Online Appendix for: Less for more? Cuts to child benefits, family adjustments, and long-run child outcomes in larger families

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Appendix A. List of datasets

Dataset	Brief description	Source
Population data		
GBAPERSOONTAB	Basic Register of Persons (BRP), IDs and basic demographic data (2021)	`https://doi.org/10.57934/0b01e4108071ba40`
GBAHUISHOUDENSBUS	Household-level information (e.g. family structure, number of children; 1994-2019)	`https://doi.org/10.57934/0b01e410802125bb`
KINDOUDERTAB	Matching IDs for parents and children (2021)	`https://doi.org/10.57934/0b01e410801f9401`
HOOGSTEOPLTAB	Highest level of education attained (2013-2021)	`https://doi.org/10.57934/0b01e410806c961`
ONDERWIJSINSCHRTAB	Current enrolment in education (2013-2019)	`https://doi.org/10.57934/0b01e4108026cb25`
GGZDBCGELEVERDZORGPROFIELTAB	Mental healthcare use & diagnoses (2011-2014)	`https://doi.org/10.57934/0b01e41080385323`
GGZDBCTRAJECTENHOOFDDIAGTAB	Mental healthcare use & diagnoses (2011-2014)	`https://doi.org/10.57934/0b01e410805a8376`
GGZDBCTRAJECTENTAB	Mental healthcare use & diagnoses (2011-2014)	`https://doi.org/10.57934/0b01e41080385766`
SECMWERKNDGAMNDBEDRAGBUS	Earnings (1999-2019)	`https://doi.org/10.57934/0b01e41080219944`
Survey data		
Budgetonderzoek	Repeated cross-sectional survey (1988-2002) on household expenditures	`https://doi.org/10.57934/0b01e410802d4eda`
Socio-economic Panel (SEP)	Panel survey focused on wealth, income and labour market (1984-2002)	`https://doi.org/10.17026/dans-z6k-ct9r`
Multinational Time Use Study (MTUS)	Repeated cross-sectional time-diary data (1975, 1980, 1985, 1995, 2000)	`www.timeuse.org/mtus/`

Appendix B. The 1994 reform

Table 1A: Child benefit amounts and losses for hypothetical households. “Reform child” indicates the birth order of the i-th and last child, born either at the end of 1994 or in 1995 and after. Amounts are expressed in 2018 euros and over the entire eligibility period (until all children in the household reach age 18). Each birth is assumed to have occurred two years after the previous one (varying birth spacing does not substantially alter these computations).

Reform child	1994 amounts	1995 amounts	$\Delta_{(1995-1994)}$	Yearly $\Delta_{(1995-1994)}$
2nd	40,412	35,152	-5,260	-292
3rd	63,249	56,656	-6,593	-366
4th	88,239	70,273	-17,966	-998

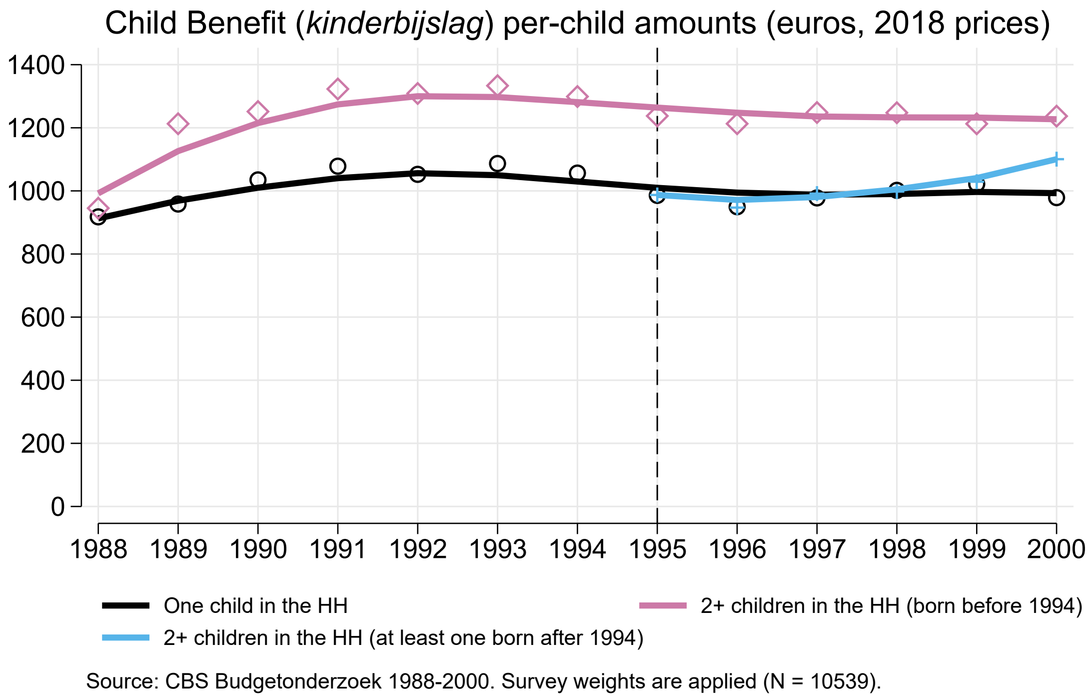

Figure 1A: Yearly self-reported per-child benefit amounts by family type. Solid lines are lowess interpolations. The sample is restricted to households with the youngest child aged 0-17 and to respondents aged 18-59.

