The Later the Better? A Novel Approach to Estimating the Effect of School Starting Age on ADHD and Academic Skills

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Appendices

A Age and Gender Differences in ADHD Diagnosis

The Norwegian healthcare system is publicly financed and covers all citizens and legal residents. For children and adolescents, both inpatient and outpatient healthcare along with medications are provided freely. Diagnosis of ADHD is usually provided from a specialist healthcare provider, with regular follow-up from the general practitioner (GP). In Norway, between 2008 and 2013, 4.3% of boys and 1.7% of girls between the ages 6 and 17 were registered with at least one ADHD diagnosis (Ørstavik et al., 2016). In the same time period, 80% of children with an ADHD diagnosis had received at least one prescription for stimulants, with the proportion of boys using a prescription stimulant around three times higher than girls (Ørstavik et al., 2016). Figure A1 shows the percentage of children/adolescents that were registered with a diagnosis of ADHD from 2008-2019, based on their age in 2019. Before school start at ages 5-6, very few children are registered with an ADHD diagnosis. This share greatly increased around the ages 7 to 11, where it then remains relatively stable with some increases in the later teenage years among girls.

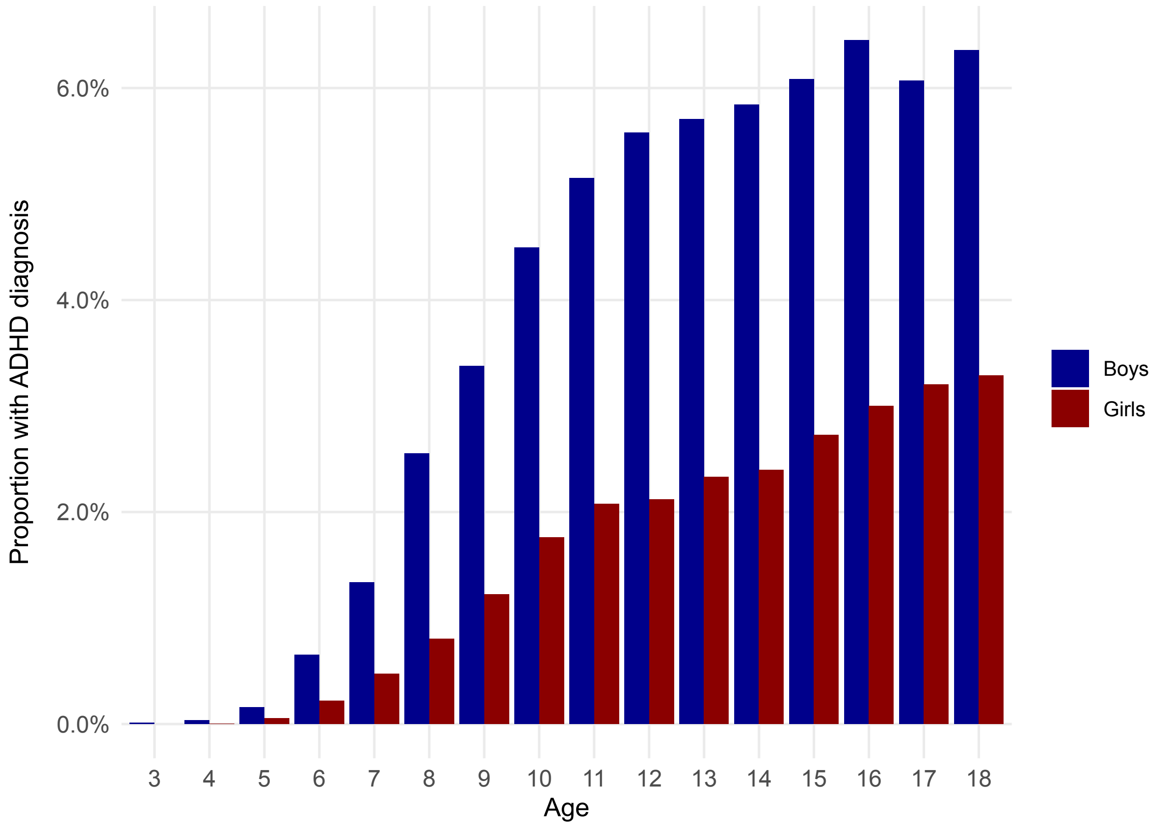

Figure A1: Age and gender differences in ADHD diagnosis. *Note.* Percentage of children/adolescents registered with an ADHD diagnosis in the period 2008-2019, based on age in 2019. Split by gender. Data from the National Patient Register (NPR).

