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Explaining benefit take-up behavior – the role of incentives and habits*

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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 sizeable spill-over effects on subsequent take-up of low-income earners.

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

JEL Classification: H24; J22.

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1 Introduction

Families with children do not always claim social benefits they are entitled to, 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 to alleviate poverty is to increase the generosity of existing programs. How do take-up rates react to such policies? If take-up is highly sensitive to benefit generosity, the effect on public expenditures will exceed the mechanical expenditure response, and a key issue is whether vulnerable low-income parents increase their take-up.[1] Moreover, as highlighted by Bhargava and Manoli (2015), policy interventions may impact on subsequent take-up behavior due to mechanisms like persistence and inertia. To get a complete picture of the take-up response, such spillover effects must be accounted for.

We study take-up responses to higher benefits using novel 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. Hence, there is no financial reason to leave the child benefits on the table, and the benefit can be viewed as an unconditional child cash transfer requiring active claiming. 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. The mere fact that a huge majority of parents are "insiders" implies that some conventional explanations of incomplete take-up, such as stigma costs and complexity in application procedures, are less important. We study how the benefit

[1] The seminal normative analysis of Nichols and Zeckhauser (1982) suggests that transfer policies should impose lower take-up costs on genuinely needy individuals.

entitlement affects take-up in a setting in which claiming is easy, but people have limited information about how to take full advantage of the system. The results are relevant to government programs (or more narrowly child transfer programs) that requires active claiming.

For identification, we exploit a birthday discontinuity in the benefit level, implying a tripling of the benefit level for parents with children born after July 1, 2006. Using full population Swedish administrative data, we first estimate the direct effect of the benefit increase on take-up. As take-up of flat rate days can be in the range from zero to 90 days, we pay special attention to the role of extensive margin and ”ceiling” responses. We then estimate subsequent spillover effects on take-up of flat rate days pertaining to younger siblings. In this way, we gain insights into the role of habit formation in take-up behavior. Throughout our analysis, we pay special attention to differential effects with respect to the parents' income.

Beginning with the direct response, we find that non-take up is reduced by more than one third as a causal effect of the reform – average take-up jumps from 60 to 71 out of 90 days. Given that the benefit level tripled, this result does perhaps not come as a surprise. Most of the action in take-up is associated with an interior response, even though we also observe a sizeable increase in the fraction of parent couples locating at the ceiling of 90 days. A central finding is that the reform is leveling take-up rates across the income distribution. In the low-benefit regime take-up was lower among low-income than middle-income parents.

By examining the subsample of parent couples who get a new child, and therefore become eligible for 90 additional days at the post-reform benefit level, we test for spillover effects in take-up behavior. Interestingly, we provide graphical evidence of a significant spillover response of 3.8 days (one third of the direct response). Our interpretation is that parents who due to a low benefit level claimed few benefits for their 2006-born children also were less prone to claim benefits for their later born children. Apparently, habit formation seems to be important in benefit claiming. The spillover effect is heavily concentrated to low-income 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 leads to an error in the dependent variable (take-up) which 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. This is not an issue in our setting, because all parents are entitled to the same pre-tax benefits.

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

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

sectional variation in benefit levels.[3] Ideally, researchers would prefer to randomly allocate different benefit levels to different individuals/households and compare their take-up responses. However, it is considerably easier to conduct field experiments in which information or application assistance varies across individuals. For instance, Bhargava and Manoli (2015) and Linos et al. (2022) randomize how the benefit level of the U.S. earned income tax credit (EITC) is advertised, but not the benefit level itself.

In the US, social policies like 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 increased among low-income households eligible for a stimulus check of USD 300 in 2008. The stimulus check also had a large causal effect on filing in subsequent years, which, in turn, led to increased workforce attachment, earnings, and EITC claiming. While 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 direct effects of financial incentives on benefit take-up is close to being a randomized experiment.[4] Anderson and Meyer (1997) exploit changes to the

[3]The literature has recently been reviewed by Ko and Moffitt (2022).

[4]By 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).

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 who had twins (entitled to double benefits) and households having 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 enrich the existing literature on take-up responses to financial incentives in several ways. First, our identification strategy is extremely 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. Thus, 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 in a novel way.

3 Institutional setting and reform

3.1 Parental leave benefits in Sweden

In the Swedish parental leave system, parents claim benefits for particular dates when the parent spends time with his/her children. 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 grade 1) but benefits not taken up by that date could be claimed retroactively. 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]

Technically, parents were allowed to claim flat rate days on Saturdays and Sundays without applying for benefits on adjacent Fridays and Mondays. As

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

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

[7]Tax 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%.

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: SGI days required 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 leave days useful.[10] Consequently, one should not expect parents to take out all flat rate days in 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.

From other contexts we know that complexity is important for the take-up of social benefits (Blumkin et al., 2020). From a practical perspective, it was easy to apply for the flat rate benefits. The flat rate and SGI benefits were part of the

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

[9]Parents' knowledge of the SGI and flat rate days have been surveyed at a couple of occasions during the last 20 years. To summarize, these studies indicate that mothers know more than fathers. Unfortunately, these studies are not, however, 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.

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

same application system, and take-up rates for the generous SGI benefits were very high (around 95 % in our sample). The mere fact that a huge majority of parent couples applied for at least some flat rate benefits, even when the benefit was very low, see Figure 1b below, suggests that most parents knew how to apply. The use of internet applications and digital bank ID increased during the claiming period 2006-2014. Parents could apply for several spells of flat rate days (e.g., on weekends) at one and the same time.

3.2 The 2006 reform

Parents to 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, i.e., SEK 41. Going from zero to a maximum of 90 parental leave days on average increased disposable income by $41 \times 90 =$SEK 3690 (USD 374). This benefit level was arguably low – the pre-tax daily benefit amounted to around 7 % of average full time equivalent daily wage in 2006. The benefit can also be contrasted with the average pre-tax SGI-day benefit for men (women) having children 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 applying to 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 2023), the nominal pre-tax flat rate benefit is still SEK 180.

The reform was announced on March 16, 2006 (prop. 2005/06:142) and 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. [11]

4 Data and sample selection

4.1 Data

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. We observe for which calendar days a parent received parental leave benefits, and the number of applications. Crucially, we are able

[11]The ceiling for SGI compensation was also increased on July 1, 2006 as part of the same reform package. This reform does not pose a threat to identification, because it applied to everyone who were claiming benefits at that point in time. There was no discontinuity between parents to children born before and after July 1.

to distinguish between flat rate benefit days and SGI benefit days.[12] 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.

4.2 Sample selection

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 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

[12]According 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.

treatment effect in the sample we are considering.[13] 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.

5 Conceptual 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.[14] 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. To rationalize non-take up, we assume that take up is costly due to e.g., poor knowledge about the rules, application costs, and psychological costs. Couples trade off the utility from consumption against the disutility from claiming benefits, and they set D to maximize the utility function $U(C, D) = v(C) - g(D; a)$. $v(C)$ 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, cou-

[13] We 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.

[14] In 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.

ples 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. $F[\overline{x}(b)]$ 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, $F[\overline{x}(b)] - F[\underline{x}(b)]$ will locate 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 $F[\overline{x}(b_0)]$ to $F[\overline{x}(b_0 + \Delta b)]$. 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.[15] 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 $\overline{D}(b)$ denote mean take-up among couples with $D > 0$ and $D < \tilde{D}$, we can write the interior response as $[\overline{D}(b_0 +$

[15]Here 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.

$\Delta b) - \overline{D}(b_0)]$. 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:

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

6 The empirical model

6.1 The RD model

Our empirical setup is a sharp regression discontinuity (RD) model, with birthday (measured at a daily frequency) as the running variable. All parents of children born on July 1, 2006, and onwards 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

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

where High_i is an indicator variable that takes the value of 1 if the child is born July 1 and after, and zero otherwise. BD is birthday in numeric format, with July 1 normalized to be zero. We estimate different linear slopes at each side of the cut-off, 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.