Appendix C. Children in less well-off households

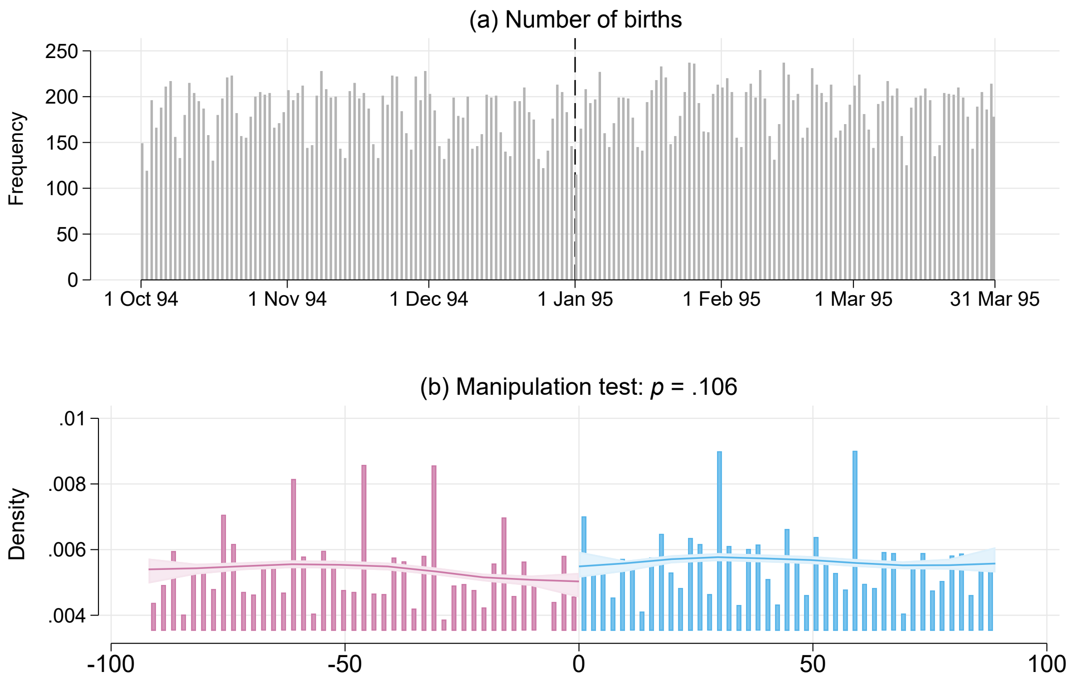

Figure 2A: Bar graph for frequency of live births for each value (day of birth) of the running variable (top) and histogram with manipulation test for bunching in the running variable (bottom). The population is restricted to parents with no postsecondary education (N = 33,615).

Table 2A: RD estimates for continuity in pre-determined covariates among households in which parents have no postsecondary education.

	Girl	Twin	Birth order	Maternal age at birth (months)	Single parent (1994)
RD Estimate	0.021	0.007	0.005	3.355	-0.003
p	0.478	0.295	0.909	0.224	0.876
N	33,615	33,615	33,615	33,615	33,615

F-test jointly predicting treatment = 1.26, p = .277. *Note*: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

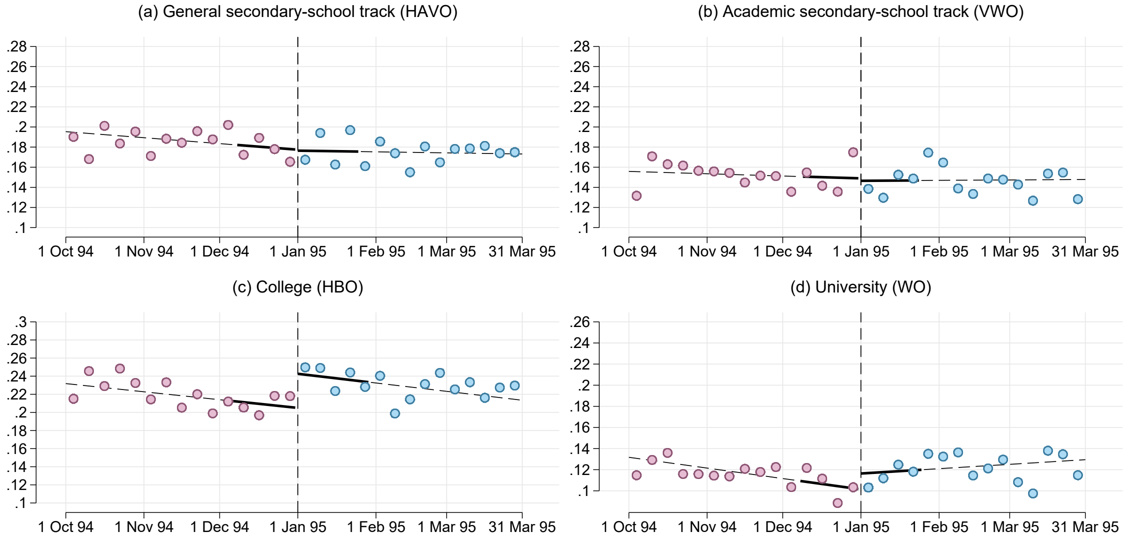

Figure 3A: RD plots for educational outcomes of children. The population is restricted to households in which parents have no postsecondary education. Outcomes are averaged within six-day birthdate bins. Solid black lines display linear predictions following (unadjusted) RD estimation within the optimal bandwidth around the cutoff of 1 January 1995 (see Section 4.2 for details). Dashed lines extend these linear predictions beyond the chosen bandwidth.

