported incomes from employers, and the parental leave benefits are reported by the Swedish Social Insurance Agency (SSIA). Most taxpayers only need to approve the pre-filled form. During the period of study, electronic filing grew in popularity in Sweden. The non-filing rate is below 1%.

out applying for benefits on adjacent Fridays and Mondays. As most people do not work any contracted hours on weekends, and therefore formally do not generate earnings on Saturdays and Sundays, take-up of flat rate benefits did not require job leave and foregone earnings.⁸ Hence, there was no *financial* reason for a parent couple not to take up all the 90 flat rate days. By contrast, the rules for the income-related SGI days were more restrictive, requiring job leave. It is likely that many parents did not fully understand that more generous rules applied to flat-rate days.⁹

Benefit days entitled to job-protected leave on specific dates during the entire 8 year claiming period. Therefore, parents may want to save at least a few days with paid leave all the way up until the child turns 8 in case something happens that makes the leave days useful.¹⁰ Consequently, one should not expect parents to take out all flat rate days at the beginning of the claiming period. This feature of the flat rate days increases the complexity of the parents' optimization problem. Still, there is no financial reason not to exhaust all benefit days by the child's 8th birthday (especially since retroactive claiming is possible).

⁸Parents with contracted hours on weekends may instead flexibly claim flat rate days on weekdays.

⁹Parents' knowledge of the SGI and flat-rate days has been surveyed on a couple of occasions during the last 20 years. To summarize, these studies indicate that mothers know more than fathers. Unfortunately, however, these studies are not informative on whether parents were aware about the possibility to take up flat-rate benefits on weekends, while working Monday and Friday. Anecdotal evidence suggest that many parents are ignorant about this option.

¹⁰During the child's first 18 months, parents are entitled to job protected leave without claiming parental leave benefits. It is common for parents to take unpaid leave when the child is little to save paid leave days for later use. According to Swedish Social Insurance Agency (2013) women (men) averaged 15.3 months (3.8 months) of parental leave while using 9.5 months (2.2 months) of parental benefit days during the child's first two years.

3.2 Mechanisms generating imperfect take-up

Moynihan et al. (2014) distinguish three components of the individual's administrative burden when interacting with the state: i) learning costs, ii) psychological costs, and iii) compliance costs. In our setting, where more than 90% of parents use some flat-rate benefits, but only a minority uses all benefits, the nature of these costs may be very different at the extensive and intensive margins.

Learning, psychological, and compliance costs are certainly all low at the extensive margin. It was easy to apply for the flat-rate benefits, and from other contexts we know that complexity is important for the take-up of social benefits (Blumkin et al., 2020). The flat-rate and SGI benefits were part of the same application system, and take-up rates for the generous SGI benefits were very high (around 95 % in our sample). The use of online applications and digital bank ID increased during the claiming period 2006-2014. Parents could apply for multiple spells of flat-rate days (e.g., on weekends) in the same application.

At the intensive margin, learning costs are likely to vary considerably across individuals, as some people may be unaware or uncertain about the possibility of using benefits on weekends and/or may find retroactive claiming cumbersome. The leading example of psychological costs are stigma costs. Traditional stigma costs are absent in this setting because a very large fraction takes up some benefits, and using benefits on weekends is not visible to others. However, people may still feel morally guilty about using benefits on weekends because they may feel that they are taking advantage of the parental leave system in an inappropriate way, even if it is legal. In sum, we think that imperfect take-up is driven by incomplete knowledge of the details of the system and psychological

costs.

3.3 The 2006 reform

Parents of children born before July 1, 2006 received a pre-tax flat-rate benefit of SEK 60 per day. The after-tax benefit was on average 69% of the pre-tax benefit, or SEK 41. Going from zero to a maximum of 90 parental leave days on average increased disposable income by $41 \times 90 = \text{SEK } 3690$ (USD 374). This benefit level was arguably low – the pre-tax daily benefit was about 7 % of the average full time equivalent daily wage in 2006. The benefit can also be compared with the average pre-tax SGI daily benefit for men (women) in 2006, which was SEK 702 (SEK 580). The maximum pre-tax SGI benefit was SEK 946 per day.

There was a historic jump in the pre-tax benefit level for parents of children born after July 1, who received a benefit of SEK 180 per benefit day. The benefit level of SEK 60 had been frozen at the same nominal level since 1987, and with inflation and real wage growth the value of the benefit declined over time. At the time of writing (in 2024), the nominal pre-tax flat rate benefit is still SEK 180.

The reform was announced on March 16, 2006 (prop. 2005/06:142) and was legislated on May 17, 2006. It is important to note that the only difference between parents of children born before and after July 1 is the benefit level. Other aspects of the choice environment, e.g., exposure to information and application procedures, rules applying to SGI days, and taxes stayed the same.

3.4 Data and sample selection

We combine full population administrative registers of the Swedish Social Insurance Agency (SSIA) and Statistics Sweden. To begin with, we limit the population to parents who had children in 2006. Since benefits may be taken up over an 8 year period, the key thing is to follow these parents up to 2014. We do, however, also access data up to 2018, which enables us to also look at subsequent behavior, e.g., take-up of benefits for younger siblings born up to 2010.

We have spell data on take-up of SGI and flat rate days, separately for each parent from the SSIA. Crucially, we are able to distinguish between flat rate benefit days and SGI benefit days.¹¹ We link children and their parents using a multigeneration register of Statistics Sweden. We observe exact birthdays of all Swedish born children. The administrative registers also contain data on incomes and demographics.

Our analysis sample differs somewhat from the total population of parents of young children. First, we only include parents of Swedish born children, as we do not observe the birthdate of foreign-born children in our data source. Second, both a mother and a father must be observed in the multigenerational register. This requirement excludes parent couples where the father is unknown and same-sex couples. Third, we only include singleton births as there were other rules for twins. Fourth, both the mother and the father must be observed

¹¹According to the raw data, approximately 7% of parent couples in our analysis sample take up more than the maximum of 90 flat rate days. This is most likely due to a confusion between different parts of the parental leave system, e.g., SGI benefits being coded as flat rate benefits by the SSIA. We set these values to 90 days. If not, the estimated effect is around 1 day larger. Additionally, we removed observations with children born *exactly* on July 1, 2006, from the analysis sample. Again, due to miscoding of benefits, take-up of flat rate days was registered at an extremely low level at the particular birthday of July 1, 2006.

in the tax registers from 2003, i.e. three years before childbirth, to 2014, i.e. eight years after childbirth. The reason is that we need to follow the parents' incomes both before and during the 8-year claiming period. These restrictions leave out 15 percent of the initial population. There is no significant change in the probability to be included in the sample at the birthday discontinuity we are exploiting for identification. Therefore, we find it unlikely that it biases the treatment effect in the sample we are considering.¹² Traditionally, endogenous sample selection has been an important issue in the take-up literature, because eligibility is often a function of characteristics like earnings and wealth.

4 Model and empirical approach

4.1 Model framework

Since entitlement to flat-rate benefits is tied to the child, we assume that the *parent couple* is the economic decision maker who decides on benefit take-up.¹³ In the tradition of Moffitt (1983) take-up of social benefits is often modeled as a binary choice, but in our setting there is an important intensive margin of take-up. All parent couples are entitled to a maximum of \tilde{D} units of the social benefit, and D is the number of units taken up. Take-up increases the parent couple's consumption, which we denote by C . Couples trade off the utility of consumption against the disutility of claiming benefits, and they set D to maximize the

¹²We run equation (2) below on the entire unrestricted population, with a dummy for being included in the analysis sample as the outcome variable, and we estimate an insignificant treatment effect.

¹³In our empirical analysis, the parent couple is always a mother and a father. Note that a parent couple is not necessarily living together during the entire claiming period.

utility function $U(C, D) = v(C) - g(D; a)$. The $g(D; a)$ function summarizes the different mechanisms discussed in Section 3.2. $v(C)$ is concave, $g(D; a)$ is convex, and the marginal take-up cost is increasing in the cost parameter a , i.e. $g_{Da} > 0$. We refer to the pre-tax value of a unit of D as b . We can thus write the budget constraint as $C = z + bD - T(z + bD)$, where $T(z + bD)$ is the tax function, and z is exogenous pre-tax income.

In the population of parent couples, whose size is normalized to unity, couples differ along two dimensions; exogenous pre-tax income, z , and the take-up cost technology parameter a . Empirically, the former is observed, while the latter is unobserved. Vector $x = \{z, a\}$ captures both sources of heterogeneity, and we do not *ex ante* impose any assumptions on their covariance. We write the joint density $f(a, z)$ and the joint cumulative density $F(a, z)$. In Appendix A we show that couples will sort into three groups. $F[\bar{x}(b)]$ with sufficiently high income and take up costs take up zero benefits ($D = 0$). Couples with sufficiently low income and take-up costs, $F[\underline{x}(b)]$, take up full benefits ($D = \tilde{D}$). Finally, $F[\bar{x}(b)] - F[\underline{x}(b)]$ will be located in the interior; these couples will equate the marginal rate of substitution with the marginal after-tax benefit value.

Our quasi-experimental variation implies a tripling of the benefit level. What happens in this environment if we increase the pre-tax unit value from b_0 to $b_0 + \Delta b$?

1. **The extensive margin response.** Couples choose to take up something instead of nothing when the benefit increases. The share of the population with $D > 0$ jumps from $F[\bar{x}(b_0)]$ to $F[\bar{x}(b_0 + \Delta b)]$. In principle, part of this response could be due to couples switching from $D = 0$ to $D = \tilde{D}$.

2. **The ceiling response.** Couples choose to take up their entire entitlements when the benefit triples.¹⁴ The share of the population with $D = \tilde{D}$ jumps from $F[\underline{x}(b_0)]$ to $F[\underline{x}(b_0 + \Delta b)]$.
3. **The interior response.** Letting $\bar{D}(b)$ denote the mean take-up among couples with $D > 0$ and $D < \tilde{D}$, we can write the interior response as $[\bar{D}(b_0 + \Delta b) - \bar{D}(b_0)]$. Note that the composition of this population will change endogenously when the benefit jumps – couples will both enter and exit the subpopulation with interior take-up. Therefore, unlike the first two responses, the sign of this quantity is theoretically undetermined.