Our most central outcome variables are (a) average take-up of benefit days, $E[D(180)] - E[D(60)]$, (b) the probability to take up a positive amount of benefits, $F[\overline{x}(180)] - F[\overline{x}(60)]$, and (c) the probability to take up all 90 days, $F[\underline{x}(180)] - F[\underline{x}(60)]$). For 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.

6.2 Specification tests

We examined covariate balance at the cut-off with respect to the two parents' years of schooling, the probability of being born in Sweden, age, and income of 2005. There are no significant effects at the cut-off, see Table B1 of Appendix B. In Appendix B we also graph the frequency distributions of births, centered at the cut-off. 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 running variables, we obtain a p-value is 0.231 when collapsing by week. This suggests that there is no significant shift in the underlying distribution at the birthday cut-off. 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.

6.3 Marginal effects and elasticities

We exploit quasi-experimental variation in the pre-tax benefit level b. However, after-tax benefits matter for consumption. Flat rate benefits are taxed progressively with earned income and other taxable social benefits. Since flat rate benefits are added to one of the parents' taxable incomes, after-tax benefits are typically higher at low incomes, and lower at high incomes.[16] To obtain the "marginal effect" on take-up from a one-unit (SEK) increment in the after-tax benefit we estimate

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

by 2SLS. The first stage equation is given by (2), with "After-tax benefit$_i$" on the left hand side. In other words, we scale the reduced form effect with the average increase in the after-tax benefit. The scaling is particularly important when comparing responses in different subgroups, where couples experience

[16] When calculating after-tax benefits we use the tax schedule and incomes of 2014, i.e. the last year the parent couple is eligible for flat rate benefits. In these calculations we assume that the parent facing the lowest marginal tax rate takes up the benefit.

different treatment doses. A graphical representation of the first stage is to be found in Appendix G. In the spirit of e.g. Anderson and Meyer (1997) we also report take-up elasticities, which we define as the percentage change in outcome (a)-(c) following a percentage change in the after-tax benefit.

7 Baseline results

7.1 The reform effect at the cut-off

Figure 1a reports the effect of the benefit increase on average 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. This is indeed a huge jump, but 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 0.09. This sounds like a modest elasticity, but the reader should note 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 $\frac{30}{60}/\frac{120}{60} = 0.25$. Non-take up, expressed in units of benefits, is reduced by more than one third in the reform.

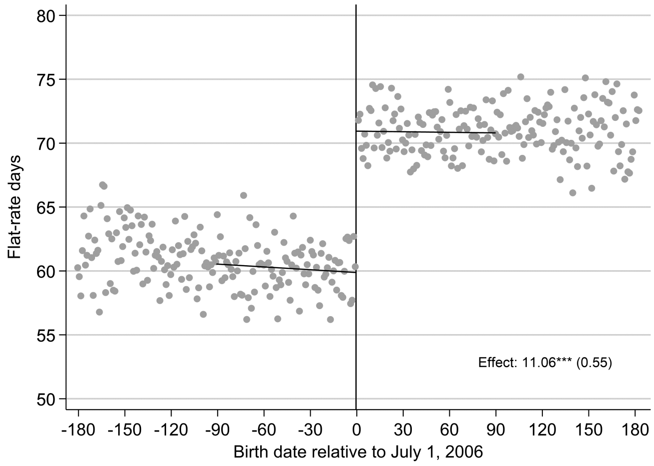
Figure 1b illustrates the effect on the extensive margin, i.e. the probability to take up any flat rate benefits. As already pointed out above, only a small fraction, around 8% of parents, took up zero benefits when the benefit level was very small. Nevertheless, there is a sizable extensive margin response of about 4.6 percentage points.

Finally, we examine the ceiling response, i.e., the probability to take up all flat rate days. There is more room for an aggregate response along this margin, because only 26% exhausted all 90 flat rate days under the low benefit regime. Figure 1c shows that the ceiling response was both large and precise: The fraction with full take-up increased by around 13 percentage points. Apparently, after the benefit increase, more parents thought it was worth the effort to take up all days. Still, a majority (62%) of parent couples took up less than 90 days after the benefit increase.

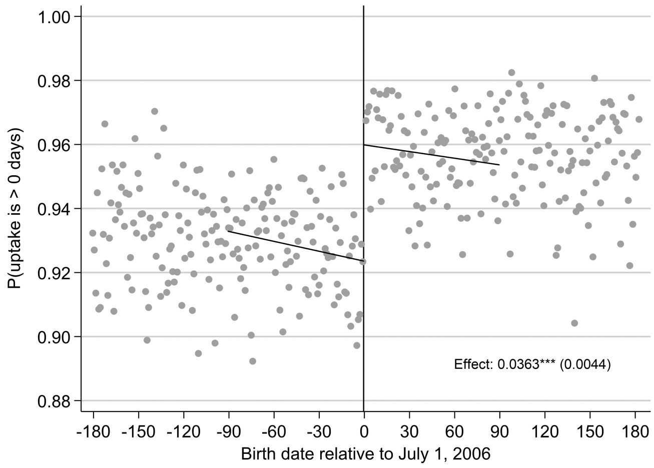
The findings illustrated in Figure 1 are remarkably clear, and in Appendix C we demonstrate that results are robust to alternative bandwidth choices, weighting procedures, and polynomial choices. Moreover, in Appendix D we graph the *raw* density and cumulative density distributions of flat rate days for parents of children born in June, 2006, and in July, 2006, respectively. Strikingly, 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.

7.2 Other outcomes

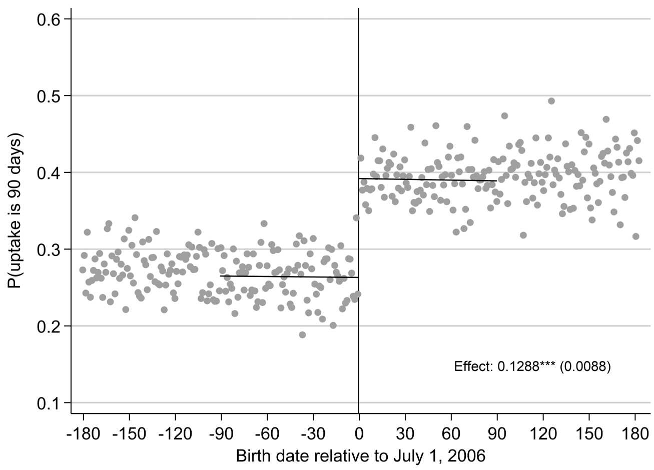
Gender division. Studies on the utilization of parental leave benefits typically focus on the gender division of benefit take-up, see e.g.Moberg (2019), Ekberg et al. (2013), and Rosenqvist (2022) for Sweden. As flat rate days are part of the Swedish parental leave system and benefit claiming is made either by the father



(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.

or the mother, our paper, at least to some extent, relates to this literature. Still, our research question is conceptually distinct, because take-up of flat rate days does not require work absence, and we therefore in practice analyze an unconditional cash-transfer to parents with small children. 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]

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, it should be noted that the windfall is small, especially when viewed from an 8-year perspective. The average after-tax benefit increases 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 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 have little power to detect those. Unsurprisingly, we estimate earnings effects that are insignificantly different from zero. This holds for the parent couples' earnings, and for both parents separately. It also holds for the entire period 2006-2014, and it holds for each individual calendar year, see Table F1 of Appendix F.3.

[17]This 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).

7.3 Take-up by income level

In general, it matters for policy whether more generous social benefits increase disposable incomes for the poor or for the rich. In this Section, we partition the sample into five quintile groups based on the parent couples' pre-reform income. We construct this income measure by first summing the two parents' incomes in the pre-reform years 2003, 2004, and 2005, and then we take an average over these three years. Income includes earnings and taxable social benefits.

In our simple theoretical framework, three distinct mechanisms may generate differential take-up rates across the income distribution. 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 first group to SEK 36 in the fifth group. Third, pre-tax 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.