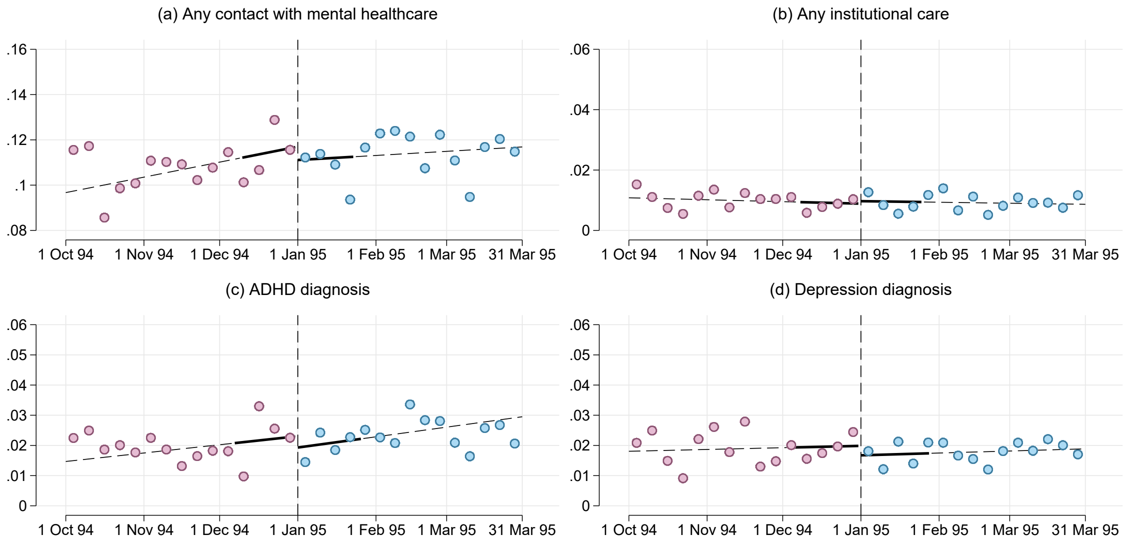

Figure 4A: RD plots for mental health outcomes of children. The population is restricted to households in which parents have no postsecondary education. Outcomes are averaged within six-day birthdate bins. Solid black lines display linear predictions following (unadjusted) RD estimation within the optimal bandwidth around the cutoff of 1 January 1995 (see Section 4.2 for details). Dashed lines extend these linear predictions beyond the chosen bandwidth.

Table 3A: RD estimates for child outcomes among less well-off households in the study population. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates. Baseline values refer to the control-group mean.

SES definition	No postsec. edu.	No postsec. edu (father)	No postsec. edu. (mother)	Bottom 4th paternal earnings	Below median paternal earnings	Below 2/3 median paternal earnings
General track (*HAVO*)						
RD Estimate	0.003	-0.001	0.006	0.015	0.019	0.019
95% CI	[-.038 , .045]	[-.04 , .039]	[-.031 , .046]	[-.053 , .074]	[-.028 , .065]	[-.038 , .074]
p	0.854	0.973	0.708	0.750	0.443	0.521
Baseline	0.18	0.19	0.19	0.17	0.17	0.17
Academic track (*VWO*)						
RD Estimate	-0.042**	-0.029*	-0.045**	-0.072**	-0.052**	-0.051**
95% CI	[-.088 , -.006]	[-.07 , .003]	[-.09 , -.009]	[-.142 , -.01]	[-.101 , -.01]	[-.112 , -.002]
p	0.026	0.074	0.016	0.024	0.017	0.041
Baseline	0.15	0.17	0.18	0.15	0.15	0.15
College (*HBO*)						
RD Estimate	0.028	0.036	0.018	0.069**	0.043*	0.089***
95% CI	[-.015 , .067]	[-.008 , .078]	[-.023 , .052]	[.006 , .15]	[-.008 , .096]	[.033 , .164]
p	0.214	0.108	0.453	0.035	0.100	0.003
Baseline	0.22	0.22	0.23	0.19	0.20	0.19
University (*WO*)						
RD Estimate	0.003	-0.004	-0.002	-0.029	-0.030	-0.031
95% CI	[-.033 , .036]	[-.037 , .029]	[-.04 , .03]	[-.085 , .027]	[-.074 , .014]	[-.084 , .018]
p	0.928	0.824	0.776	0.310	0.175	0.206
Baseline	0.12	0.13	0.14	0.12	0.11	0.12
N	33,615	37,466	39,263	11,520	23,040	14,923
p for SES continuity at the cutoff	0.21	0.20	0.36	0.65	0.95	0.74

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline.

Table 4A: RD estimates for child outcomes among less well-off households in the study population. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates. Baseline values refer to the control-group mean.

SES definition	No postsec. edu.	No postsec. edu (father)	No postsec. edu. (mother)	Bottom 4th paternal earnings	Below median paternal earnings	Below 2/3 median paternal earnings
Any contact						
RD Estimate	-0.005	-0.008	0.003	-0.019	-0.016	-0.018
95% CI	[-.038 , .035]	[-.042 , .03]	[-.027 , .04]	[-.084 , .041]	[-.057 , .022]	[-.069 , .032]
p	0.934	0.748	0.701	0.505	0.389	0.465
Baseline	0.11	0.11	0.11	0.12	0.12	0.12
Any institutional care						
RD Estimate	-0.001	-0.001	-0.002	-0.004	-0.001	-0.004
95% CI	[-.012 , .01]	[-.012 , .01]	[-.014 , .007]	[-.027 , .018]	[-.017 , .012]	[-.023 , .015]
p	0.832	0.805	0.554	0.714	0.729	0.654
Baseline	0.01	0.01	0.01	0.01	0.01	0.01
ADHD						
RD Estimate	-0.008	-0.008	-0.001	-0.015	-0.014	-0.011
95% CI	[-.022 , .008]	[-.022 , .005]	[-.015 , .015]	[-.043 , .013]	[-.035 , .004]	[-.037 , .014]
p	0.365	0.236	0.963	0.299	0.128	0.371
Baseline	0.02	0.02	0.02	0.02	0.02	0.02
Depression						
RD Estimate	-0.008	-0.007	-0.005	-0.010	-0.006	-0.006
95% CI	[-.024 , .007]	[-.022 , .008]	[-.02 , .009]	[-.037 , .016]	[-.026 , .014]	[-.027 , .016]
p	0.286	0.344	0.443	0.442	0.535	0.591
Baseline	0.02	0.02	0.02	0.02	0.02	0.02
N	33,615	37,466	39,263	11,520	23,040	14,923

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline.

Appendix D. Spillover effects on the other children in the household

I retrieve 52,740 records for the older siblings of children living in households in which no parent has postsecondary education. Around 80/90% of these records also have available information on educational outcomes as measured at around age 15 and age 26, whereas some of the oldest children cannot be tracked due to the absence of register data for the relevant calendar year.

Table 5A shows little evidence of discontinuities in secondary school track enrolment among the older siblings of children born around the policy cutoff. Differently, older siblings exhibit higher chances of finishing postsecondary education with a college degree (3 pp, $p = .071$ and $q = .119$; 14% of baseline) as opposed to a university degree (–4 pp, $p = .015$ and $q = .066$; 29%), when exposed to the reform. Estimates for mental healthcare use and mental ill-health diagnoses are less conclusive and comparable to those for children in the study population.