In Appendix A we demonstrate that the average treatment effect of the discrete benefit increase can be decomposed in the following way:

$$\begin{aligned}
E[D(b_0 + \Delta b)] - E[D(b_0)] = & \\
& [\tilde{D} - \bar{D}(b_0 + \Delta b)] \times \underbrace{\{F[\underline{x}(b_0 + \Delta b)] - F[\underline{x}(b_0)]\}}_{\text{Ceiling response}} \\
& + \bar{D}(b_0 + \Delta b) \times \underbrace{\{F[\bar{x}(b_0 + \Delta b)] - F[\bar{x}(b_0)]\}}_{\text{Extensive margin response}} \\
& + \{F[\bar{x}(b_0)] - F[\underline{x}(b_0)]\} \times \underbrace{[\bar{D}(b_0 + \Delta b) - \bar{D}(b_0)]}_{\text{Interior response}}.
\end{aligned} \tag{1}$$

4.2 Empirical approach

Our empirical setup is a sharp regression discontinuity (RD) model, with birth-date (measured at a daily frequency) as the running variable. All parents of

¹⁴Here we make the innocuous assumption that the substitution effect dominates the income effect from the benefit increase. As we elaborate on in Appendix A, the assumption is indeed innocuous, because the value of the benefits is small relative to parents' income.

children born on July 1, 2006, and later were deterministically assigned to a pre-tax benefit entitlement of SEK 180 per benefit unit, whereas parents of children born before July 1, 2006, were entitled to SEK 60 per benefit unit. We estimate the average treatment effect from the large benefit increase using the regression equation

$$\text{Outcome}_i = \alpha_0 + \alpha_1 \text{High}_i + \alpha_2 \text{BD}_i + \alpha_3 \text{BD}_i \times \text{High}_i + \epsilon_i, \quad (2)$$

where High_i is an indicator variable that takes the value of 1 if the child is born July 1 and later, and zero otherwise. BD is birthdate in numeric format, with July 1 normalized to be zero. We estimate different linear slopes at each side of the cutoff, and the average treatment effect α_1 represents the vertical distance between the left and the right intercepts. Throughout the entire analysis, we use a fixed bandwidth of ± 90 birthdays.

4.3 Specification tests

We examined covariate balance at the cutoff with respect to the two parents' years of schooling, the probability of being born in Sweden, age, and income for 2005. There are no significant effects at the cutoff, see Table B1 of Appendix B. In Appendix B we also graphed the frequency distributions of births, centered at the cutoff. There are always fewer births on weekends, see Figure B1a, due to fewer planned childbirths on weekends, creating a "within-week seasonality". When removing this seasonality by simply collapsing data into weekly frequencies (Figure B1b), the distribution is substantially smoother. When performing the Frandsen (2017) test of no manipulation for distributions with discrete run-

ning variables, we obtain a p-value of 0.231 when collapsing by week. This suggests that there is no significant shift in the underlying distribution at the birthday cutoff. The Calonico et al. (2017) manipulation test produces a p-value of 0.796. Taken together, these specification tests suggest that our identifying variation provides compelling exogenous variation in the benefit level.

5 Results

5.1 The reform effect at the cutoff

Figure 1 shows the outcome variable on the vertical axis and the birthdate on the horizontal axis. The first outcome of interest is take-up of flat-rate days (including zeroes and full take-up). The behavioral effect is extremely salient: Average take-up of eligible parent couples jumps from 60 to 71 as a causal effect of the reform. However, one should keep in mind that the benefit tripled. If we relate the percentage change in take-up to the percentage change in the benefit, we obtain a take-up elasticity of $\frac{11}{60} / \frac{(180-60)}{60} = 0.09$.¹⁵ This sounds like a modest elasticity, but the reader should keep in mind that the elasticity is bounded by the ceiling of 90 days. In the hypothetical extreme case, in which everyone chooses to take up 100% after the reform, the treatment effect would amount to 30 days, and the elasticity would be 0.25. An alternative way to frame the result is that non-take-up was reduced by more than one-third after the reform.

The probability to take up any flat-rate benefits is the outcome in Figure 1b. Only a small fraction (8% of parent couples) took up zero benefits before the

¹⁵The percentage change in the after-tax benefit is always very similar to the percentage change in the pre-tax benefit.

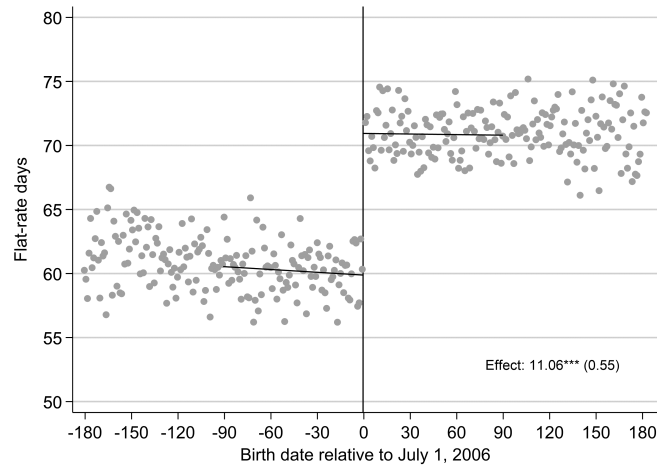
reform. There is nonetheless a sizable extensive margin response of about 4.6 percentage points.¹⁶ Figure 1b shows that there is also a clear reaction in the probability to take up all flat-rate days. Only 26% of parent couples exhausted all 90 flat-rate days under the low-benefit regime, but this fraction increased by around 13 percentage points when the benefit tripled. A majority (62%) of parent couples still took up less than 90 days after the benefit increase.

By virtue of our large fine-grained data and clean reform, Figure 1 provides very informative graphical evidence on behavioral responses. In Appendix C we demonstrate that results are robust to alternative bandwidth choices, weighting procedures, and polynomial choices. Moreover, in Appendix D we graph the *raw* density and cumulative density distributions of flat-rate days for parents of children born in June, 2006, and in July, 2006, respectively. The main results (extensive margin, ceiling, and interior response) are conveyed already in basic descriptive graphs. To conduct placebo tests, we also estimated equation (2) on all July 1 cutoffs from 2002 to 2010, see Figure E2 of Appendix E, and we also graphed the long-term trends, see Figure E1 of Appendix E. The 2006 jump in take-up on July 1 is unprecedented.

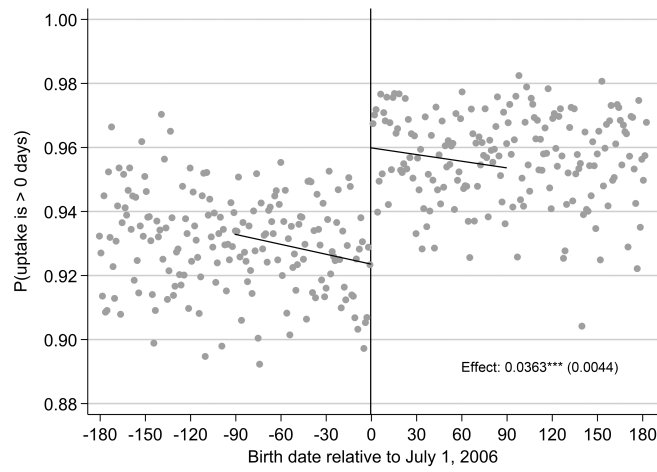
5.2 Other outcomes

Gender division. Studies on the utilization of parental leave benefits typically focus on the gender division of benefit take-up, see e.g. Moberg (2019), Ekberg et al. (2013), and Rosenqvist (2024) for Sweden and Jørgensen and Søgård

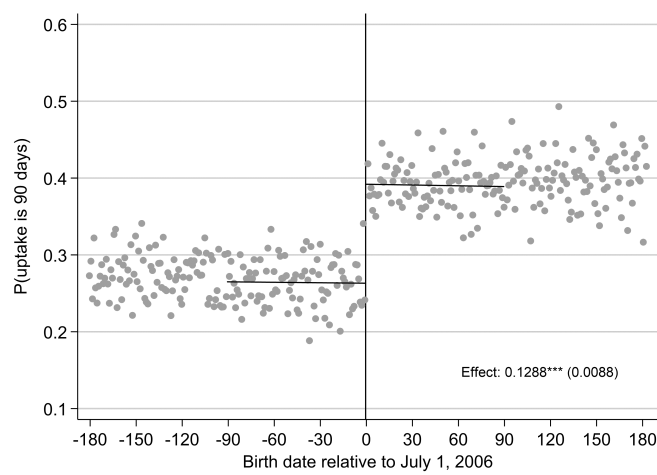
¹⁶For binary outcomes, equation (2) is estimated as a linear probability model; the results are very similar when using probit or logit models, also when the mean of the dependent variable is close to being 1.



(a) Overall response



(b) Extensive margin



(c) Ceiling response

Figure 1: Estimated treatment effects. The vertical distance between the control and treatment intercepts reflect the coefficient α_1 of equation (2). Robust standard errors. Fixed bandwidth of +/-90 days.

(2024) for Denmark. Our research question is related but *ex ante* conceptually distinct, because take-up of flat-rate days does not require work absence. We collapsed the share of flat-rate days taken up by the mother by birthday and reran equation (2). In percentage terms, mothers and fathers responded fairly similarly, and there was only a small positive, borderline significant, effect on the mother's share of take-up of flat-rate days.¹⁷

Weekdays versus Weekends. As already emphasized, flat-rate days can be taken up on weekends (Saturdays and Sundays) when people typically do not earn income. While the precision of the data is not ideal, we have decomposed the overall response (11.06 days) into a weekend response of 3.19 days (control intercept 22.34) and a weekday response of 7.87 (37.54).¹⁸ Hence, the take-up of weekdays actually increases more than the take-up of Saturdays and Sundays, also in percentage terms. This indicates that parents primarily used the additional flat-rate days for the purpose of staying home from work with their kids, rather than optimizing the monetary rewards within the system.