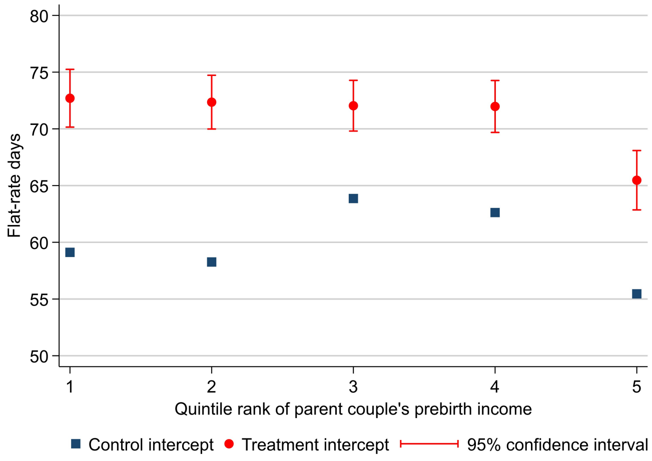
Figure 2 illustrates estimated take-up just to the left of the birthday cut-off (control intercept), and just to the right of the cutoff (treatment intercept). To begin with, we focus on ″pre-reform″ take-up under the SEK 60 regime, i.e.

the control intercepts.[18] Figure 2a shows that there are no dramatic differences across income groups. Average take-up was, however, the highest among middle income couples in quintile groups 3 and 4. This is interesting, and it suggests that other factors than declining marginal utility of income and the after-tax benefit level are important for the take-up decision, even though high income couples in quintile group 5 exhibited the lowest take-up. Turning to the extensive margin (Figure 2b) we see that pre-reform take-up followed a pronounced inverse U-shaped pattern. Despite their larger economic incentives to take up benefits, a relatively large fraction of low-income couples left all flat rate benefits on the table. The pre-reform income gradient in take-up was very different when looking at the share taking up all 90 days (Figure 2c). Considerably more couples, as a fraction of couples both with positive and zero take-up, took up all 90 days before the reform in the first quintile group.

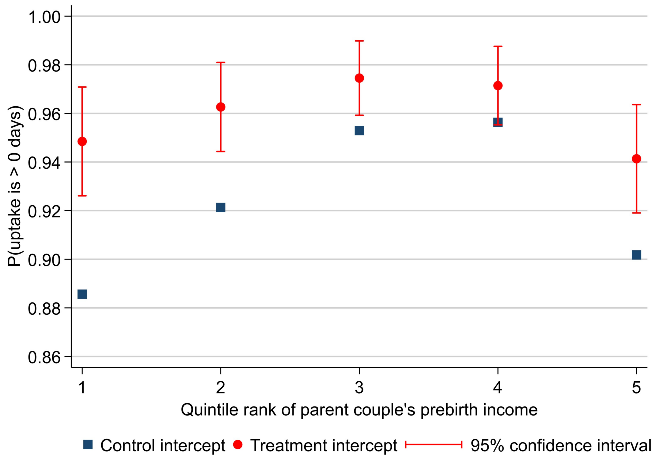
Figure 2 illustrates the profound causal impact of the benefit increase on take-up levels across the distribution. Figure 2a provides a good summary: The reform is leveling take-up rates in quintile groups 1-4, while average take-up still is somewhat lower in quintile group 5 after the reform. In Section 8 we will further discuss the scaled treatment effects. Turning to the extensive margin response, we see that there is a strong relationship between the pre-reform take-up level and the magnitude of the response. Intuitively, if the pre-reform level is quite close to 100%, which is the case among middle income earners, the extensive margin has a hard time to increase.[19] After the reform, the fraction of

[18]We use the term ”pre-reform” when referring to the control intercepts. One should keep in mind that there is no temporal dimension involved: Treated and untreated couples coexisted during the claiming period 2006-2014.

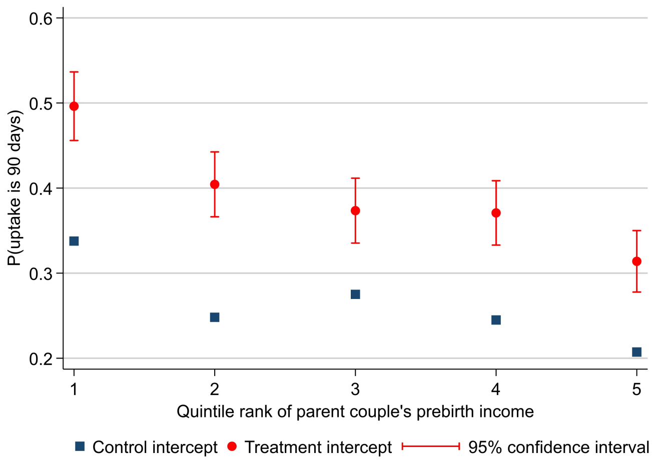
[19]See Bastani et al. (2021) for a structured discussion of the relationship between extensive margin responses and levels in the context of female labor force participation.



(a) Overall response



(b) Extensive margin



(c) Ceiling response

Figure 2: Results by income quintile. The vertical distances between the two dots reflect the treatment effect, with the 95% confidence interval of the treatment effect centered around the treatment intercept (red dot). The specifications follow equation (2), estimated on each specific quintile group. Robust standard errors. Fixed bandwidth of +/-90 days.

couples who take up flat rate benefits is almost as large in quintile group 1 as in groups 3 and 4. This transformation of the gradient in take-up, which is caused by the benefit reform, is indeed dramatic. By contrast, the ceiling response illustrated in 2c follows a different logic. Even though the fraction taking up all 90 days was the largest in quintile group 1 before the reform, the response is the largest in this group. After the reform, 50% of couples in quintile group 1 take up all days, while the fraction is lower than 40% in the other groups.

7.4 Spillover effects

A substantial proportion of children born around July 1, 2006, have siblings. Notably, the benefit levels of flat rate days applying to siblings do not change at the cut-off. For older siblings, parents were entitled to the low pre-tax benefit of SEK 60, while parents were entitled to SEK 180 per benefit day for younger siblings. Figure 3 illustrates the effect on the number of days taken up for younger siblings. In 3a we estimate a large overall effect of 3.77 days. The point estimate amounts to one third of the direct effect, which is 11.06 days. From Figure 3b we infer that the effect is the largest at the bottom of the income distribution. Figure F2a of Appendix F.2 reveals that there was no such significant spillover effect for older siblings (SEK 60 per day). As the initial increase in disposable income is low in the treated group over an 8-year period, we do not expect income effects to matter. If anything, income effects would lower take-up of benefits pertaining to the later born child, and therefore push the estimated spillover effects downwards.

It is striking when comparing the direct effect of Figure 1a and the spillover effect of Figure 3a, that the right intercepts are approximately the same.[20] This suggests that the spillover effect is driven by parents with children born to the left of the cut-off. Our interpretation is that the low take-up of benefits for the older child is inherited to take-up for younger children. It is also interesting that the largest spillover effect is to be found at the bottom of the distribution. Apparently, habits and inertia are more important at low incomes.[21]

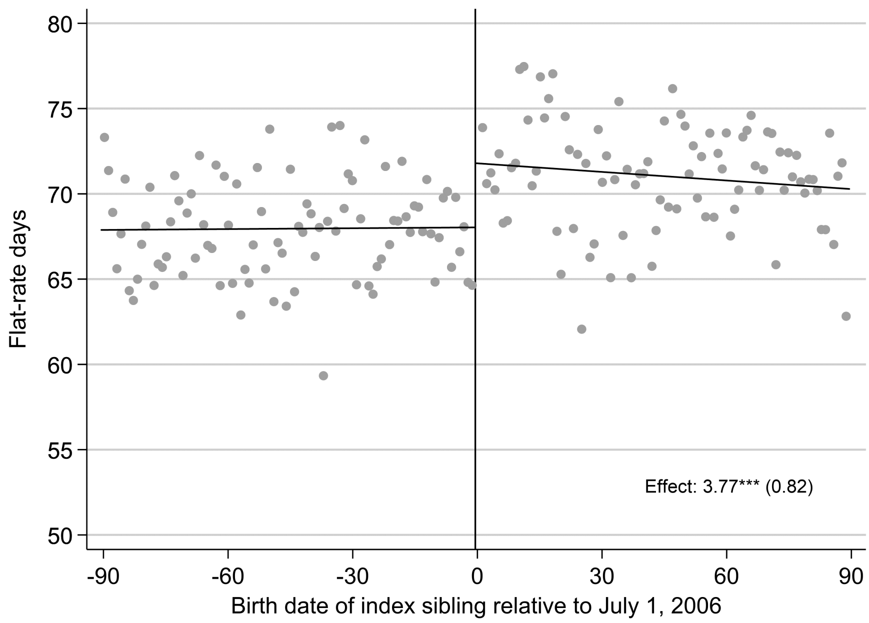
Relatedly, Ramnath and Tong (2017) found that a financial incentive to file a tax return gives rise to long-run responses in EITC claiming. It is important to acknowledge the difference between that study and our spillover analysis. In Ramnath and Tong (2017), a financial incentive induces *eligibility* to 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 were exposed to a higher benefit for their child born in 2006, which impacted on subsequent take-up.