Similar to reform effects on children born around the cutoff, evidence of spillover effects is limited to educational outcomes. Further, evidence for older siblings is more clear-cut when it comes to tertiary compared to secondary-school outcomes, the opposite pattern to the one found for higher-order children. The reform increased the economic costs of raising children which are front-loaded in early childhood, a period in which investments are particularly conducive to skill development (e.g. Heckman & Mosso, 2014; Duncan et al., 2017; van Huizen & Plantenga, 2018; Carneiro et al., 2021). Older siblings enjoyed pre-reform levels of income support for part or the entirety of their early years. Parents may have thus kept up with child-related investments more easily than for subsequent children. Yet, the cumulative nature of the income shock brought about by the 1994 reform might have resulted in lower investments in another sensitive period for monetary investments, i.e. at the end of secondary school (see, e.g., Carneiro et al., 2021). In line with previous studies on the intergenerational effects of income shocks (Bastian & Michelmore, 2018), the 1994 reform might have worsened credit constraints for households facing higher-education choices, including those of their older children.

Table 5A: RD estimates for child outcomes among the older siblings of children in the study population. The population is restricted to households in which parents have no postsecondary education. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates. Baseline values refer to the control-group mean.

Education	General track (*HAVO*)	Academic track (*VWO*)	College (*HBO*)	University (*WO*)
RD estimate	-0.015	-0.008	0.033*	-0.038**
95% CI	[-.058 , .029]	[-.043 , .027]	[-.003 , .077]	[-.079 , -.008]
p	0.513	0.647	0.071	0.015
Baseline	0.19	0.14	0.22	0.13
N	36,949	36,949	43,155	43,155
Mental health	Any contact	Any institutional care	ADHD	Depression
RD estimate	-0.004	-0.000	-0.005	-0.003
95% CI	[-.026 , .023]	[-.009 , .008]	[-.017 , .006]	[-.015 , .01]
p	0.890	0.893	0.331	0.685
Baseline	0.11	0.01	0.01	0.02
N	52,740	52,740	52,740	52,740

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth.

Appendix E. Family adjustments

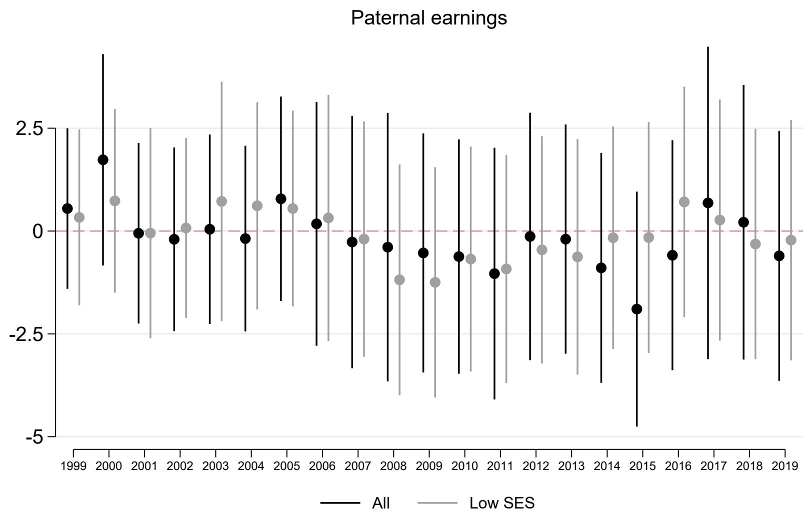

Figure 5A: RD estimates for paternal earnings in the study population.

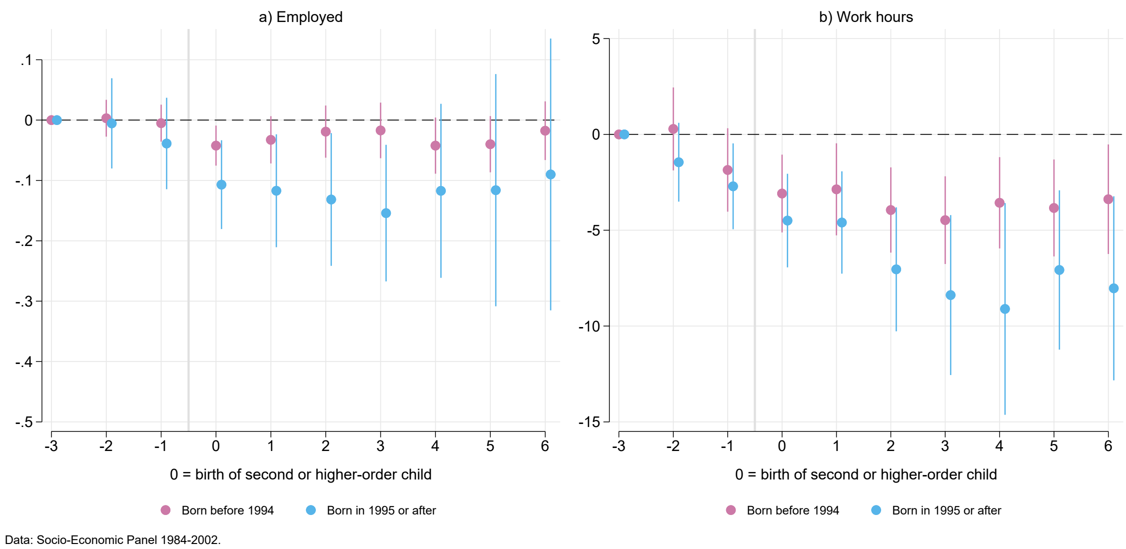

Figure 6A: Event studies for women's labour supply around the birth of a second or higher-order child. Households with lower SES (no postsecondary education). Socio-Economic Panel (SEP) 1984-2002 (for employment, $N_{-1994} = 26{,}482$, $N_{1995-} = 21{,}912$; for work hours, $N_{-1994} = 9{,}613$, $N_{1995-} = 8{,}952$). Standard errors are clustered at the individual level and survey weights are applied.

Table 6A: RD estimates for fertility responses. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models adjust for pre-determined covariates. Baseline values refer to the control-group mean.

Fertility	No. children	
	Whole	Low-SES
RD Estimate	-0.015	-0.005
95% CI	[-.125 , .091]	[-.141 , .128]
p	0.760	0.924
Baseline	3.02	3.08
N	46,080	33,615

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline, parental education.