Earnings. At a given level of take-up, parents to children born after July 1, 2006, received a windfall income gain, and from basic economic theory we therefore expect them to work less. However, the windfall is small, especially when viewed from an 8-year perspective. The average after-tax benefit in-

¹⁷This finding is illustrated graphically in Figure F1 of Appendix F.1 along with separate treatment effects for mothers and fathers. In Appendix E, Figure E1, we also graph increasing (decreasing) long term trends in take-up of fathers (mothers).

¹⁸We observe the start date and the termination date of each spell. For 20% of all spells, the number of claimed benefit days is lower than the number of days from the start to the end date. In these cases, we cannot perfectly measure weekdays and weekends. For these spells, we distribute the claimed benefit days equally over the days in the spell.

creases from SEK 41 to SEK 124. In the extreme case of a parent couple taking up 0 days before the reform, and all 90 days after the reform, disposable income increases by $\text{SEK } 90 \times 124 \approx 11,200$ (approx. USD 1,100) over an 8-year period. Cesarini et al. (2017, Table 3, column 1) found that a SEK 100 in lottery income led to a SEK 1.066 decline in *annual* pre-tax earnings. In our setting, income effects on earnings are likely to be small, and we therefore have little power to detect those. In general, we estimate earnings effects that are insignificantly different from zero for both parents in all years 2006-2014. However, in line with theory, the point estimates for mothers are always negative, and in the second calendar year after child birth the negative effect on earnings is significant at 10%, see Table F1 of Appendix F.3.

5.3 Take-up by income level

Three distinct mechanisms may generate differential take-up rates across the income distribution in our analytical framework. First, since marginal utility of consumption is declining in income, a low income couple *ceteris paribus* on the margin values a social benefit of 1 SEK/USD more than a high income couple. Second, the monetary value of the after-tax benefit will be larger for low-income couples due to the progressivity of the income tax system. The pre-reform after-tax benefit falls monotonically from SEK 47 per benefit day in the lowest income group to SEK 36 in the highest income group. Third, income and take-up costs may covary. The first two mechanisms unambiguously predict take-up to be higher at low incomes. The third one is theoretically ambiguous, but perhaps

most likely to work in the opposite direction if e.g., high income parents are more financially literate.

For each pre-reform income quintile group, Figure 2a reports estimated take-up just to the left of the birthday cutoff (control intercept), and just to the right of the cutoff (treatment intercept). Perhaps somewhat surprisingly, pre-reform take-up was the highest among middle income couples in quintile groups 3 and 4. This suggests that other factors than declining marginal utility of income and the after-tax benefit level are important for the take-up decision. Crucially, Figure 2a shows that the benefit increase had a causal impact on take-up levels across the distribution. The reform is leveling take-up rates in quintile groups 1-4. Average take-up was, however, the lowest in the richest quintile both in the SEK 60 and in the SEK 180 regime.

As already mentioned, the "treatment dose" is declining in income as flat-rate benefits are taxed progressively together with income. To obtain the "marginal effect" on take-up from a one-unit (SEK) increment in the after-tax benefit we estimate

$$\text{Outcome}_i = \beta_0 + \beta_1 \text{After-tax benefit}_i + \beta_2 \text{BD}_i + \beta_3 \text{BD}_i \times \text{High}_i + \epsilon_i, \quad (3)$$

by 2SLS on each income quintile group. The first stage equation is given by (2), with "After-tax benefit_{*i*}" on the left hand side. In essence, we scale the reduced form effect with the average increase in the after-tax benefit. Figure 2b plots the β_1 coefficients. Additionally, the marginal effects are decomposed into an extensive margin response, interior response, and ceiling response using equation

(1).¹⁹

Figure 2b reveals that, after scaling, the marginal effects are quite similar across the income distribution, especially after accounting for heterogeneity in extensive margin responses. The reason for which the extensive margin response is small in quintiles 3 and 4 is that pre-reform level is close to 96%, leaving little room for a response.²⁰ The most striking feature of Figure 2d is the sizable interior response in quintile group 2. It should also be noted that the ceiling response is quite low among middle income couples in quintile 3.

5.4 Spillover effects

Many of the children born around July 1, 2006, have siblings. Parents of children born around the reform cutoff were entitled to SEK 180 per flat-rate benefit day for the younger sibling, regardless of which side of the cutoff the older sibling was born in 2006. We have estimated equation (2) on the subset of parent couples that had a new child in 2007-2010, with flat-rate days for the younger sibling as the outcome. Figure 3a reports a substantial overall spillover effect of 3.77 days, or one-third of the direct effect (11.06 days) reported in Figure 1a.²¹

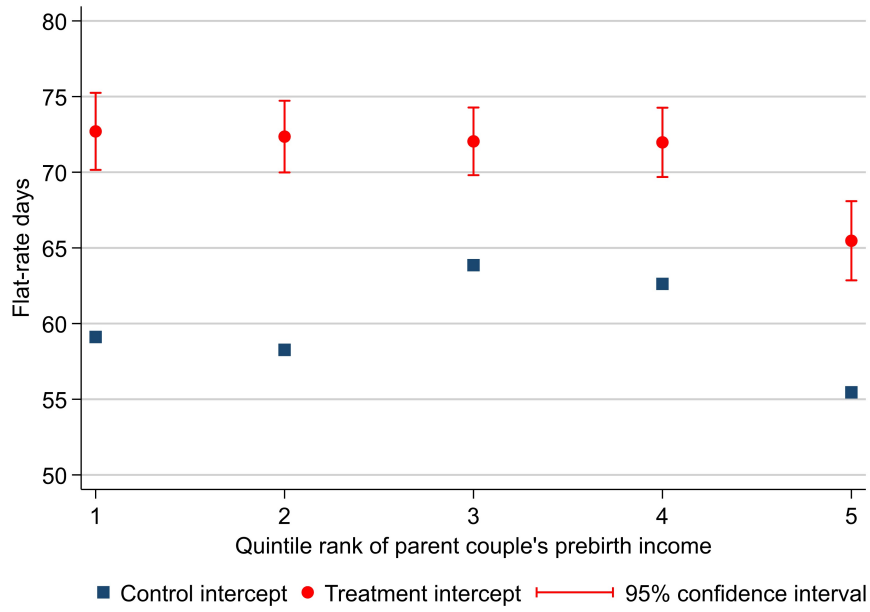
When comparing Figures 1a and 3a, it is striking that the right intercepts are close in levels.²² This suggests that the spillover effect is driven by parents

¹⁹ As inputs we use information on the treatment and control intercepts from extensive margin and ceiling regressions that are fully reported in Appendix H1.

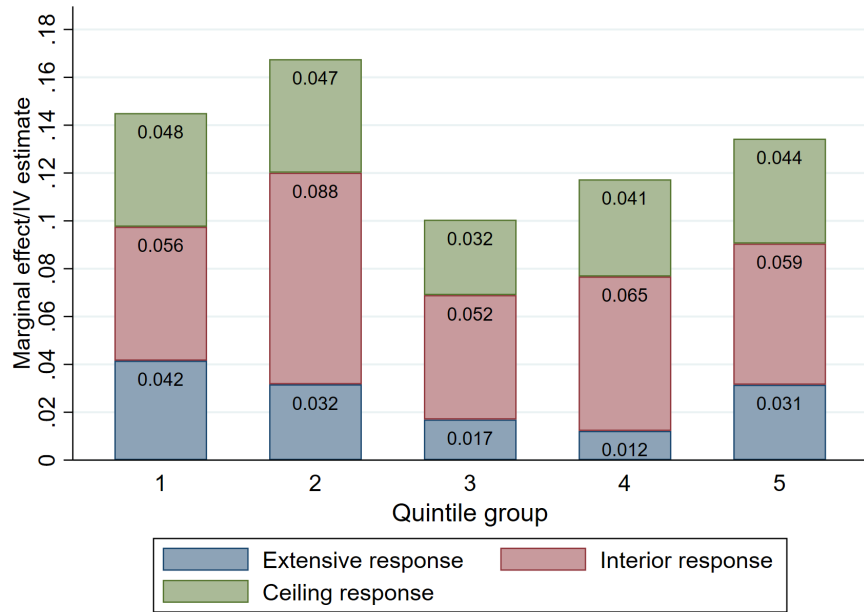
²⁰ See Bastani et al. (2021) for a structured discussion of the relationship between extensive margin responses and levels in the context of female labor force participation.

²¹ We also tested for spillover effects on flat-rate days of older siblings (SEK 60 per day), see Figure F2a of Appendix F.2, who were already in the system when the 2006 child was born. We found no significant spillover effects in this dimension.

²² The samples differ somewhat of course. However, we have estimated the direct effect on the sample with younger siblings, and the qualitative insights are indeed similar.