8 Scaling and decomposing the response

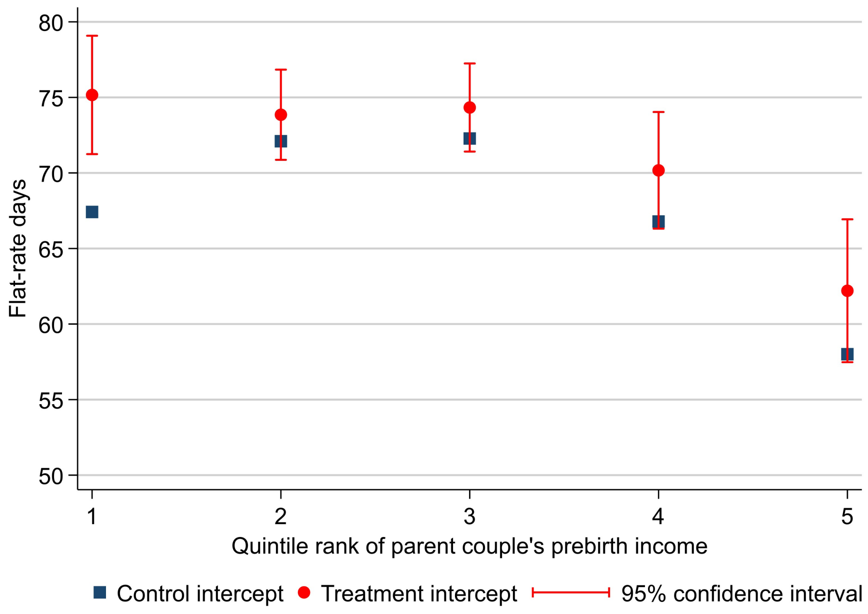
The interpretation of the estimated effects must also account for that the treatment dose is declining in income as flat rate benefits are taxed progressively together with income. Figure 4a plots marginal effects from a SEK increase in the

[20]One should keep in mind, however, that the samples differ, because only a fraction of parent couples in the main sample have younger children. However, we have estimated the direct effect on the sample with younger siblings, and the qualitative insights are similar.

[21]We have re-estimated the direct take-up on first-borns vs 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).



(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.

benefit level, reflecting $\hat{\beta}_1$ of equation (3) by quintile. Moreover, using equation (1), and the treatment and control intercepts from the regressions reported in Figure 2, we decompose the marginal effects into an extensive margin response, interior response, and ceiling response following equation (1). In Figure 4b we do the same thing for spillover effects.

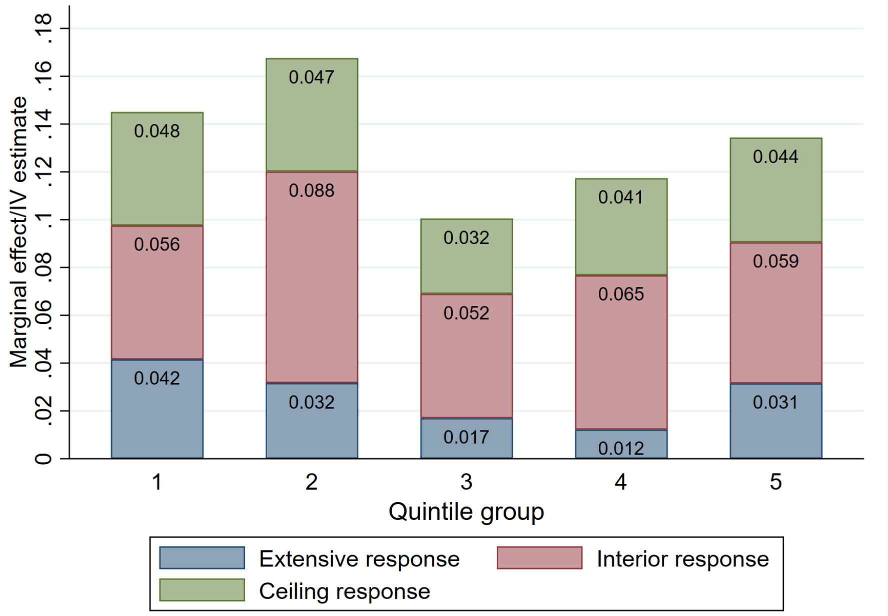
Marginal effects are actually rather similar across the income distribution, especially after accounting for heterogeneity in extensive margin responses, which are related to the pre-reform levels. The most striking feature of Figure 2 is the sizable interior response in quintile group 2. It should also be noted that the ceiling response is quite low among middle income couples in quintile 3.

Figure 4b highlights the magnitude of the spillover effect at the bottom of the income distribution. In quintile group 1, the spillover effect is more than half of the direct effect. The composition of the extensive margin, ceiling response, and interior response is fairly similar to the direct effect in group 1.

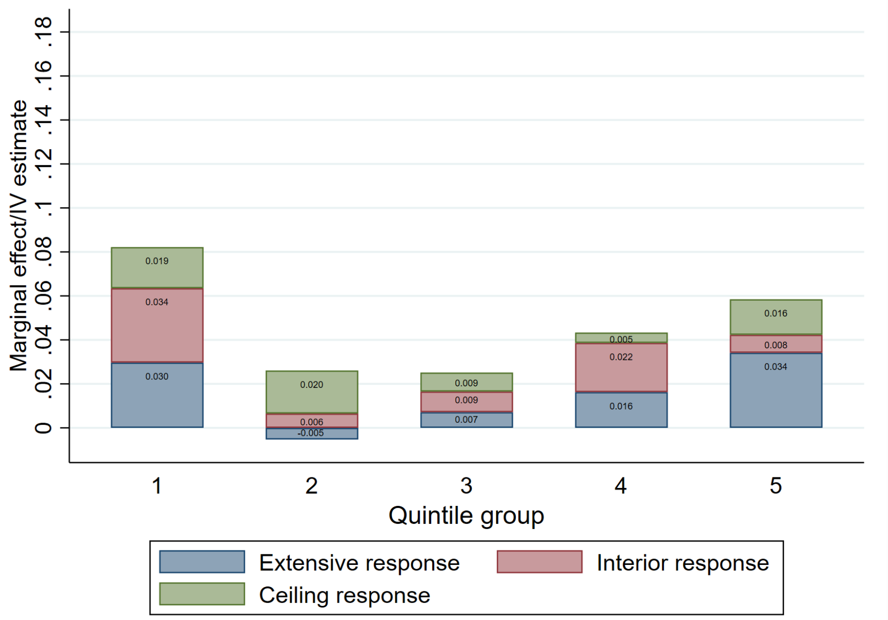
9 Concluding discussion

There is very little well-identified evidence on how take-up of social benefits reacts to the benefit level. We estimate the causal effect of the benefit level on take-up in a setting, which is close to being a randomized experiment. All Swedish parent couples are entitled to "flat rate parental leave benefits". There are no financial reasons to leave these benefits on the table because take-up does not require work absence.