Table 7A: RD estimates for household stress. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models adjust for pre-determined covariates. Baseline values refer to the control-group mean.

Household stress	Maternal depression		Ever single parent		Parent-child relational problems	
	Whole	Lower SES	Whole	Lower SES	Whole	Lower SES
RD Estimate	0.0001	-0.010	0.040**	0.020	0.004*	0.006**
95% CI	[-.017 , .02]	[-.033 , .012]	[.001 , .083]	[-.019 , .07]	[-.0002 , .009]	[.0004 , .012]
p	0.864	0.358	0.044	0.263	0.060	.034
Baseline	0.04	0.04	0.30	0.31	.005	.005
N	46,080	33,615	46,080	33,615	46,080	33,615

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline, parental education.

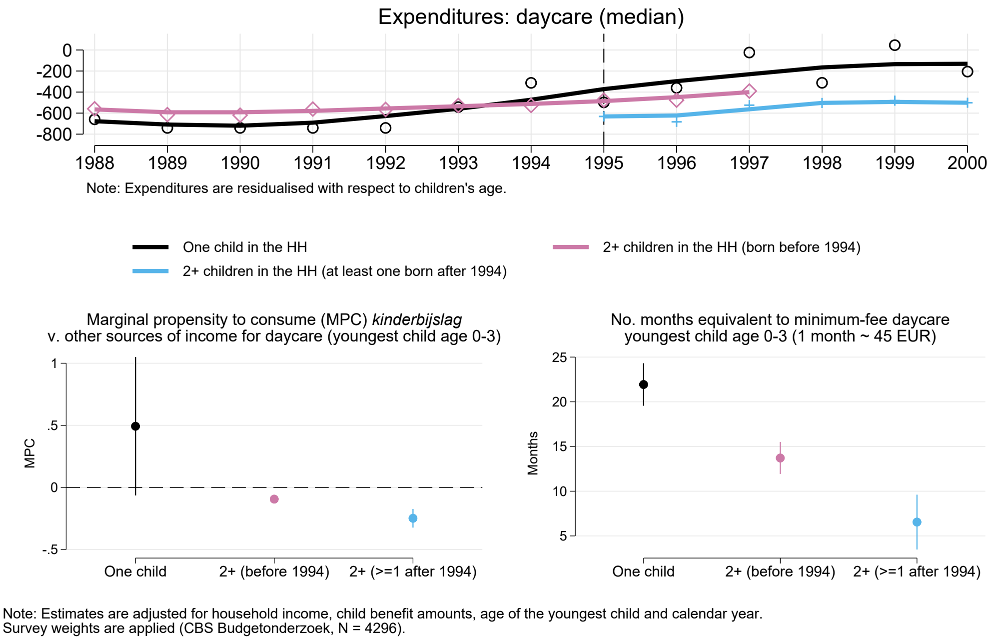

Figure 7A: Analyses of monetary investments in day-care (youngest child in the household aged 0-3, Budget Survey 1988-2002). Expenditures in the top panel are residualised with respect to the age of the first child. Solid lines are lowess interpolations. Marginal propensities to consume (MPCs, bottom-left panel) are obtained by taking the difference of the coefficient for child-benefit income and that of household income by sibship size (one child, two children or more and all born in 1994 or before, two children or more and all born after 1994).

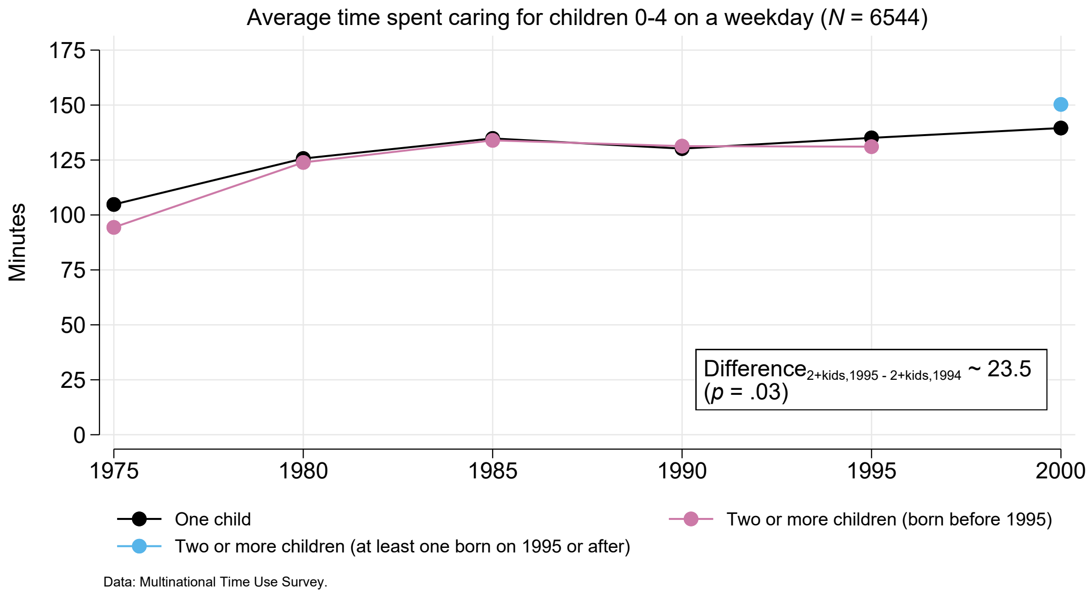

Figure 8A: Average time spent doing childcare among mothers with no postsecondary education by sibship size and year of birth of the youngest child (pooled MTUS data 1975-2000). The sample comprises respondents aged 18-45 whose youngest child is aged 0-4 and in which no household member has postsecondary education. The estimated difference is derived from a linear regression with day, month, and age of respondent fixed effects. Time-diary data is recorded during multiple days by each respondent and standard errors are thus clustered at the respondent level.