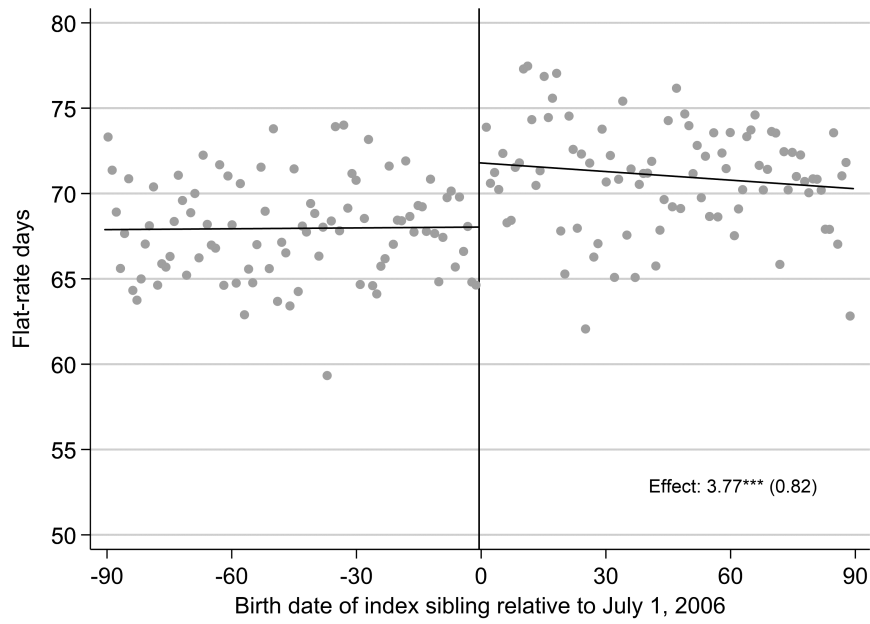


(a) Take-up, by quintile group

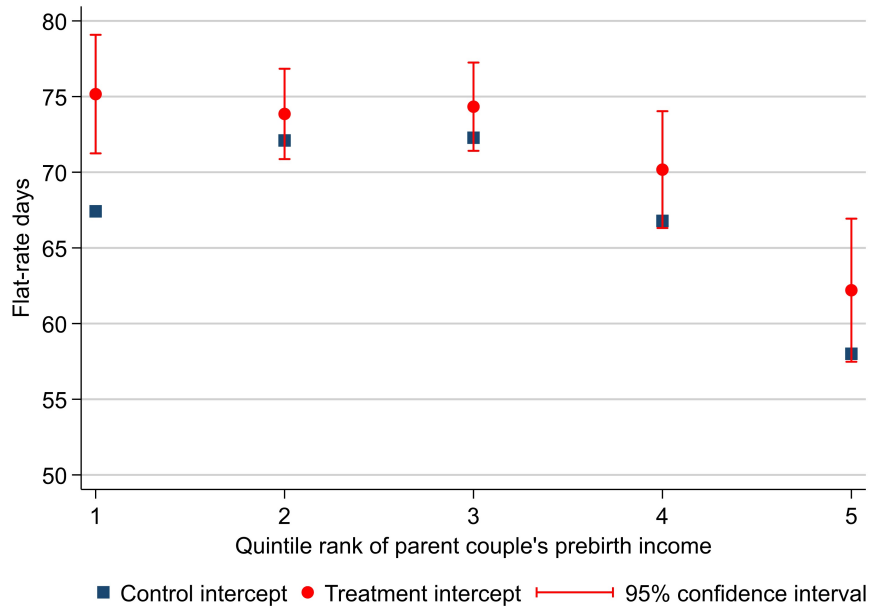


(b) Decomposition of marginal effects, by quintile group

Figure 2: Results by income quintile group. The income measure (earnings) is obtained by first summing the two parents' incomes in the pre-reform years 2003, 2004, and 2005, and then we take an average over the three years.



(a) Overall spillover response



(b) Spillover response by quintile group

Figure 3: Spillover effects on flat-rate days applying to younger siblings. Graph (a) is constructed in the same way as Figure 1a, and (b) follows Figure 2a. The dependent variable is flat-rate days for younger siblings.

with children born to the left of the cutoff. Apparently, parents initially facing low financial incentives do not only take up little benefits for the child born in 2006, but they also take up fewer flat-rate days for the younger sibling.²³ The results suggest that historical take-up behavior is important. Learning and habit formation are plausible mechanisms.

From Figure 3b, which is analogous to Figure 2a, we infer that the spillover effect is driven by the bottom of the income distribution. We know from other studies that behavioral biases such as inertia tend to be greatest among low earners, see for example Jones (2012). Among low earners, it also appears that the direct response is larger among parents of first-born children in 2006, who may be more receptive to new benefit claiming habits.²⁴ Relatedly, Ramnath and Tong (2017) found that a financial incentive to file a tax return gives rise to long-term responses in EITC claiming. It is important to acknowledge the difference between that study and our spillover analysis. In Ramnath and Tong (2017), a financial incentive induces *eligibility* for social benefits administered via the tax code. In our setting, the entitlement structure is the same in the treatment and control groups, but one group of parents was exposed to a higher benefit for their child born in 2006, which impacted on subsequent take-up.

²³There could also be slight differences in disposable income among treated and untreated with implications for take-up. According to this logic, parents with children born before July 1, 2006, will be poorer and therefore take up more benefits for younger siblings – counteracting the ‘habit formation’ effect. However, as already discussed, these differences in disposable incomes are small.

²⁴We re-estimated the direct take-up effect on first-borns versus non-first-borns. If take-up of the flat-rate benefits for the first child determines take-up of benefits for younger children, we expect a larger effect for first-borns. In quantile groups 2-5 the differences were small between first-borns and later-borns. However, in the first quintile there was a sizable difference between the point estimates: 16.57 (std error 2.02) for first-borns vs. 11.73 (1.69) for later-borns).

6 Concluding remarks

A first lesson from this study is that financial incentives play a crucial role in the take-up decision. This may sound trivial, but it has never been shown before in a setting as clean and transparent as ours, where take-up and labor supply incentives are independent. A more non-trivial lesson can be drawn from the substantial positive spillover effects on the take-up of benefits for younger siblings, especially at low incomes. These results are interesting because the existing literature focuses on other policy instruments, such as application assistance (Finkelstein and Notowidigdo, 2019) and information treatments (Bhargava and Manoli, 2015), to increase take-up among the needy. Our analysis illustrates that financial incentives *per se* can mitigate behavioral biases. A final and potentially important lesson applies to child transfer programs more generally: Governments wishing to increase take-up among low-income earners may wish to consider special financial incentives for *first-time* claimants.

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Online Appendix (not for publication)

A Model

A.1 Basic structure

Consider a population of parent couples with a size that is normalized to unity. There are two sources of heterogeneity in the population, pre-tax income z and the take-up technology parameter a . The latter parameter reflects knowledge about the transfer system, application skills, and attitudes toward the government. We allow z and a to be arbitrarily correlated with joint density $f(a, z)$ and cumulative density $F(a, z)$. Both z and a are defined with support on \mathbb{R}^+ . Parent couples derive utility from consumption, C , and disutility from claiming benefits, D . The utility function of the parent couple can be written

$$U(C, D) = v(C) - g(D; a), \quad (\text{A1})$$

where $v(C)$ is the utility of consumption, and $g(D; a)$ is the effort cost of taking up the benefit. We use subscripts of the functions to denote partial derivatives, second derivatives, and cross derivatives. We assume that $v_C > 0, v_{CC} < 0, g_D > 0, g_{DD} > 0$, and $g_{Da} > 0$. The last assumption implies that the marginal cost of taking up an additional unit of benefit increases in the cost parameter a . The binding budget constraint can be expressed as

$$C = z + b \times D - T(z + b \times D), \quad (\text{A2})$$

where D represents units taken up, and b represents the pre-tax value of one unit of the benefit, which is the policy variation of interest. Parent couples earn exogenous income z and pay taxes of $T(z + b \times D)$. We assume that the first derivative of the tax function, which we denote by T' , is in the unit interval, i.e. $T' \in [0, 1]$, and we assume that the second derivative, T'' is non-negative, i.e.

$T'' \geq 0$.²⁵ An essential feature of our setting is also that take-up must be non-negative, $D \geq 0$, and that take-up must not exceed the upper limit (ceiling) \tilde{D} , i.e. $D \leq \tilde{D}$.

A.2 Optimization and comparative statics

If we plug (A1) into (A2) we can formulate the following Lagrangian optimization problem:

$$\max_{D, \lambda, \mu} \mathcal{L} = v[z + b \times D - T(z + b \times D)] - g(D; a) + \lambda D + \mu(\tilde{D} - D), \quad (\text{A3})$$

where λ is the Lagrange multiplier for the non-negativity constraint ($D \geq 0$), and μ is the multiplier for the ceiling constraint ($\tilde{D} - D \geq 0$). A necessary condition for an optimum is

$$\frac{\partial \mathcal{L}}{\partial D} = (1 - T')bv_C - g_D + \lambda - \mu = 0. \quad (\text{A4})$$

The intensive margin. We first consider the pure intensive margin case when none of the constraints bind, i.e. $\lambda = \mu = 0$. Then the parent couple equates the marginal rate of substitution with the (endogenous) marginal benefit of taking up the benefit:

$$\frac{g_D}{v_C} = [1 - T'(z + b \times D)]b. \quad (\text{A5})$$

What happens *ceteris paribus* to take-up when increasing the pre-tax benefit b ? Applying the implicit function theorem we obtain the following derivative

$$\frac{dD}{db} = -\frac{(1 - T')[bv_{CC}(1 - T')D + v_C] + T''bv_C}{v_{CC}b^2(1 - T')^2 - v_C T''b^2 - g_{DD}}. \quad (\text{A6})$$

Since the denominator of (A6) is negative, a sufficient condition for (A6) to be

²⁵For analytical convenience, we have simplified the Swedish tax system as a function of the joint income of the parent couple. In reality, the incomes of the two parents are taxed separately. In addition, we have chosen to represent the parents' choice in a one-period model. This is also a simplification, because flat-rate days can be taken up over an 8-year-period

positive is that $[bv_{CC}(1 - T')D + v_C]$ is positive. Another way of phrasing this condition is that the substitution effect from the benefit increase dominates the income effect. This is a plausible assumption in our setting, because the value of the social benefits is typically small relative to income.²⁶

Repeating the same exercise with respect to exogenous income, z , and the take-up cost parameter, a , we obtain

$$\frac{dD}{dz} = -\frac{v_{CC}(1 - T')^2b - v_C T''b}{v_{CC}b^2(1 - T')^2 - v_C T''b^2 - g_{DD}} < 0 \quad (A7)$$

$$\frac{dD}{da} = \frac{g_{Da}}{v_{CC}b^2(1 - T')^2 - v_C T''b^2 - g_{DD}} < 0. \quad (A8)$$

We can sign both (A7) and (A8) to be negative. Hence, take-up, D , falls in pre-tax income, z , holding a fixed. This is a combined effect of declining marginal utility of consumption and non-decreasing marginal tax rates in income. Equation (A8) is negative as the marginal take-up cost is increasing in the cost parameter a .