In one way or the other, all results in the literature on the take-up of social



(a) Scaled direct treatment effects



(b) Scaled spillover effects

Figure 4: Scaled treatment effects The decomposition has been done using equation (1) in the main text.

benefits are program-specific, and our study is of course no exception. However, we do believe that our study provides general insights, especially when it comes to the design of child benefits.

First, we conclude that financial incentives matter for the take-up decision. This may sound trivial, but it has never been shown before in a transparent setting. Hence, when assessing social welfare and public expenditures, it is not enough to consider the mechanical effect of the benefit increase – changes to the population of beneficiaries must also be accounted for.

Second, it is less trivial that a one dollar increase in the benefit level should lead to a sizeable take-up response of low-income earners. Take-up was surprisingly low when benefits were low at the bottom half of the distribution, but the reform was leveling take-up rates. We conclude that the benefit level matters for take-up rates across the income distribution.

Third, we also estimate large and significant positive spillover effects on flat rate benefits pertaining to younger siblings, especially so at low incomes. Our interpretation is that these surprisingly large effects are driven by habits and inertia. Parents with low take-up of benefits for an older child do not seem to fully adjust their take-up when younger children are born. Governments wanting to increase take-up of low-income earners may therefore consider special financial incentives to claim benefits for first born children.

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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 towards 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

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

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

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

where D is units taken up, and b is the pre-tax value of one unit of the benefit, i.e. the policy variation of interest. Parent couples earn exogenous income z, and pay taxes $T(z + b \times D)$. 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. $T'' \geq 0$.[22]. 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 ceiling $\tilde{D}$, i.e. $D \leq \tilde{D}$.

A.2 Optimization and comparative statics

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

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

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

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

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

[22]For analytical convenience, we have simplified the Swedish tax system as being a function of the parent couple's joint income. In reality, incomes of the two parents are taxed separately. Additionally, 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

up the benefit:

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

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

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

Since the denominator of (A6) is negative, a sufficient condition for (A6) to be 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 plausible assumption in our setting, because the value of the social benefits is typically small relative to income.[23]

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

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

[23]This can be seen from the Slutsky decomposition $\frac{dD}{db} = \{\frac{\partial D(1-\tau,R)}{\partial b}\Big|_u + \frac{\partial D(1-\tau,R)}{\partial R}\}(1-\tau)$, 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, $\varepsilon^c = \frac{dD}{db}\frac{b}{D} = \frac{dD}{d(1-\tau)b}\Big|_u \frac{b}{(1-\tau)D}$ 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 are 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.

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

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 from 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 binds, we have $D = 0$ and $\lambda > 0$, and hence

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

In words, if the marginal rate of substitution, evaluated at zero take-up, is larger than the 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 $\overline{z}(b,a)$ denote the income threshold value for which a larger z will induce a parent couple to take up zero benefits. The corresponding cut-off with respect to a is denoted $\overline{a}(b,z)$

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

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

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 cut-off with respect to a which we refer to as $\underline{a}(b,z)$.

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

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

This implies $\frac{d\overline{z}(b,a)}{da} < 0$, $\frac{d\overline{a}(b,z)}{dz} < 0$, $\frac{d\underline{z}(b,a)}{da} < 0$, and $\frac{d\underline{a}(b,z)}{dz} < 0$. Hence, for a given value of a there always exist unique cut-offs with respect 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 $D = \tilde{D}$, $[F(\overline{z},\overline{a}) - F(\underline{z},\underline{a})]$ 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:

$$E(D) = \tilde{D}\int_0^{\underline{z}(b,a)}\int_0^{\underline{a}(b,z)} f(z,a)dadz + \int_{\underline{z}(b,a)}^{\overline{z}(b,a)}\int_{\underline{a}(b,z)}^{\overline{a}(b,z)} D(b,z,a)f(z,a)dadz + 0 \times \int_{\overline{z}(b,a)}^{\infty}\int_{\overline{a}(b,z)}^{\infty} f(z,a)dadz, \tag{A12}$$

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

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

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

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

Adding and subtracting $\overline{D}(b_0 + \Delta b)\{F[\overline{x}(b_0)] - F[\underline{x}(b_0)]\}$ 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 cut-off. 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 by α^E, and the coefficients from the ceiling regression by α^C, implying $E\{F[\overline{x}(b_0 + \Delta b)] - F[\overline{x}(b_0)]\} = \alpha_1^E$, 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

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

$$\overline{D}(b_0) = \frac{\alpha_0^T - \alpha_0^C \tilde{D}}{\alpha_0^E - \alpha_0^C}, \tag{A16}$$

$$F[\overline{x}(b_0)] = \alpha_0^E, \tag{A17}$$

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

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

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 4.

B Specification tests

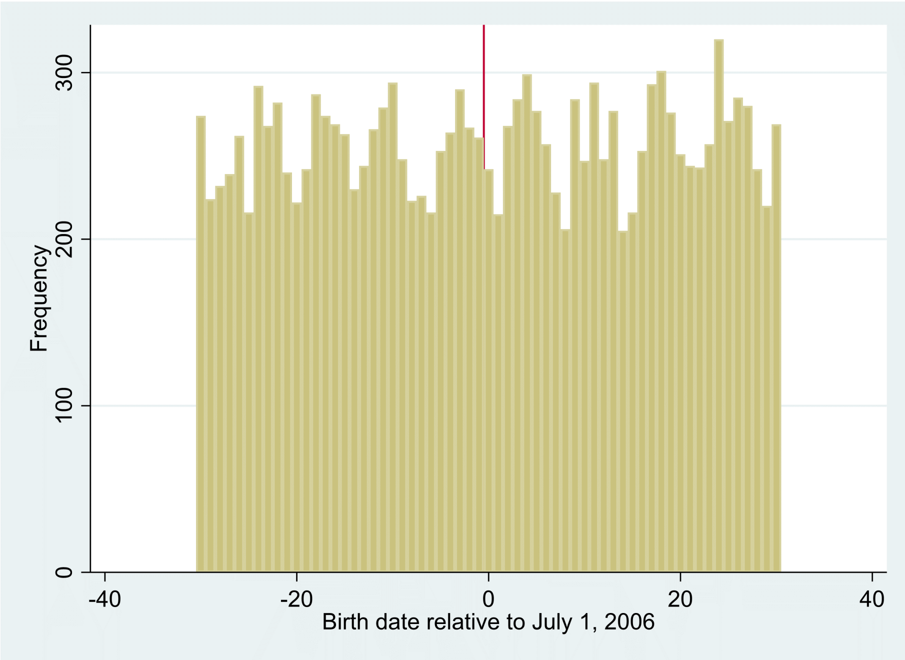
In Table B1 we report covariate balance. For the same bandwidth (+/- 90 days) as in the baseline analysis, we replace the outcome variables with key covariates, namely years of schooling, a dummy for being born in Sweden, age, and pre-reform income. We do this separately for the mother and the father.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

Table B1: Balancing of covariates

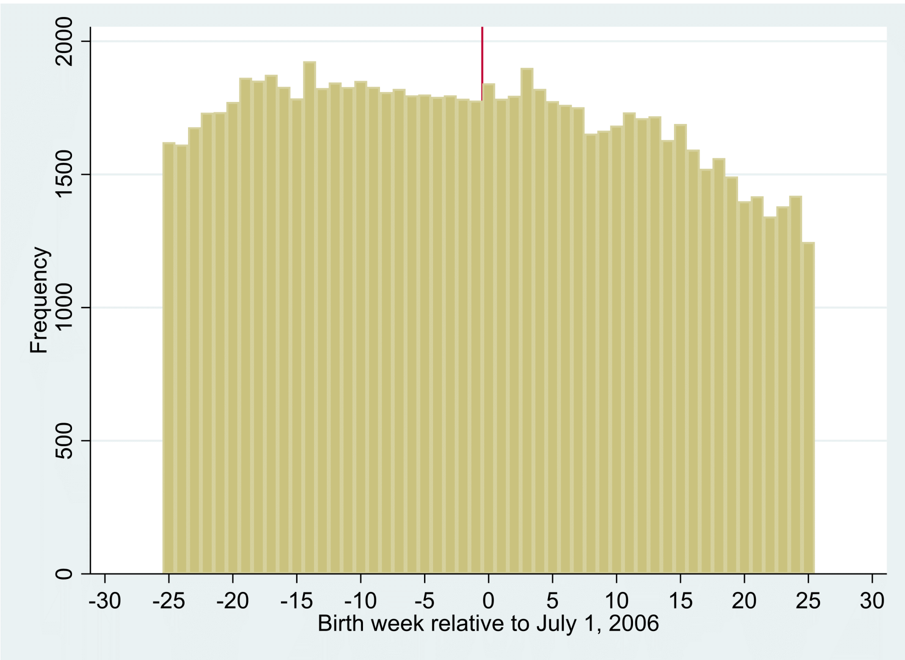
Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Schooling		Born in Sweden		Age		Income t=-1	
Parent:	Mother	Father	Mother	Father	Mother	Father	Mother	Father
RD-estimate	-0.04	-0.05	0.00	0.00	-0.08	0.00	-57	-2,010
	(0.04)	(0.04)	(0.01)	(0.01)	(0.09)	(0.11)	(2,829)	(4,215)
Observations	45432	45559	45721	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.