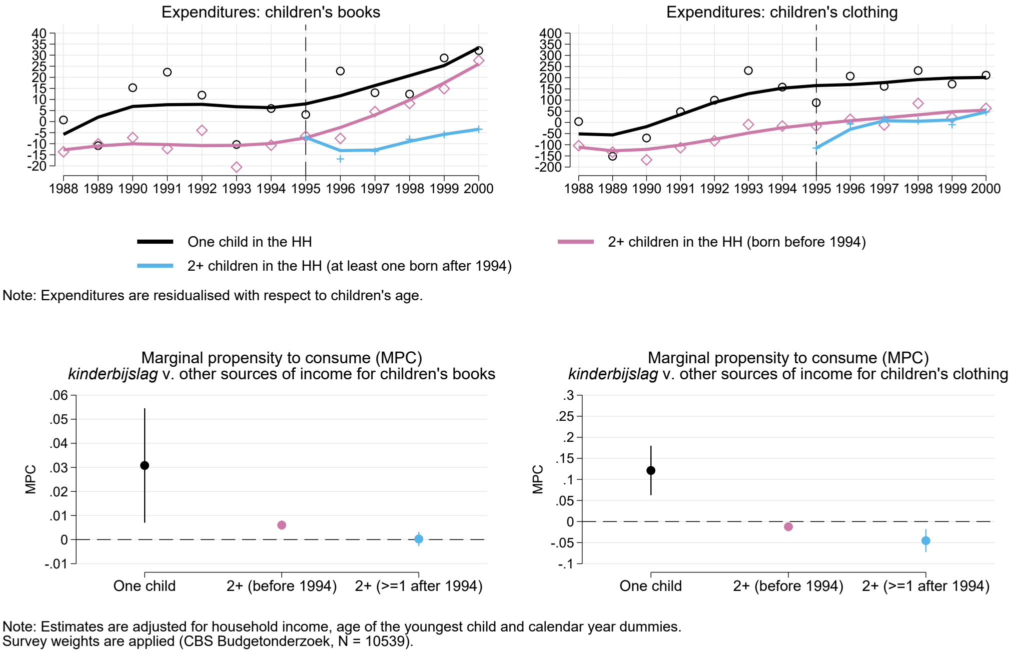

Figure 9A: Analyses of monetary investments in children's books (left) and children's clothing (right; youngest child in the household aged 0-17, respondents aged 20-59; Budget Survey 1988-2002). Expenditures in the top panels are residualised with respect to the age of the first child. Solid lines are lowess interpolations. Marginal propensities to consume (MPCs, bottom panels) are obtained by taking the difference of the coefficient for child-benefit income and that of household income by sibship size (one child, two children or more and all born in 1994 or before, two children or more and all born after 1994).

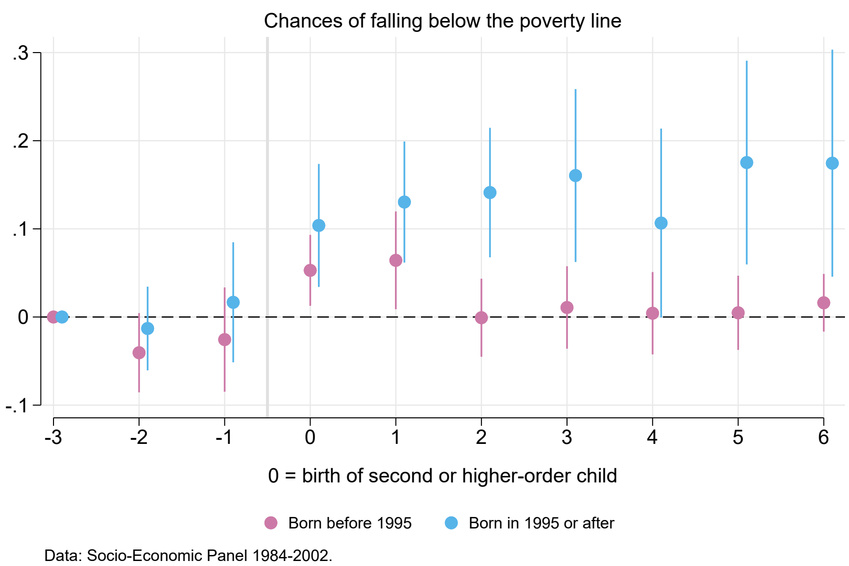

Figure 10A: Event studies for household exposure to poverty risks around the birth of a second or higher-order child. Households with lower SES (no postsecondary education). Socio-Economic Panel (SEP) 1984-2002 (N_{-1994} = 16,001, N_{1995-} = 13,791). Standard errors are clustered at the individual level and survey weights are applied.

Appendix F. Are effects on educational outcomes long-lasting?

Table 8A: Transitions (%) in the Dutch educational system by children in the study population. The population is restricted to households in which parents have no postsecondary education (Dutch microdata, N = 33,615).

	VMBO to *HAVO*	*HAVO* to *VWO*	*HBO* to *WO*
Born Oct-Dec 1994	3.7	0.3	2.3
Born Jan-Mar 1995	4	0.6	2.7

Table 9A: RD estimates for child outcomes. The population is restricted to households with no post-secondary education. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates. Baseline values refer to the control-group mean.

	VMBO to *HAVO*	*HAVO* to *VWO*	*HBO* to *WO*
RD Estimate	0.012	0.003	0.008
95% CI	[-.012 ; .033]	[-.003 ; .01]	[-.011 ; .024]
p	0.373	0.293	0.452
Baseline	0.037	0.003	0.023
N	33,615	33,615	33,615

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline.