The extensive margin. When the non-negativity constraint is binding, we have $D = 0$ and $\lambda > 0$, and thus

$$\left. \frac{g_D}{v_C} \right|_{D=0} > [1 - T'(z)]b. \quad (A9)$$

If the marginal rate of substitution, evaluated at zero take-up, is larger than the

²⁶This can be seen from the Slutsky decomposition $\frac{dD}{db} = \left\{ \frac{\partial D(1-\tau, R)}{\partial b} \right|_u + \frac{\partial D(1-\tau, R)}{\partial R} \right\} (1 - \tau)$, where τ is the linearized marginal tax rate, and R is "virtual income", defined as $R = z + \tau bD - T(z + bD)$. When rewriting the Slutsky relationship in elasticity form we obtain $\varepsilon^u = \varepsilon^c + \varepsilon^I \times \frac{(1-\tau)bD}{R}$, where $\varepsilon^u = \frac{dD}{db} \frac{b}{D} = \frac{dD}{d(1-\tau)b} \frac{b}{(1-\tau)D}$ is the uncompensated elasticity, $\varepsilon^c = \frac{dD}{db} \frac{b}{D} = \frac{dD}{d(1-\tau)b} \frac{b}{(1-\tau)D}$ is the compensated elasticity, and $\varepsilon^I = \frac{dD}{dR} \frac{R}{D}$ is the take-up elasticity with respect to exogenous income. Apparently, when the after-tax value of the benefits is small relative to income, i.e. the ratio $\frac{(1-\tau)bD}{R}$ is small, the income effect of the benefit increase is also likely to be small.

value of the benefit, the parent couple will take up zero units. Equation (A9) holds with equality when the parent couple is indifferent, and we let $\bar{z}(b, a)$ denote the income threshold value for which a larger z will induce a parent couple to take up zero benefits. The corresponding cutoff with respect to a is denoted $\bar{a}(b, z)$

The ceiling response. When the ceiling constraint binds, we have $D = \tilde{D}$ and $\mu > 0$, and hence

$$\left. \frac{g_D}{v_C} \right|_{D=\tilde{D}} < [1 - T'(z + b\tilde{D})]b. \quad (\text{A10})$$

For an indifferent couple, equation (A10) holds with equality. We let $\underline{z}(b, a)$ denote the threshold value of income for which a higher income will induce the couple to take up less than full benefits. There is a similar cutoff with respect to a which we refer to as $\underline{a}(b, z)$.

Cutoff values and segments of the population. It can be shown, once more using the implicit function theorem, that

$$\frac{dz}{da} = \frac{g_{Da}}{v_{CC}(1 - T')^2 b - v_C T'' b} < 0. \quad (\text{A11})$$

This implies $\frac{d\bar{z}(b, a)}{da} < 0$, $\frac{d\bar{a}(b, z)}{dz} < 0$, $\frac{d\underline{z}(b, a)}{da} < 0$, and $\frac{d\underline{a}(b, z)}{dz} < 0$. Therefore, for a given value of a there always exists a unique cutoff with respect to z , and vice versa. For that reason, we can partition the population into three segments based on the two parameters of heterogeneity: $F(\underline{z}, \underline{a})$ couples set $D = \tilde{D}$, $[F(\bar{z}, \bar{a}) - F(\underline{z}, \underline{a})]$ couples set $D \in (0, \tilde{D})$, and $[1 - F(\underline{z}, \underline{a})]$ set $D = 0$.

A.3 Decomposing the take-up response

In expectation, take-up in the population is:

$$E(D) = \tilde{D} \int_0^{\bar{z}(b,a)} \int_0^{\bar{a}(b,z)} f(z,a) da dz + \int_{\bar{z}(b,a)}^{\bar{z}(b,a)} \int_{\bar{a}(b,z)}^{\bar{a}(b,z)} D(b,z,a) f(z,a) da dz + 0 \times \int_{\bar{z}(b,a)}^{\infty} \int_{\bar{a}(b,z)}^{\infty} f(z,a) da dz, \quad (\text{A12})$$

Defining $\bar{D}(b) \equiv \frac{\int_{\bar{z}(b)}^{\bar{z}(b)} \int_{\bar{a}(b)}^{\bar{a}(b)} D(b,z,a) f(z,a) dz da}{\int_{\bar{z}(b)}^{\bar{z}(b)} \int_{\bar{a}(b)}^{\bar{a}(b)} f(z,a) dz da}$ and the vector $x = \{z, a\}$ we can rewrite (A12) as

$$E[D(b)] = \tilde{D} \times F[\underline{x}(b)] + \bar{D}(b) \times \{F[\bar{x}(b)] - F[\underline{x}(b)]\} \quad (\text{A13})$$

Our quasi-experiment provides isolated variation in the before-tax benefit b , while holding other features of the decision environment constant. The effect on $E[D(b_0)]$ from a large change in b can be expressed as

$$\begin{aligned} E[D(b_0 + \Delta b)] - E[D(b_0)] = & \tilde{D} \times F[\underline{x}(b_0 + \Delta b)] - \tilde{D} \times F[\underline{x}(b_0)] \\ & + \bar{D}(b_0 + \Delta b) \times \{F[\bar{x}(b_0 + \Delta b)] - F[\underline{x}(b_0 + \Delta b)]\} \\ & - \bar{D}(b_0) \times \{F[\bar{x}(b_0)] - F[\underline{x}(b_0)]\} \end{aligned} \quad (\text{A14})$$

Adding and subtracting $\bar{D}(b_0 + \Delta b) \{F[\bar{x}(b_0)] - F[\underline{x}(b_0)]\}$ to (A14) we obtain equation (1) in the main text.

A.4 Mapping regression coefficients to the model

It is possible to express the quantities in (1) in terms of regression coefficients. Let α^T refer to a vector of coefficients from the treatment effects regression given by equation (2). Remember that the running variable BD is normalized to be zero at the cutoff. Then we have $E[D(b_0 + \Delta b)] - E[D(b_0)] = \alpha_1^T$. Similarly, we denote the coefficients from the extensive margin regression as α^E , and the coefficients from the ceiling regression as α^C , implying $E\{F[\bar{x}(b_0 + \Delta b)] -$

$F[\bar{x}(b_0)]\} = \alpha_1^E$, and $E\{F[\underline{x}(b_0 + \Delta b)] - F[\underline{x}(b_0)]\} = \alpha_1^C$. The pre- and post-reform levels in (1) can be obtained as

$$\bar{D}(b_0 + \Delta b) = \frac{\alpha_0^T + \alpha_1^T - (\alpha_0^C + \alpha_1^C)\tilde{D}}{(\alpha_0^E + \alpha_1^E) - (\alpha_0^C + \alpha_1^C)}, \quad (\text{A15})$$

$$\bar{D}(b_0) = \frac{\alpha_0^T - \alpha_0^C\tilde{D}}{\alpha_0^E - \alpha_0^C}, \quad (\text{A16})$$

$$F[\bar{x}(b_0)] = \alpha_0^E, \quad (\text{A17})$$

$$F[\bar{x}(b_0 + \Delta b)] = \alpha_0^E + \alpha_1^E, \quad (\text{A18})$$

$$F[\underline{x}(b_0 + \Delta b)] = \alpha_0^C + \alpha_1^C. \quad (\text{A19})$$

The estimated coefficients are reported in Table H1. Plugging these values into (1), while setting $\tilde{D} = 90$ (the maximum number of benefit days), we obtain the decomposition reported in Figure 2b.

B Specification tests

In Table B1 we report covariate balance. For the same bandwidth (+/- 90 days) as in the baseline analysis, we replace the outcome variables with key covariates, namely years of schooling, a dummy for being born in Sweden, age, and pre-reform income. We do this separately for the mother and the father. We see that the point estimates are small and statistically insignificant.

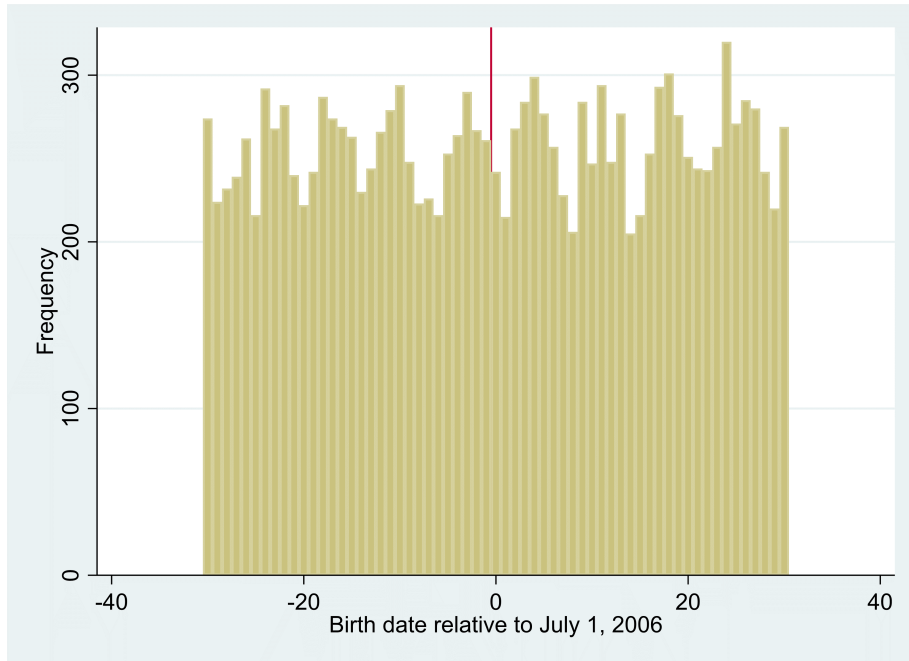
In Figure B1 we graph the frequency distribution of birthdays in our estimation sample at a daily frequency (Figure B1a), and at a weekly frequency (Figure B1b). We conducted two statistical tests on the weekly distribution to determine if there was any manipulation. We first performed the test suggested by Calonico et al. (2017), which produced a p-value of 0.796 on the weekly dis-