B Data sources and Variables

School starting age (SSA) was obtained using information on the year in which the individual could sit for the 5$^{\text{th}}$ grade national test together with their month of birth. The year of the 5$^{\text{th}}$ grade national test was used to obtain the individual's SSA by subtracting the month/year of 5$^{\text{th}}$ grade enrollment (August) from their month/year of birth, giving their age at beginning 5$^{\text{th}}$ grade in months, and then subtracting 4 years. The year of the 5$^{\text{th}}$ grade test also includes information for students who were eligible to sit for the test but that were absent or exempted from taking the test. As automatic grade promotion, i.e. students are promoted to the next grade regardless of performance, is mandated in compulsory education, all individuals are exposed to 4 years of schooling before enrolling in 5$^{\text{th}}$ grade (Borodankova & Coutinho, 2011). Therefore, it is plausible that this SSA variable is a reliable estimate of the true SSA for these birth cohorts.

Diagnoses for ADHD are obtained from both the National Health Insurance Scheme (KUHR) and National Patient Register which covers both visits to general practitioners and specialist physicians. ADHD diagnoses were measured at ages 7 to 15, due to data restrictions. Patients are required to receive a referral from general practitioners before receiving specialist healthcare, and general practitioners are often responsible for the follow-up care of those diagnosed with ADHD by specialist physicians. After 2014, general practitioners were allowed to maintain stimulant treatment for ADHD, although specialists are required for the initiation of stimulant treatment (Karlstad et al., 2017). Information on diagnoses from the primary care system was available from 2006-2018, and diagnoses from specialist healthcare were available from 2008-2019.

Data for academic skills comes from the National Education Database which provides information on the individuals' national test scores. National tests are mandatory for students in grades 5, 8, and 9, and measure students' basic skills in English, Math, and Reading and whether these are in accordance with the national teaching objectives. The scores from these tests are used for quality development and assessment of the school system and do not have an impact on the student's GPA or school grades. For the analyses on academic skills the students' national test scores in math and reading in 5th and 9th grade are used.

C The Due Date Instrument

Much of the previous literature on SSA has relied on either month of birth or exact date of birth with the assumption that birth date is ”randomly assigned” and thus allows for the estimation of causal effects (Angrist & Krueger, 1991, 1992; Balestra et al., 2020; Elder, 2010). However, if parents strategically plan the timing of their child's birth to avoid December birth dates, the resulting estimates may be biased as the children born in December would be negatively selected on certain parental characteristics. As shown, this assumption is not always met, where births close to the cutoff can be systematically manipulated and vary according to certain characteristics. I present an alternative approach using due date as determined by ultrasound examination, which is not affected by such shifting of births across the cutoff.

For due date to be a valid instrument for SSA, it must meet four criteria: First, it must cause variation in SSA, known as the relevance assumption. Second, it must impact the outcome only through SSA, known as the exclusion restriction. The first criterion is testable, and as shown in this paper, due date meets this requirement. The second criterion is not testable, but rather must be assumed based on prior knowledge and understanding. It is plausible, however, to assume that one's due date would not have a direct impact on their ADHD diagnosis risk or national test scores, but must act through school entry age. For example, there are no substantive differences in the timing of enrollment in kindergarten between those born in December and January, as the entry cutoff for kindergarten was set at August 31st and gradually extended to November 31st for the recent cohorts in Norway. Thus, it is unlikely that any effect of due date on ADHD risk or academic skills would be through kindergarten entry, as opposed to school entry age. Similarly, the independence assumption states that the instrument does not share any common confounders with the outcomes. Finally, in order to estimate the local average treatment effect (LATE), the instrument must meet the monotonicity assumption. This assumption states that an increase in the instrument (i.e. a later due date) will always affect the endogenous variable in the same direction (the

SSA is the same or later), and that the instrument will never lead to a younger SSA for any subgroup.

On the left panel of Figure C2, the density of birth dates within ±45 days around January 1st is shown. There are fewer births approaching the cutoff, with a large drop at around 7 days before and again on the last day before January 1st. After the cutoff, there is a higher frequency of births, which are particularly bunched within the first week of January. On the right side, the density of due dates are plotted for the same window around the cutoff. In this graph, there is a much smoother distribution across the cutoff, with no apparent bunching on either side of January 1st, until after 15 days into January where the density of due dates increases. This provides support to the assumption that due date is not systematically manipulated by parents across the cutoff, whereas this is not the case for birth dates.