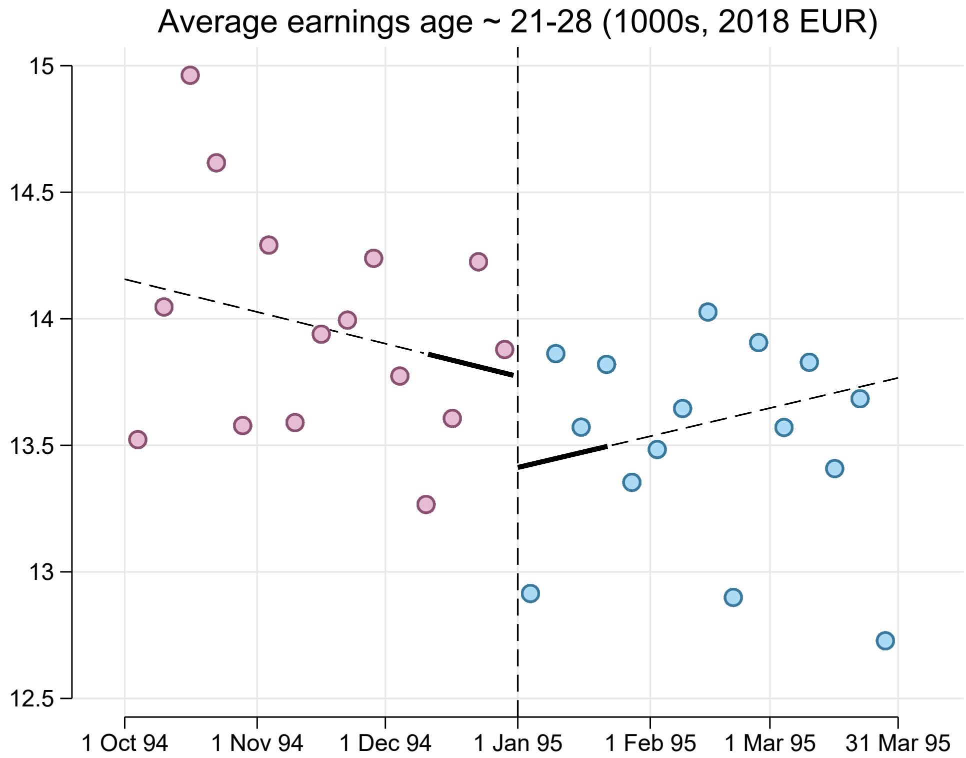

Figure 11A: RD plots for average earnings upon labour market entry for children in the bottom fourth of the paternal earnings distribution. Outcomes are averaged within six-day birthdate bins. Solid black lines display linear predictions following (unadjusted) RD estimation within the optimal bandwidth around the cutoff of 1 January 1995 (see Section 4.2 for details). Dashed lines extend these linear predictions beyond the chosen bandwidth.

Table 10A: RD estimates for children's earnings among households with lower SES in the study population. Earnings are trimmed at the 99th percentile. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates. Baseline values refer to the control-group mean.

SES definition	No postsec. edu.	No postsec. edu (father)	No postsec. edu. (mother)	Bottom 4th paternal earnings	Below median paternal earnings	Below 2/3 median paternal earnings
Average earnings age 21-28 (1000s, 2018 EUR)						
RD Estimate	-0.529	-0.487	-0.614	-1.615**	-0.424	-1.076*
95% CI	[-1.656 , .432]	[-1.581 , .415]	[-1.675 , .26]	[-3.723 , -.098]	[-1.605 , .527]	[-2.699 , .109]
p	0.251	0.252	0.152	0.039	0.321	0.071
Baseline	16.14	16.01	16.06	13.97	14.93	14.24
Rank of average earnings age 21-28						
RD Estimate	-1.644	-1.520	-1.928	-5.435**	-1.416	-3.652*
95% CI	[-5.117 , 1.438]	[-4.934 , 1.397]	[-5.241 , .866]	[-12.199 , -.521]	[-5.319 , 1.675]	[-8.911 , .159]
p	0.271	0.273	0.160	0.033	0.307	0.059
Baseline	52.15	51.68	51.88	45.06	48.22	45.96
N	33,233	37,062	38,844	11,431	22,856	14,817

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, and single parent at baseline.

Appendix G. Sensitivity to estimation choices

Appendix G.1. Excluding covariates

Table 11A: RD estimates for child outcomes in the study population. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models exclude pre-determined covariates.

Education	General track (*HAVO*)	Academic track (*VWO*)	College (*HBO*)	University (*WO*)
RD Estimate	0.003	-0.027*	0.029	-0.007
95% CI	[-.033 , .039]	[-.064 , .003]	[-.013 , .066]	[-.041 , .024]
p	0.859	0.070	0.188	0.625
RD Estimate	0.001	-0.003	-0.006	-0.004
95% CI	[-.03 , .036]	[-.014 , .006]	[-.019 , .006]	[-.018 , .01]
p	0.853	0.424	0.303	0.567
N	46,080	46,080	46,080	46,080

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 12A: RD estimates for child outcomes among households with lower SES in the study population. Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models exclude pre-determined covariates.

Education	General track (*HAVO*)	Academic track (*VWO*)	College (*HBO*)	University (*WO*)
RD Estimate	0.006	-0.038**	0.032	0.008
95% CI	[-.036 , .048]	[-.084 , -.001]	[-.013 , .074]	[-.029 , .04]
p	0.777	0.042	0.168	0.753
RD Estimate	-0.004	-0.001	-0.009	-0.008
95% CI	[-.038 , .036]	[-.012 , .01]	[-.023 , .007]	[-.023 , .007]
p	0.960	0.861	0.307	0.303
N	33,615	33,615	33,615	33,615

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Appendix G.2. Kernel, polynomial fit, and bandwidth

Table 13A: RD estimates for the chances of being enrolled in *VWO* among children in the study population. The population is restricted to households in which parents have no postsecondary education. Estimates in the preferred specification (Main) are compared to those obtained using a different polynomial (quadratic), different kernels (uniform, Epanechnikov), an asymmetric bandwidth, and a CER-optimal bandwidth (Cattaneo et al., 2019). Point estimates with robust bias-corrected 95% confidence intervals and p-values. Models are adjusted for pre-determined covariates.

Estimation	Main	Quadratic	Uniform	Epanechnikov	Asymmetric	CER
RD Estimate	-0.042**	-0.046**	-0.046**	-0.043**	-0.046*	-0.044*
95% CI	[-.088 , -.006]	[-.092 , -.002]	[-.091 , -.011]	[-.088 , -.007]	[-.099 , .007]	[-.094 ; .003]
p	0.026	0.043	0.013	0.022	0.090	0.063
N	33,615	33,615	33,615	33,615	33,615	33,615

Note: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. All specifications are adjusted for sex of the child, twin birth, birth order, maternal age at birth, single parent at baseline.