Table B1: Balancing of covariates

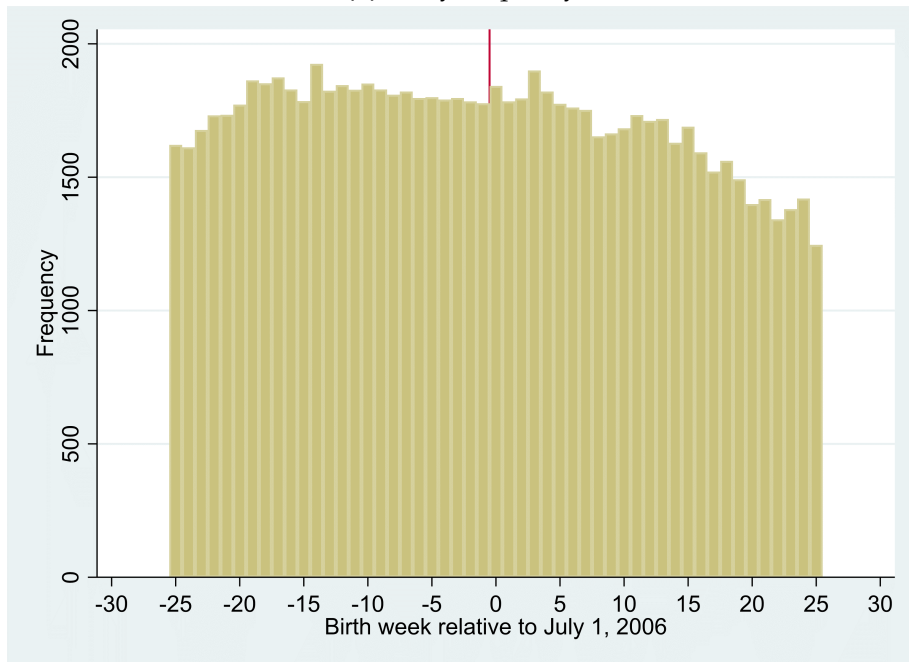
Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Schooling		Born in Sweden		Age		Income t=-1	
Parent:	Mother	Father	Mother	Father	Mother	Father	Mother	Father
RD-estimate	-0.04 (0.04)	-0.05 (0.04)	0.00 (0.01)	0.00 (0.01)	-0.08 (0.09)	0.00 (0.11)	-57 (2,829)	-2,010 (4,215)
Observations	45432	45559	45721	45721	45721	45721	45721	45721
Bandwidth	90	90	90	90	90	90	90	90
Control intercept	12.94	12.57	0.86	0.87	31.12	33.66	188069	308027

Notes: Regressions follow equation (2). Robust standard errors in parenthesis *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The bandwidth is +/-90 days in all regressions.

tribution. Note, however, that this test has been developed for a continuous running variable, while we have a discrete running variable. We therefore also applied the Frandsen (2017) test for manipulation of discrete frequency distributions. We then obtained a p-value of 0.231 on the weekly frequency.



(a) Daily frequency



(b) Weekly frequency

Figure B1: Frequencies around the July 1, 2006, cutoff

Table C1: Robustness

Column: Model:	(1) Baseline	(2) Triangular	(3) Quadratic	(4) Cubic
RD-estimate	11.06*** (0.55)	11.10*** (0.61)	11.18*** (0.84)	11.48*** (1.14)
Observations	45,721	45,721	45,721	45,721
Bandwidth	90	90	90	90
Control intercept	59.88	59.98	60.14	59.80

C Robustness

We assessed the robustness of the baseline treatment effect model, reported in Figure 1a and column 1 of Table C1. In column 2 of Table C1 we give larger weight to observations near the cutoff using a triangular kernel. The result is remarkably close to the baseline estimate. In the two rightmost columns we estimate quadratic (column 3) and cubic (column 4) polynomials at each side of the cutoff. Once again, the results are robust.

In Figure C1 we plot estimated treatment effects, and their confidence intervals, while varying the bandwidth. In all other respects, the specification follows the baseline. The estimates are slightly lower for extremely low values of bandwidth (15 days), but otherwise the estimates are stable.

D Raw distributions

In Figure D1 we compare the raw distributions of utilized flat-rate days for children born in June 2006 and July 2006, i.e. one month on each side of the cutoff on July 1. Both before and after the reform a vast majority of parent couples take up at least *some* flat-rate benefits. From the density distributions displayed in Figure D1a, we see, however, that after the tripling of the benefit, there is a clear reduction in the number of parents with zero take-up. In both groups of parents, there is also a clear spike at the maximum entitlement of 90 benefit days. There is a marked increase in the number of parents who take

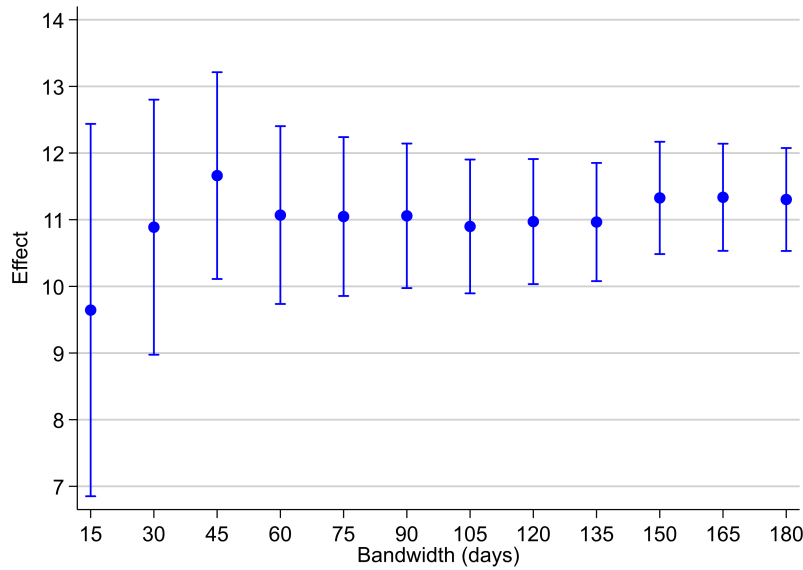


Figure C1: Treatment effect estimates for different bandwidths. 90 days is the baseline.

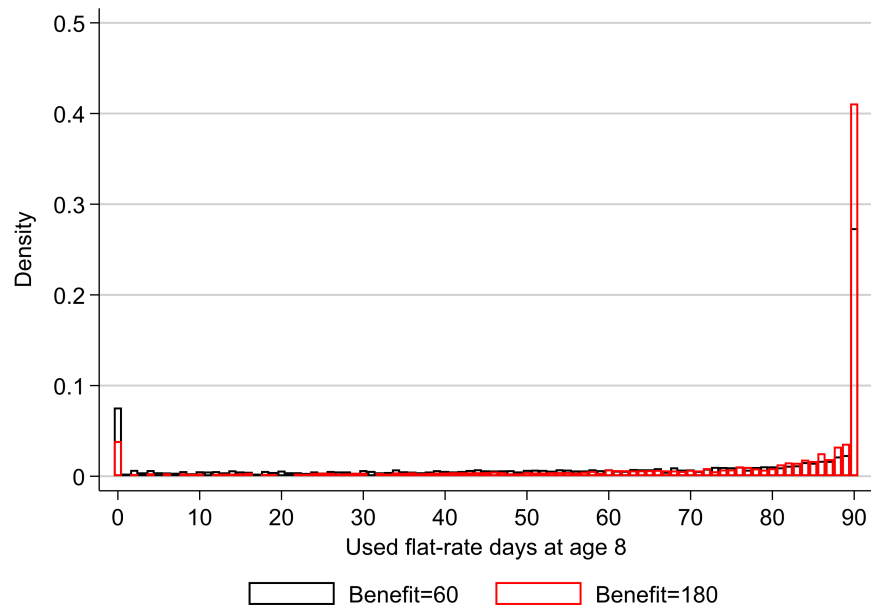
up all 90 benefit days, and over 40% of parents of children born in July 2006 exhaust all benefit days.

From the cumulative density graph (Figure D1b) it is readily inferred that the distribution shifted to the right at interior values of benefit take-up as well. After the reform, fewer benefit days were left on the table, and more people also chose to take up *almost* all 90 days. In sum, the insights from the baseline RD analysis of Figure 1 are captured already in the raw distributions of June and July of 2006.

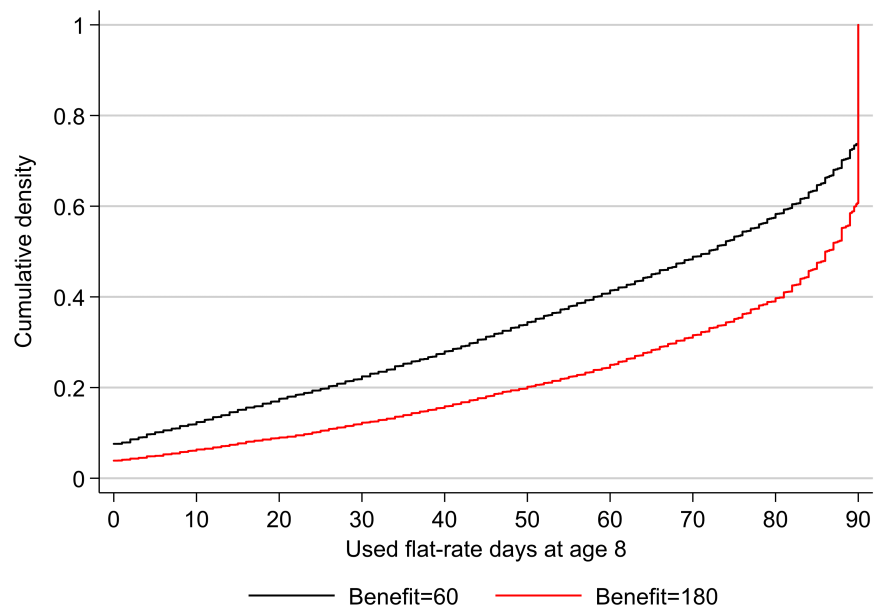
E Other years

Figure E1 shows long-term trends in the take-up of (a) both parents, (b) mothers, and (c) fathers. Prior to July 1, 2006, take-up of flat-rate days was strongly declining over time.