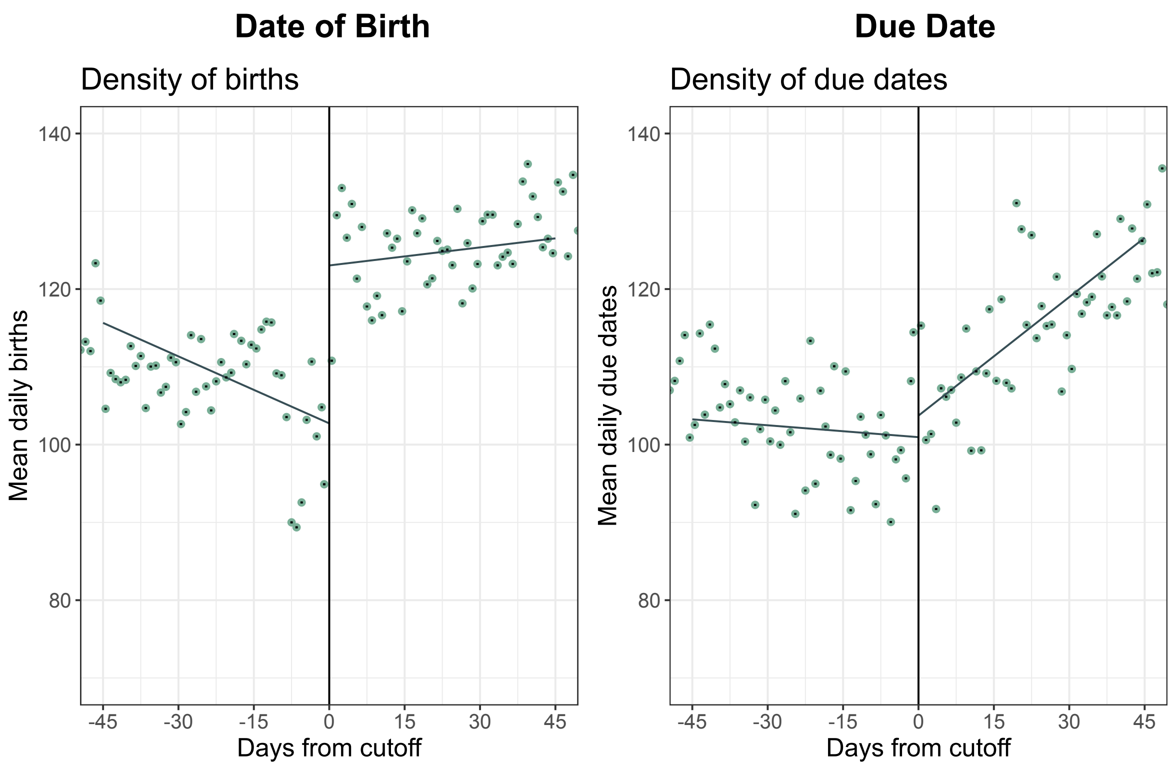

Figure C2: Density of Date of Birth and Due Date around Cutoff. *Note.* Date of Birth: Sample from 1999-2007 birth cohorts (N=407,789). Date of birth recentered around January 1st cutoff. Due Date: Sample from 1999-2007 birth cohorts (N=392,311). Due date recentered around January 1st cutoff.

To examine the assumption that those with a due date just before and after the January 1st cutoff date are not systematically different, pre-determined parental characteristics are plotted for each day within a ±15 day window around the cutoff. The predetermined characteristics for date of birth are also plotted across the cutoff using MSE-optimal bandwidth. Figure C3 presents the pre-determined characteristics including mother's and father's age at birth, the proportion whose mother completed upper secondary education and proportion whose father completed upper secondary education by date of birth on the top and due date on the bottom. For date of birth, there do not appear to be large differences across the cutoff in terms of parental age at birth, however, those born after the cutoff appear to have a slightly higher proportion with higher paternal education compared to those born before. For due date there do not appear to be any systematic differences across the January 1st threshold in terms of parental age at term or education. This is as expected, since there are no apparent benefits or feasible possibility for parents to manipulate the due date beyond strategic conception, whereas there is a potential for parents to shift the date of birth.