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 have performed two statistical tests of no manipulation on the weekly distribution. First, we performed the test suggested by Calonico et al. (2017). This test produced a p-value of 0.796 on the weekly distribution. 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-vale of 0.231 on the weekly frequency.



(a) Daily frequency



(b) Weekly frequency

Figure B1: Frequencies around the July 1, 2006, cut-off

Table C1: Robustness

Column:	(1)	(2)	(3)	(4)
Model:	Baseline	Triangular	Quadratic	Cubic
RD-estimate	11.06***	11.10***	11.18***	11.48***
	(0.55)	(0.61)	(0.84)	(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

In this appendix we assess 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 cut-off using a triangular kernel. The result is strikingly 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 cut-off. Once more, 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.

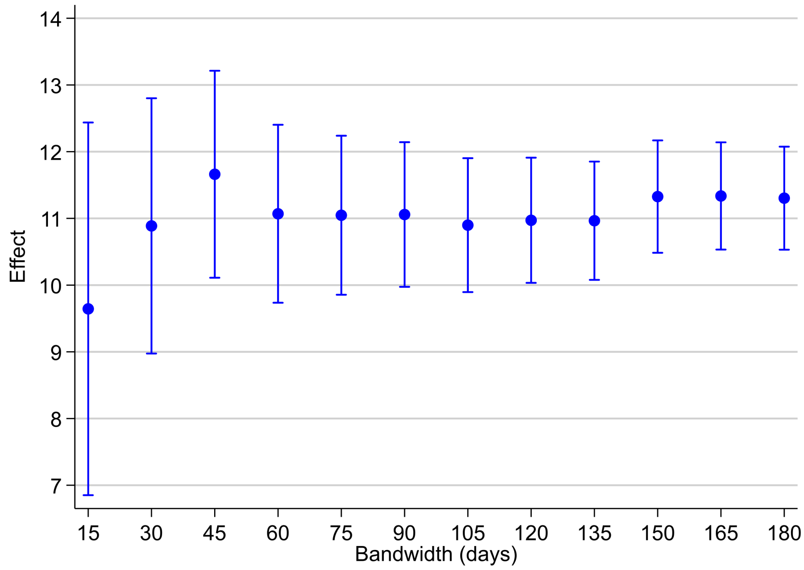
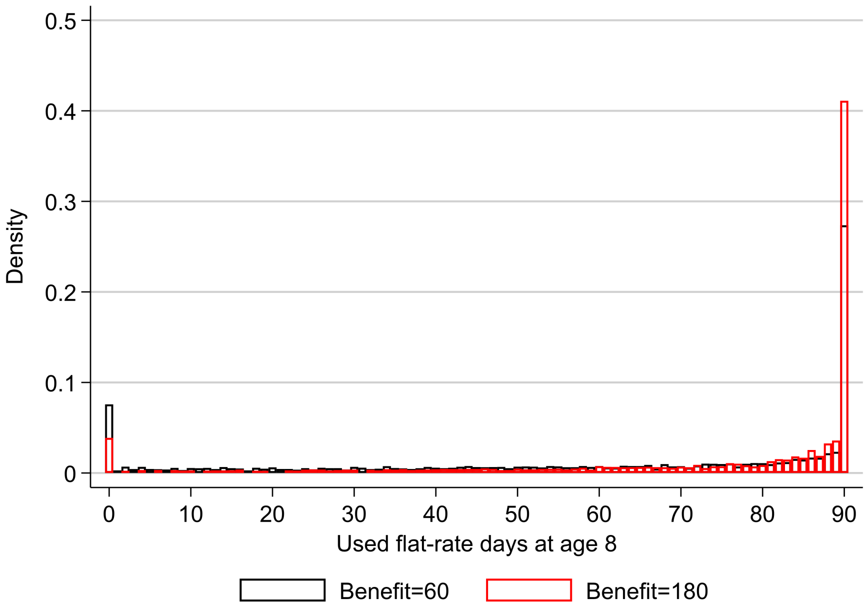


Figure C1: Treatment effect estimates for different bandwidths. 90 days is the baseline.

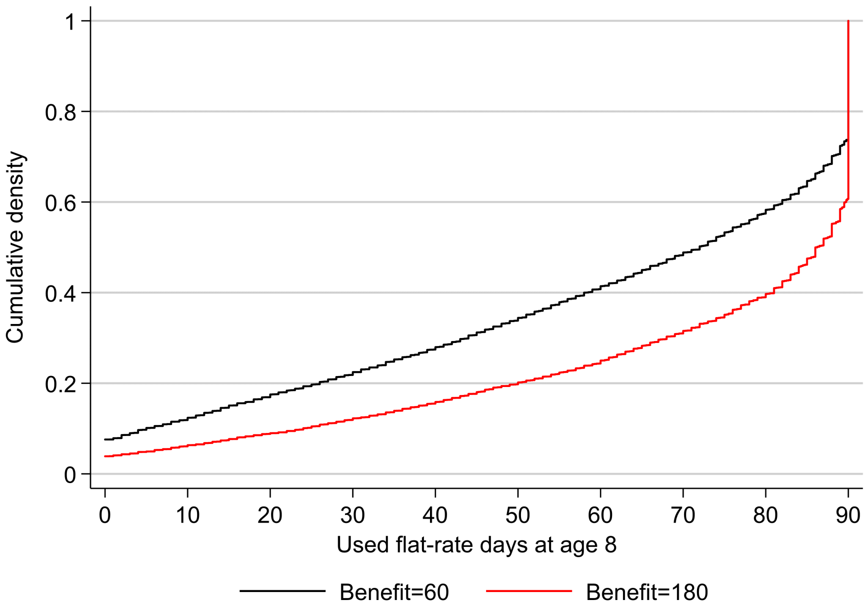
D Raw distributions

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 cut-off on July 1. Both before and after the reform a huge 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 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 also at interior values of benefit take-up. 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.