A central lesson from Figure E1 is that the jump on July 1, 2006, is unprecedented. There are no large shifts on July 1 in other years. In Figure E2 we

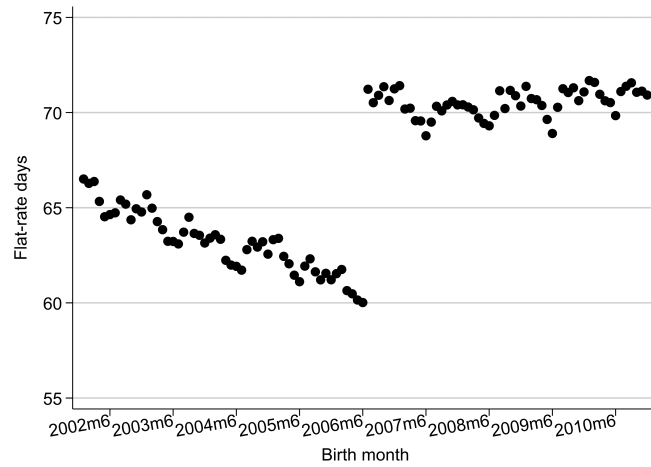


(a) Density

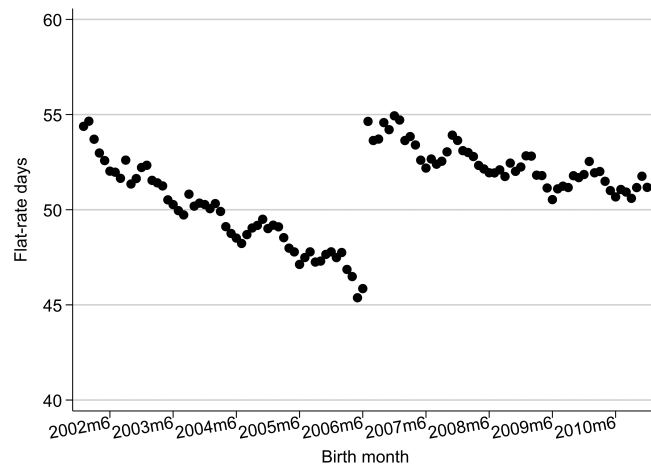


(b) Cumulative density

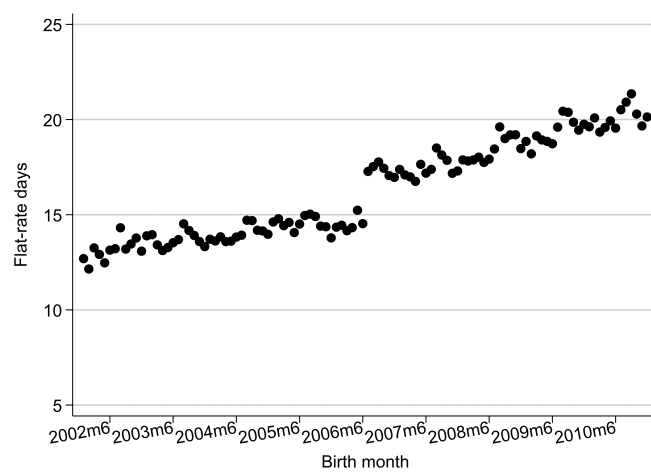
Figure D1: The distribution of flat-rate days, parents to children born in June 2006 and July 2006.



(a) Both mothers and fathers



(b) Mothers



(c) Fathers

Figure E1: Long term trends in take-up

plot RD estimates for July 1, 2002-2010, comparing the 2006 estimate with the placebo estimates for the other years.

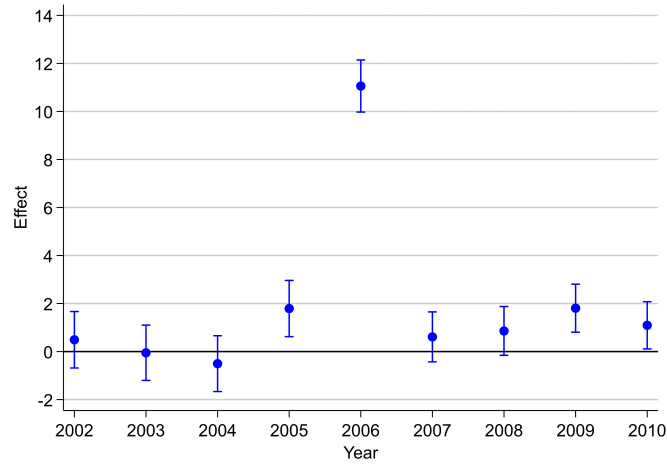


Figure E2: Estimates on July 1 cutoffs 2002-10.

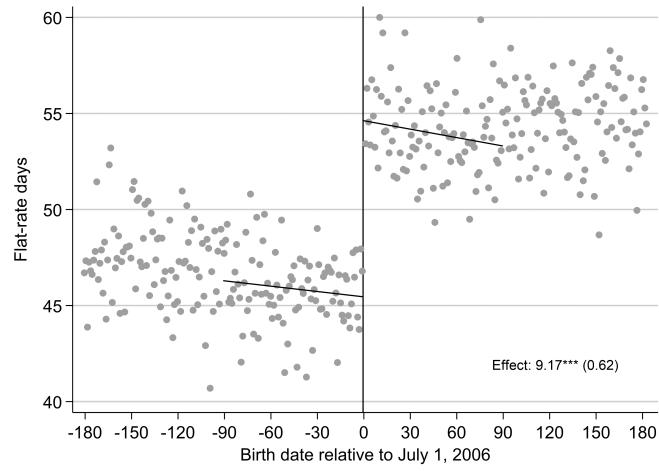
F Additional results

F.1 Gender division

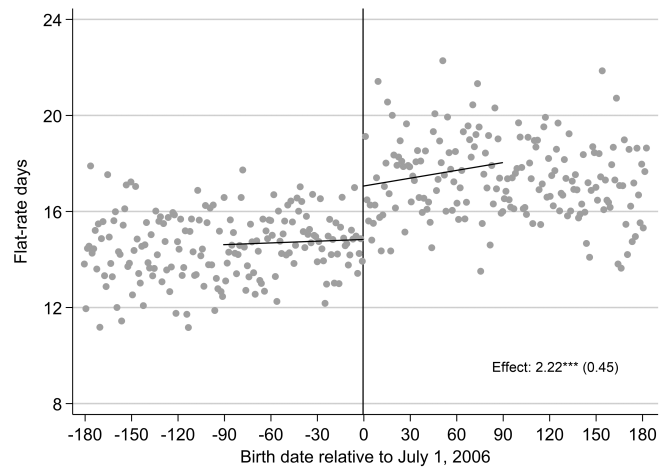
Figure F1 shows the reduced form effects for (a) mothers, and (b) fathers. In absolute terms, the response is larger for mothers than for fathers. However, as the initial level is larger for mothers, the responses are rather similar in percentage terms. When collapsing the share of days taken up by mothers by birth date, we see that there is only a slight increase in the mother's share, which is only borderline significant. The reason why we take averages by day rather than by couple is that some couples do not take up any benefits. In such cases, the share is undefined at the couple level.

F.2 Spillover effects

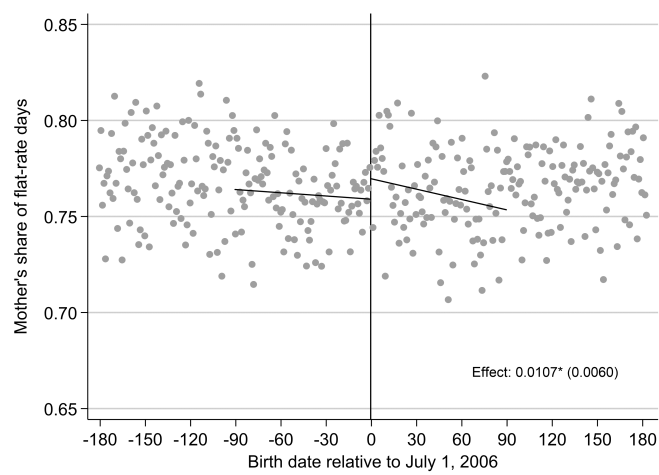
Some parents who were part of the quasi-experiment around July 1, 2006, have more than one child. One possibility is that a parent couple already had children



(a) Mothers



(b) Fathers



(c) Mother's share, by day

Figure F1: Take-up of mothers and fathers

before the July 1, 2006 event. For these older siblings, parents were entitled to the lower benefit level of SEK 60. In Figure F2 we examine whether the take-up of flat-rate benefits applying to siblings reacts among treated parents. There is no significant shift in take-up at the cutoff. Note that there is less time for these parents to respond, and that the benefit is very low.

Another possibility is that parents close to the July 1, 2006, cutoff have *younger* children. This case is discussed in Section 5.4.

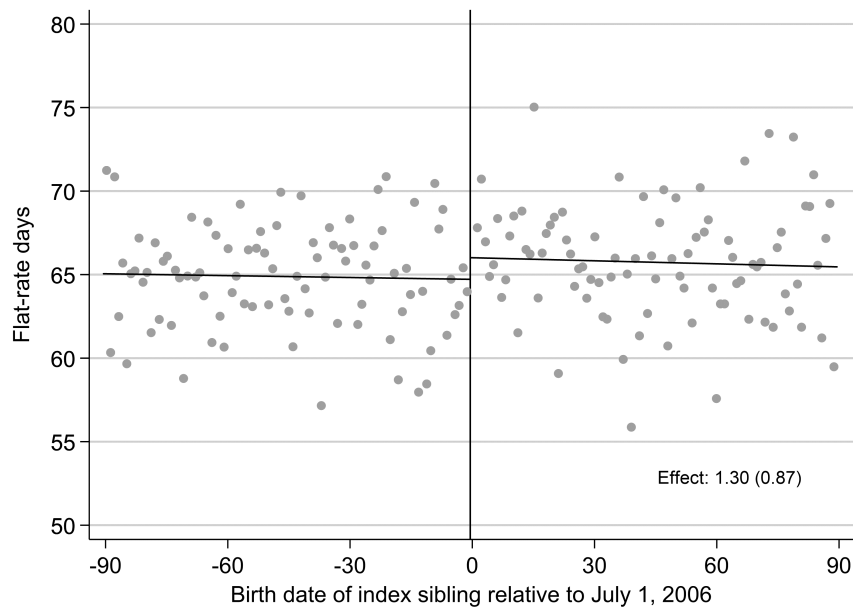


Figure F2: Spillover effects on benefits applying to older siblings (SEK 60).