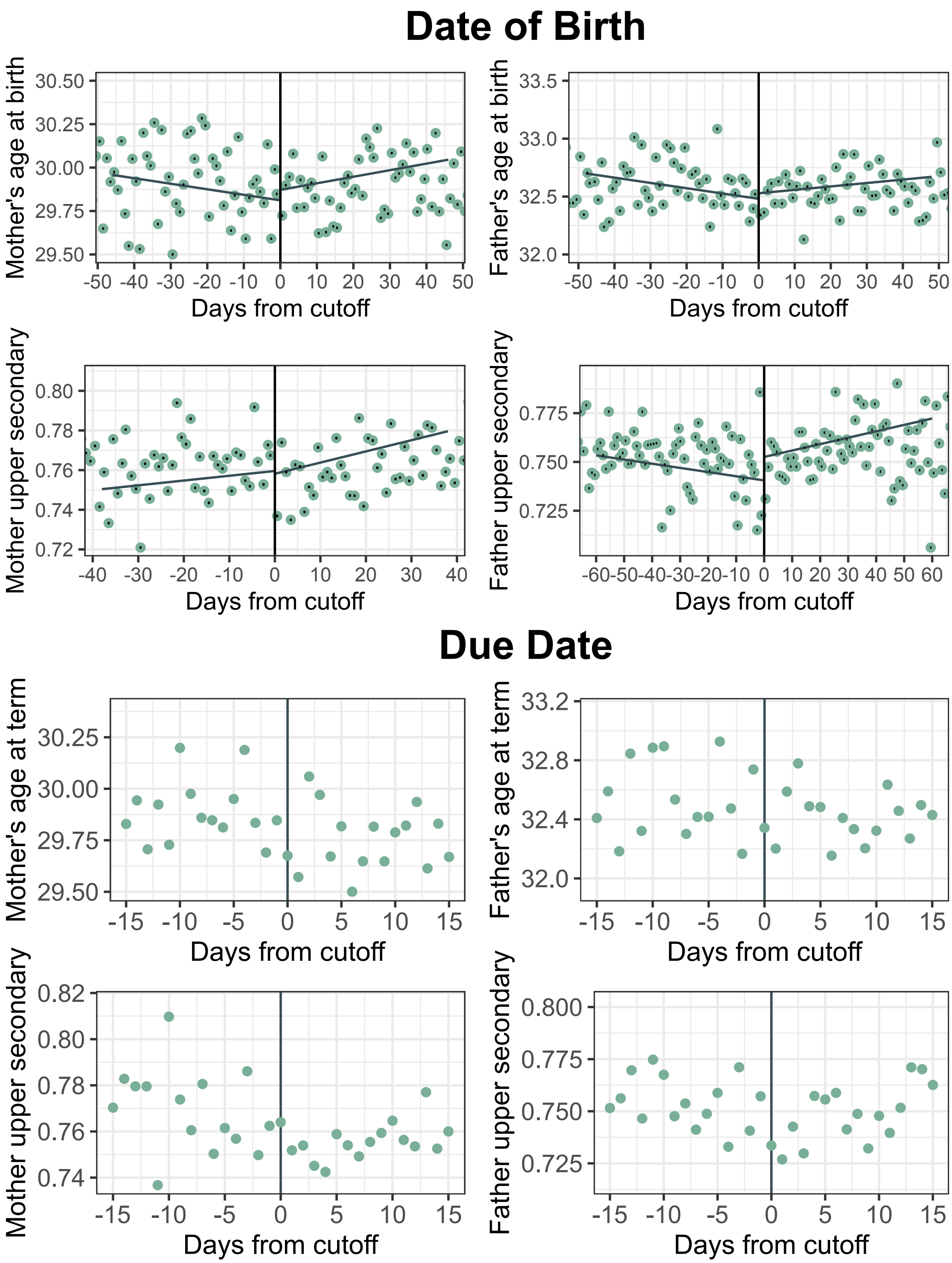

Figure C3: Parental Characteristics for Due Date and Date of Birth around Cutoff. *Note.* Date of Birth: Sample from 1999-2007 birth cohorts (N=407,789). Date of birth recentered around January 1st cutoff and MSE-optimal bandwidth used. Due Date: Sample from 1999-2007 birth cohorts (N=392,311). Due date recentered around the cutoff and restricted to ±15 days around January 1st.