Appendix G.3. "Donut-hole" approach

I have excluded up to eight days on each side of the cutoff. Research has found evidence of timing effects in the week surrounding the policy cutoff when the policy incentive is to bring births forward (e.g. Borra et al., 2019). In addition, the eight-day donut size is in continuity with relevant previous studies (Barr et al., 2022). To make sure that statistical power is unaffected when using the donut-hole approach, I rely on the same bandwidth throughout (*ibidem*), fixing it at the value used in the main analyses for each outcome.

I focus on children's educational outcomes in less well-off households to check the sensitivity of the main results of the paper. Examining children's chances of attending the academic track (Figure 12A, left), point estimates are largely compatible across donut-hole specifications, with most estimates ranging between 4 and 6 percentage points. Excluding one to two days, estimates are about the same or even larger than the main estimate and statistically significant. After that, interval estimates are largely compatible but somewhat noisier, and point estimates are more unstable. This is especially true when excluding three to seven days and, hence, giving more weight to births on and around Christmas in the estimation. Similar conclusions can be drawn when examining college attainment in the main population (Figure 12A) and college and university attainment in the sibling population (Figure 13A).

A similar pattern has also been found in previous studies. Births around Christmas are typically more selective. Donut-hole approaches giving more weight to those individuals may thus be inconsistent with the estimates around policy cutoff (see, e.g., Barr et al., 2022: 2557). Further, policy anticipation around Christmas is unlikely. Births can be scheduled early either via C-sections or induced labour. These procedures are less likely to occur during holidays and weekends as hospitals are relatively understaffed (for the NL, see Elferink-Stinkens et al., 2001). In 1994, Christmas days fell during a weekend, making policy anticipation during those days highly unlikely. I also do not find a peak in births just after Christmas (Figure 2), suggesting no evidence of "strategically timed" births delayed from the Christmas weekend to the following days.

Hence, while I have reported estimates with donut holes of different sizes for completeness, more stock should arguably be put in the estimates closest to the cutoff.

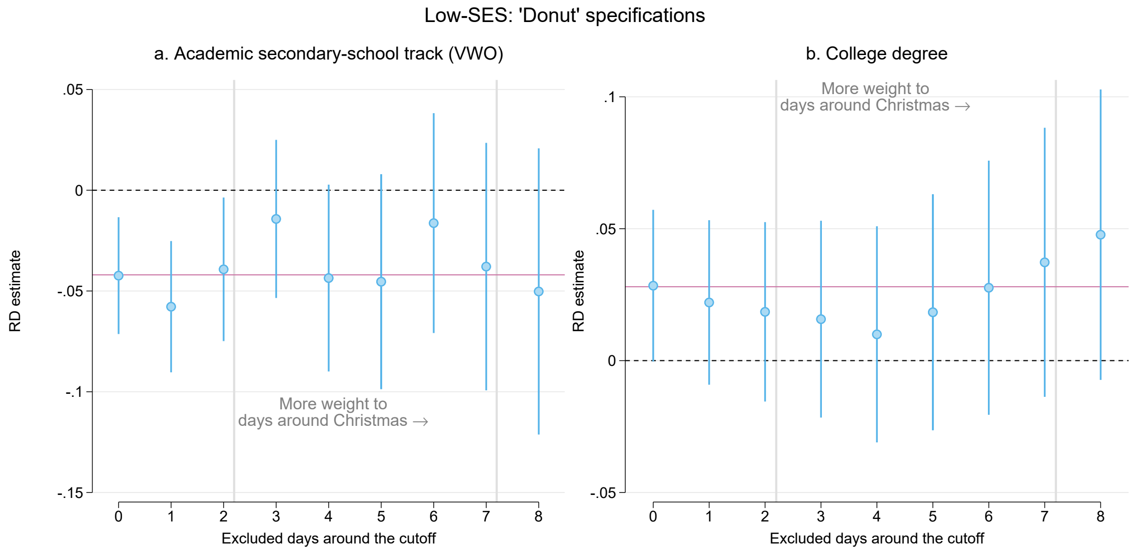

Figure 12A: Sensitivity of selected RD estimates. Donut-hole (x-axis) specifications (Cattaneo et al., 2019).

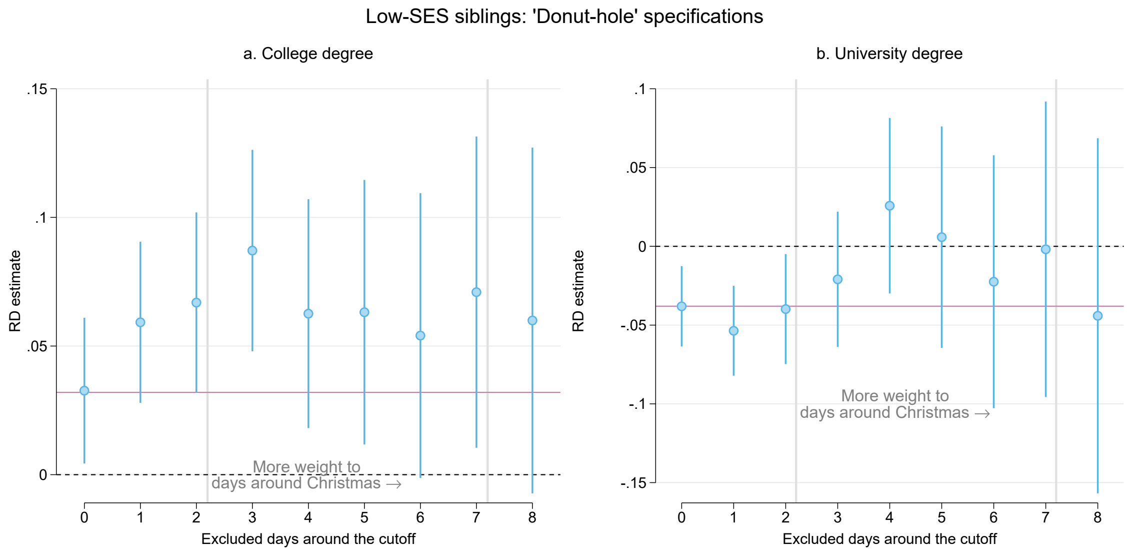

Figure 13A: Sensitivity of selected RD estimates. Donut-hole (x-axis) specifications (Cattaneo et al., 2019).