F.3 Earnings responses

Given that the change in non-labor income is small, especially when viewed over an 8-year period, we do not expect to observe a significant effect on earnings. In Table F1 we report in detail how earnings respond to the July 1, 2006, cutoff. Note that earnings are measured by the calendar year, and not by the age of the child. As some individuals have zero earnings, we do not take logs. There are no significant earnings responses.

Table F1: Effects on earnings

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Income during:	All years	2006	2007	2008	2009	2010	2011	2012	2013	2014
A. Mothers										
RD-estimate	-26,990 (23,425)	-1,220 (1,979)	-2,486 (2,092)	-5,411* (2,865)	-3,359 (3,057)	-1,263 (3,196)	-4,225 (3,268)	-2,894 (3,400)	-3,762 (3,544)	-2,369 (3,914)
Observations	45721	45721	45721	45721	45721	45721	45721	45721	45721	45721
Bandwidth	90	90	90	90	90	90	90	90	90	90
Control intercept	1.849e+06	108505	96804	193079	193640	211419	235232	252648	269048	288680
B. Fathers										
RD-estimate	9,560 (41,648)	-1,258 (4,551)	2,830 (4,888)	2,992 (5,648)	2,275 (4,978)	217 (5,361)	-210 (5,163)	1,791 (5,363)	2,849 (5,461)	-1,927 (5,969)
Observations	45721	45721	45721	45721	45721	45721	45721	45721	45721	45721
Bandwidth	90	90	90	90	90	90	90	90	90	90
Control intercept	3.321e+06	325103	315375	348705	354846	368955	381684	395140	405876	425563
C. Parent couple										
RD-estimate	-17,430 (53,469)	-2,478 (5,419)	343 (5,422)	-2,418 (6,786)	-1,084 (6,401)	-1,046 (6,815)	-4,435 (6,769)	-1,103 (7,056)	-913 (7,260)	-4,296 (7,904)
Observations	45721	45721	45721	45721	45721	45721	45721	45721	45721	45721
Bandwidth	90	90	90	90	90	90	90	90	90	90
Control intercept	5.170e+06	433609	412179	541785	548486	580373	616916	647788	674924	714243

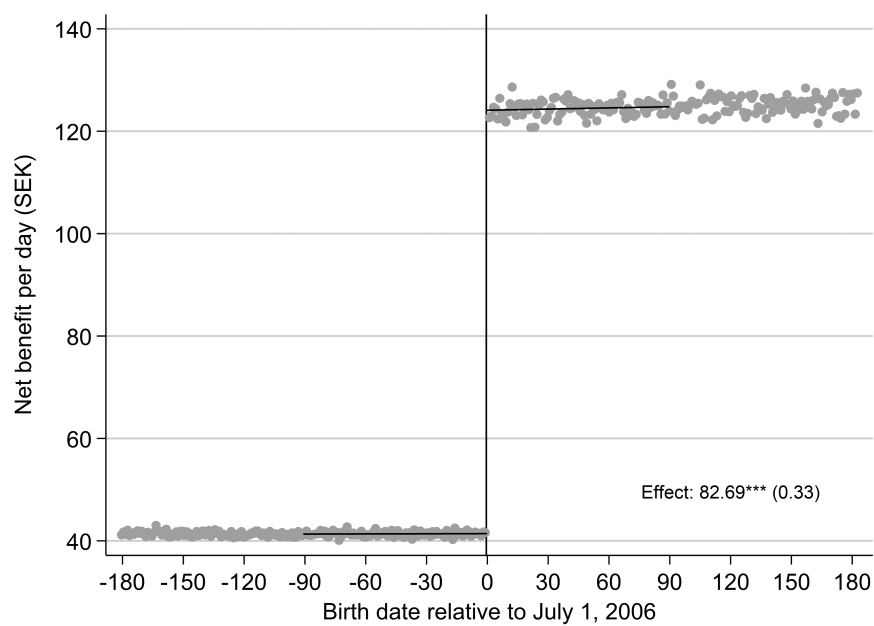
Notes: Regressions follow equation (2). Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The bandwidth is +/-90 days in all regressions.

G First stage graphs

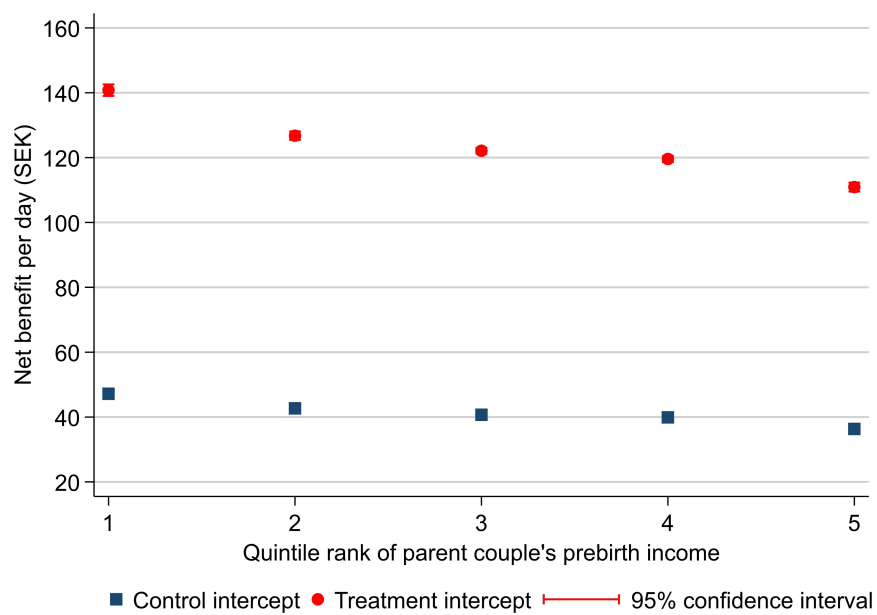
The first stage to the IV regression is illustrated in Figure G1a. The variation across bins arises because parents have different incomes. The first stages for the quintile groups are represented in Figure G1b, which is analogous to Figure 2a. The graph contains confidence intervals, but these are extremely tight.

H Marginal effects and elasticities

In Table H1 we report all coefficients and intercepts necessary to infer the main results in our analysis.



(a) First stage: total sample



(b) First stage estimates and standard errors, by income quintile group

Figure G1: First stage graphs

Table H1: Elasticities and results in table format

Quintile group	1	2	3	4	5	All
A. First stage: After-tax benefit						
First stage estimate	93.62*** (0.9022)	84.07*** (0.6591)	81.40*** (0.4861)	79.67*** (0.5026)	74.58*** (0.7081)	82.69*** (0.3300)
Control intercept	47.18	42.69	40.71	39.90	36.33	41.41
Percentage change	198.43	196.93	199.95	199.67	205.28	199.69
B. Overall days						
Treatment effect	13.58*** (1.30)	14.09*** (1.21)	8.18*** (1.14)	9.35*** (1.17)	10.02*** (1.33)	11.06*** (0.55)
Control intercept	59.12	58.26	63.86	62.62	55.46	59.88
IV estimate	0.1451*** (0.0139)	0.1676*** (0.0145)	0.1005*** (0.0140)	0.1174*** (0.0147)	0.1343*** (0.0178)	0.1337*** (0.0067)
Implied elasticity	0.116	0.123	0.064	0.075	0.088	0.092
C. More than 0						
Treatment effect	0.0628*** (0.0114)	0.0413*** (0.0093)	0.0216*** (0.0078)	0.0151* (0.0082)	0.0396*** (0.0114)	0.0363*** (0.0044)
Control intercept	0.886	0.921	0.953	0.956	0.902	0.924
IV estimate	0.00067*** (0.00012)	0.00049*** (0.00011)	0.00027*** (0.00010)	0.00019* (0.00010)	0.00053*** (0.00015)	0.00044*** (0.00005)
Implied elasticity	0.036	0.023	0.011	0.008	0.021	0.020
D. Full take-up						
Treatment effect	0.1585*** (0.0206)	0.1562*** (0.0195)	0.0984*** (0.0195)	0.1259*** (0.0193)	0.1066*** (0.0184)	0.1288*** (0.0087)
Control intercept	0.338	0.248	0.275	0.245	0.207	0.263
IV estimate	0.0017*** (0.0002)	0.0019*** (0.0002)	0.0012*** (0.0002)	0.0016*** (0.0002)	0.0014*** (0.0002)	0.0016*** (0.0001)
Implied elasticity	0.236	0.320	0.179	0.257	0.251	0.245
Observations	9028	9155	9238	9195	9105	45721

Notes: Let $\hat{\gamma}_1$ denote the first stage estimate and $\hat{\gamma}_0$ the control intercept from the first stage regression "After-tax benefit_i = $\gamma_0 + \gamma_1 \text{High}_i + \gamma_2 \text{BD}_i + \alpha_3 \text{BD}_i \times \text{High}_i + u_i$ ". The "percentage change" in panel A is given by $\frac{\hat{\gamma}_1}{\hat{\gamma}_0} \times 100$. For a given outcome and group, the reduced form estimates reflect $\hat{\alpha}_1$ of equation (2), and are reported graphically in Figure 1 and Figure 2a. In panels B-D, "Control intercept" refers to $\hat{\alpha}_0$ of equation (2). The IV estimates correspond to $\hat{\beta}_1$ of equation (3). The IV estimates in Panel B, columns 1-5, are graphically shown in Figure 2b. The elasticities are given by $\frac{\hat{\alpha}_1/\hat{\alpha}_0}{\hat{\gamma}_1/\hat{\gamma}_0}$. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The bandwidth is +/-90 days in all regressions.