D Effect Estimate Comparison

Tables D1 and D2 provide estimates for the effect of school starting age on ADHD and academic skills using both date of birth and due date by OLS regression, regression discontinuity (RD), and two stage least squares (2SLS) regression. Prior literature on the effect of SSA on a variety of outcomes has utilized either month of birth or date of birth with the assumption that the chosen method is randomly assigned across the cutoff and thus results in unbiased estimates. Through the analysis on shifting of births this assumption has been shown to not be met in every case, and at the least should not be assumed *a priori*. In this paper, I present a novel approach to SSA which uses due date to avoid this selection bias. To compare the effect estimate using the due date instrument (column 6), here I estimate the effect of SSA on ADHD and academic skills using common methods utilized in the previous literature.

Column (1) presents the naïve OLS regression estimates for the association between SSA and ADHD/academic skills. These estimates are downwardly biased compared to the RD and IV estimates and the December vs. January birth month comparison. These estimates are sensitive to bias through selective conception, shifting of births, and redshirting. In column (2), the association between ADHD diagnosis or academic skills and month of birth is estimated comparing December-borns to January-borns. Using this method, the results for ADHD show that being born in January compared to December is associated with a decrease in the likelihood of receiving a diagnosis by 2.3 percentage points or a 38% reduction in risk. For academic skills, the results show that being born in January compared to December is associated with an increase in test scores of about 0.3 standard deviations for 5th grade math and reading tests, and of 0.18 and 0.21 standard deviations for 9th grade math and reading tests, respectively. While this method avoids bias due to redshirting, it is still susceptible to selection bias through selective conception and shifting of births.

Columns (3) and (4) present the first and second stage RD estimates using exact date of birth and mean-squared error (MSE) optimal bandwidths. The 2nd stage RD estimate finds that a one-year incresase in SSA results in a decrease in the likelihood of receiving an ADHD diagnosis of 2.3 percentage points, or a 39% reduction in risk. In the case of academic skills, the RD estimates show that a one-year increase in SSA results in an increase in test scores of about 0.4 standard deviations for 5th grade math and reading tests, and an increase of 0.19 and 0.24 standard deviations for 9th grade math and reading tests, respectively. While the RD method is not susceptible to bias via selective conception, by controlling for the exact date of birth, or bias due to redshirting, the estimate is still biased by the shifting of births near the cutoff.

Columns (5) and (6) provide the 2SLS regression estimates using the due date instrument. For ADHD, the estimate shows that a one-year increase in SSA results in a decrease in the likelihood of receiving a diagnosis by 3.7 percentage points, or a 59% reduction in risk. For academic skills, the effect of a one-year increase in SSA results in an increase in 5th grade test scores of 0.37 standard deviations for math and reading, and an increase of 0.18 and 0.24 standard deviations for the math and reading tests in 9th grade. In this method, the sample is restricted to due dates within ±15 days of the cutoff, therefore the estimates should not be biased through selective conception. Additionally, the estimates are not biased through the shifting of births around the cutoff or through redshirting.

For ADHD diagnosis, the RD appears to under-estimate the effect of SSA compared to the IV estimates, while for academic skills, the RD estimates appear to over-estimate the effect of SSA compared to the IV estimates. However, none of the IV and RD estimates are statistically significantly different, so it can not be concluded with certainty that in this context, RD estimates using date of birth would find statistically significantly different effect sizes even with the bias induced by birth shifting.

Table D1: Methods estimating the effect of school starting age on ADHD diagnosis

	SSA	**Date of Birth**			**Due Date**	
	Naïve OLS Regression:	December vs. January	Regression Discontinuity		IV 2SLS	
			1st stage	2nd stage	1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
	ADHD	ADHD	SSA	ADHD	SSA	ADHD
School Starting Age	-0.008**			-0.023***		-0.037***
	(0.003)			(0.004)		(0.008)
January		-0.023***				
		(0.003)				
Due Date/Birth Date			0.917***		0.028***	
			(0.003)		(0.0003)	
Term/birth year fixed effects	Yes	Yes	Yes		Yes	
F-test of instrument					9175.82	
Risk (%)	-13.1	-38.3	-38.7		-58.8	
Bandwidth (days)			57		15	
N	**42,959**	**42,959**	**81,090**		**19,083**	

Note. Year of term/birth recentered to run from July to June. Robust standard errors in parentheses. Robust first stage F statistic. Regression discontinuity estimate uses MSE-optimal bandwidth. Date of birth and SSA sample from 1999-2004 birth cohorts (N=274,619). Naïve OLS includes those born in December and January. Due date sample includes those with a due date between 1999 and 2004 (N=263,600).
***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

Table D2: Methods estimating the effect of school starting age on academic skills