(a) Density



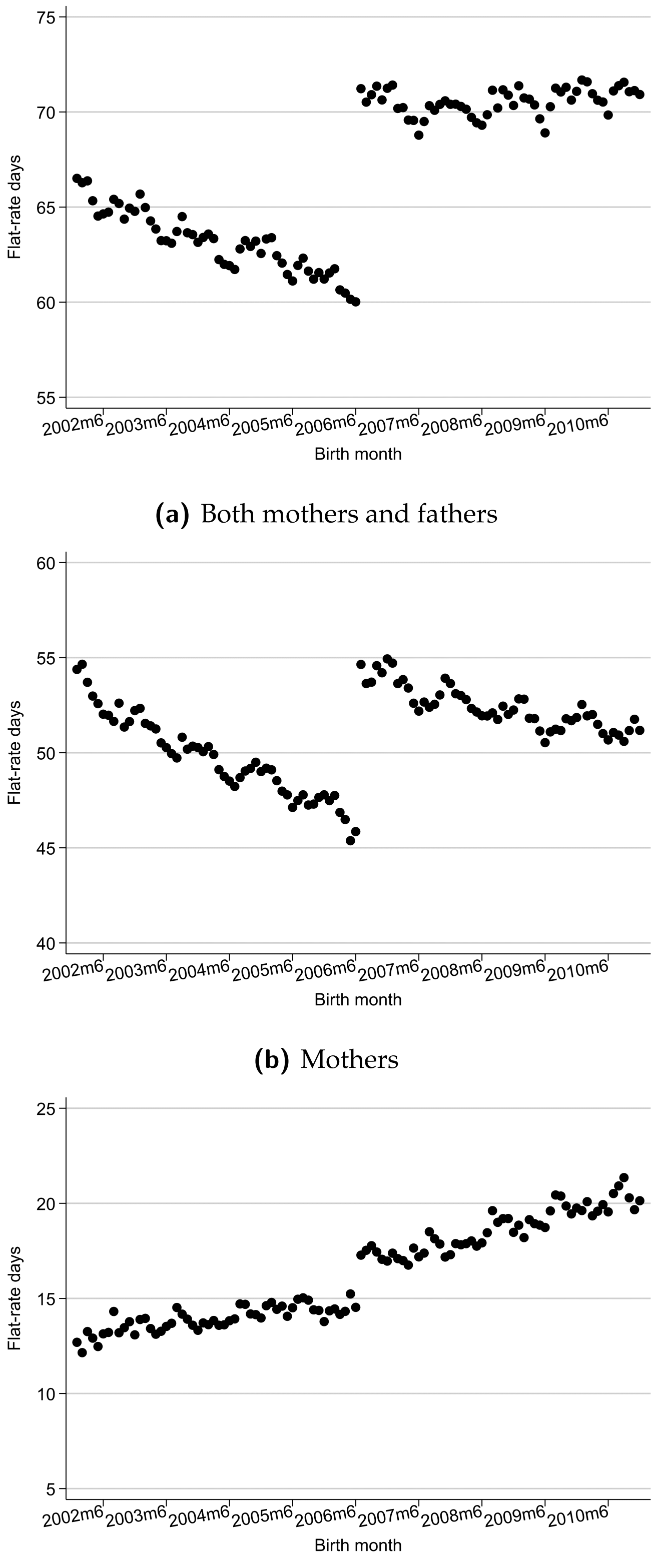
(b) Cumulative density

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

E Other years

Figure E1 shows long term trends in 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 at July 1 in other years. In Figure E2 we plot RD estimates for July 1, 2002-2010, comparing the 2006 estimate with the placebo estimates for the other years.



(a) Both mothers and fathers

(b) Mothers

(c) Fathers

Figure E1: Long term trends in take-up

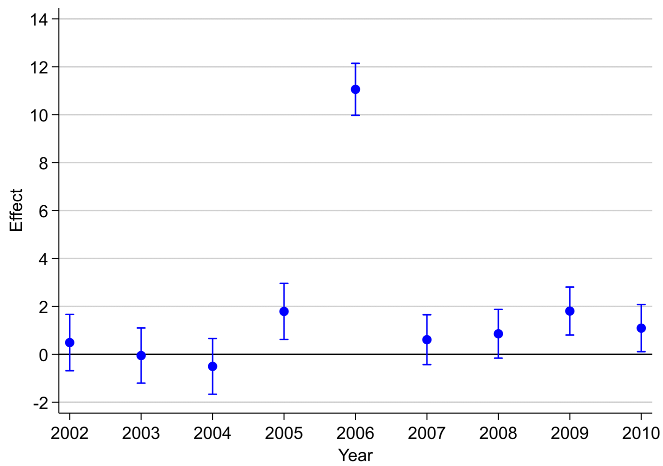


Figure E2: Estimates on July 1 cutoffs 2002-10.

F Additional results

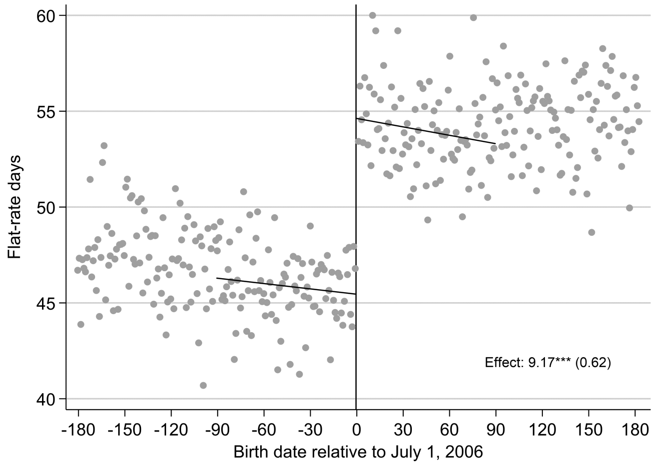
F.1 Gender division

Figure F1 shows the reduced form effects for (a) mothers, and (b) fathers. In absolute terms, the response is larger for mothers than 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 the there is only a slight increase in the mother's share, which is only borderline significant. The reason for which we take averages by day rather than by couple is that some couples take up zero benefits. In those cases, the share is undefined at the couple level.

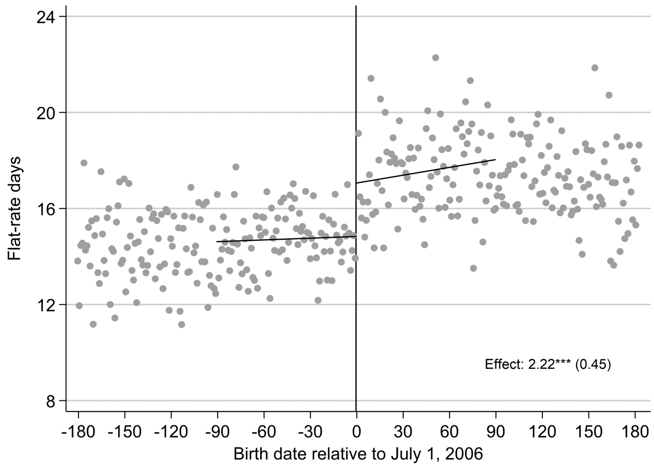
F.2 Spillover effects

Some parents who were part of the quasi-experiment of July 1, 2006, have more than one child. One possibility is that a parent couple already had children 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 if take-up of flat rate benefits applying to siblings react among treated parents. There is no significant shift in take-up at the cut-off. Note that there is less time for these parents to respond, and that the benefit is very low.

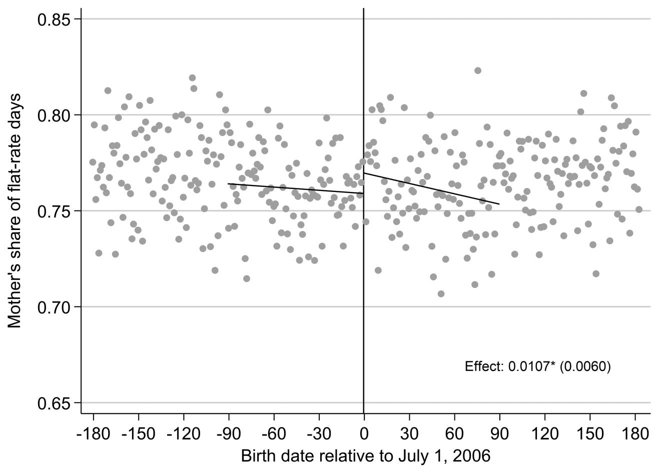
Another possibility is that parents close to the July 1, 2006, cut-off have *younger* children. This case is discussed in Section 7.4.



(a) Mothers



(b) Fathers



(c) Mother's share, by day

Figure F1: Take-up of mothers and fathers

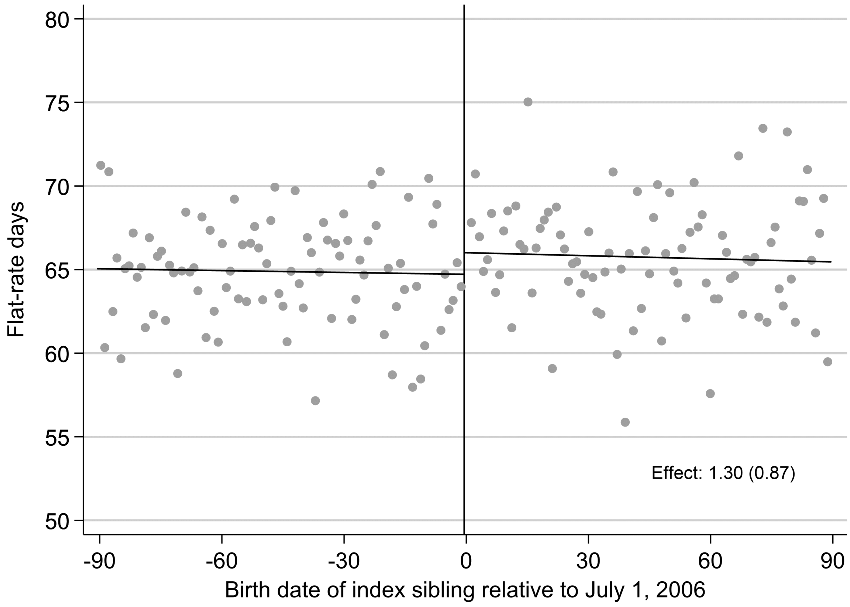


Figure F2: Spillover effects on benefits applying to older siblings (SEK 60).

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 significant effect on earnings. In Table F1 we report in detail how earnings respond to the July 1, 2006, cut-off. Note that earnings are measured by calendar year, and not by age of the child. As some individuals have zero earnings, we do not take logs. There are no significant earnings responses.

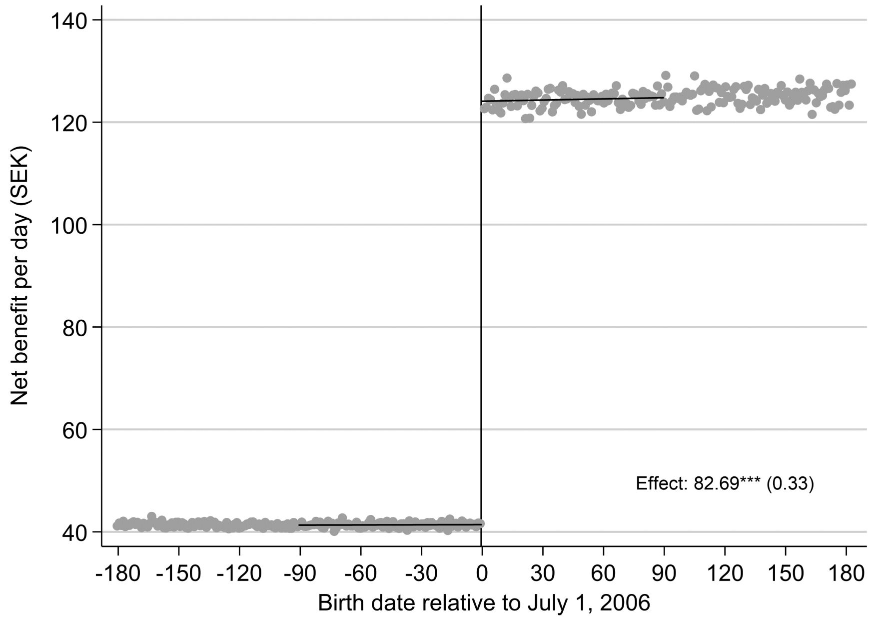
Table F1: Effects on earnings

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	-1,220	-2,486	-5,411*	-3,359	-1,263	-4,225	-2,894	-3,762	-2,369
	(23,425)	(1,979)	(2,092)	(2,865)	(3,057)	(3,196)	(3,268)	(3,400)	(3,544)	(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	-1,258	2,830	2,992	2,275	217	-210	1,791	2,849	-1,927
	(41,648)	(4,551)	(4,888)	(5,648)	(4,978)	(5,361)	(5,163)	(5,363)	(5,461)	(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	-2,478	343	-2,418	-1,084	-1,046	-4,435	-1,103	-913	-4,296
	(53,469)	(5,419)	(5,422)	(6,786)	(6,401)	(6,815)	(6,769)	(7,056)	(7,260)	(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

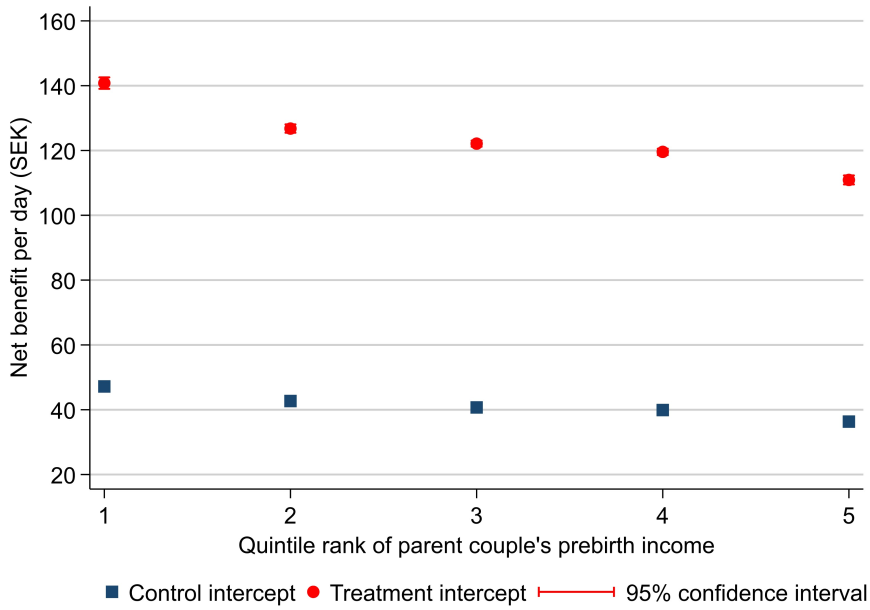
Notes: Regressions follow equation (2). Robust standard errors in paranthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The bandwidth is +/-90 days in all regressions.

G First stage graphs

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



(a) First stage: total sample



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

Figure G1: First stage graphs

H Marginal effects and elasticities

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

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***	84.07***	81.40***	79.67***	74.58***	82.69***
	(0.9022)	(0.6591)	(0.4861)	(0.5026)	(0.7081)	(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***	14.09***	8.18***	9.35***	10.02***	11.06***
	(1.30)	(1.21)	(1.14)	(1.17)	(1.33)	(0.55)
Control intercept	59.12	58.26	63.86	62.62	55.46	59.88
IV estimate	0.1451***	0.1676***	0.1005***	0.1174***	0.1343***	0.1337***
	(0.0139)	(0.0145)	(0.0140)	(0.0147)	(0.0178)	(0.0067)
Implied elasticity	0.116	0.123	0.064	0.075	0.088	0.092
C. More than 0						
Treatment effect	0.0628***	0.0413***	0.0216***	0.0151*	0.0396***	0.0363***
	(0.0114)	(0.0093)	(0.0078)	(0.0082)	(0.0114)	(0.0044)
Control intercept	0.886	0.921	0.953	0.956	0.902	0.924
IV estimate	0.00067***	0.00049***	0.00027***	0.00019*	0.00053***	0.00044***
	(0.00012)	(0.00011)	(0.00010)	(0.00010)	(0.00015)	(0.00005)
Implied elasticity	0.036	0.023	0.011	0.008	0.021	0.020
D. Full take-up						
Treatment effect	0.1585***	0.1562***	0.0984***	0.1259***	0.1066***	0.1288***
	(0.0206)	(0.0195)	(0.0195)	(0.0193)	(0.0184)	(0.0087)
Control intercept	0.338	0.248	0.275	0.245	0.207	0.263
IV estimate	0.0017***	0.0019***	0.0012***	0.0016***	0.0014***	0.0016***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(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 2. 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 reported in Figure 4. 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.