	SSA	**Date of Birth**			**Due Date**	
	Naïve OLS Regression:	December vs. January	Regression Discontinuity		IV 2SLS	
			1st stage	2nd stage	1st stage	2nd stage
	(1) Test Score	(2) Test Score	(3) SSA	(4) Test Score	(5) SSA	(6) Test Score
5th Grade: Math						
School Starting Age	0.294***			0.375***		0.370***
	(0.009)			(0.015)		(0.026)
January		0.331***				
		(0.008)				
Due Date/Birth Date			0.925***		0.027***	
			(0.003)		(0.0002)	
F-test of instrument					13374.1	
Bandwidth (days)			48		15	
N	**61,763**	**61,763**	**97,951**		**27,926**	
5th Grade: Reading						
School Starting Age	0.305***			0.395***		0.365***
	(0.009)			(0.013)		(0.026)
January		0.344***				
		(0.008)				
Due Date/Birth Date			0.930***		0.027***	
			(0.002)		(0.0002)	
F-test of instrument					13229.3	
Bandwidth (days)			61		15	
N	**61,142**	**61,142**	**123,100**		**27,615**	
9th Grade: Math						
School Starting Age	0.135***			0.190***		0.179***
	(0.009)			(0.015)		(0.026)
January		0.182***				
		(0.008)				
Due Date/Birth Date			0.925***		0.027***	
			(0.003)		(0.0002)	
F-test of instrument					13469.8	
Bandwidth (days)			45		15	
N	**61,149**	**61,149**	**90,699**		**27,639**	
9th Grade: Reading						
School Starting Age	0.161***			0.240***		0.239***
	(0.009)			(0.013)		(0.025)
January		0.209***				
		(0.008)				
Due Date/Birth Date			0.932***		0.027***	
			(0.002)		(0.0002)	
F-test of instrument					13361.5	
Bandwidth (days)			66		15	
N	**61,263**	**61,263**	**133,914**		**27,693**	

Note. Year of term/birth recentered to run from July to June. Adjusted for term/birth year fixed effects. Robust standard errors in parentheses. Robust first stage F statistic. Regression discontinuity estimate uses MSE-optimal bandwidth. Naïve OLS includes those born in December and January.
***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

E Subgroup Analyses for the Effect of School Starting Age

Table E3 presents the effect of SSA and risk of ADHD diagnosis by gender. For boys, the IV estimate shows that a one-year increase in SSA resulted in a decrease in the probability of receiving an ADHD diagnosis of 4.55 percentage points, or 73%. Among girls, the effect of a one-year increase in school start on ADHD diagnosis is a reduction of 2.73 percentage points, corresponding to a 44% reduction in risk. However, the estimates for boys and girls are not statistically significantly different (p-value for difference = 0.23). Similarly as seen in the main estimates, the OLS estimates are downwardly biased compared to the IV regression, and the null hypothesis of a weak instrument is rejected. Tables E4 and E5 present the results for academic skills by gender. Overall, the effect of a one-year increase in SSA on national test scores is slightly larger for girls than boys in both math and reading tests in 5^{th} and 9^{th} grade. For boys, a one-year increase in SSA resulted in an increased score of 0.33 and 0.34 standard deviations for the 5^{th} grade math and reading national tests, respectively. For the 9^{th} grade tests, the increase was smaller, at 0.16 and 0.20 standard deviations for math and reading, respectively. Among girls, a one-year increase in SSA resulted in an increased score of of 0.41 and 0.39 standard deviations for the 5^{th} grade math and reading national tests, respectively. Similarly to the estimates for boys, the effect was smaller for the 9^{th} grade tests at 0.20 and 0.28 standard deviations for math and reading, respectively. However, none of the estimates for boys and girls are statistically significantly different. In general, the OLS estimates are downwardly biased compared to the IV regression estimates, except for the 9^{th} grade math test for boys, where the OLS estimate slightly overestimates the effect.

Table E3: School Starting Age and ADHD Diagnosis by Gender

	OLS Regression	**IV-2SLS**	
	(1)	(2)	(3)
		1st stage	2nd stage
Variables	ADHD Diagnosis	SSA	ADHD Diagnosis
Panel A: Boys			
$S\hat{S}A$	-0.0236***		-0.0455***
	(0.0065)		(0.0124)
Due Date		0.0275***	
$[-15, 15]$		(0.0004)	
Term year fixed effects	Yes	Yes	Yes
F-test of instrument	-	-	4296.26
Risk (%)	-28.0	-	-73.1
N	9,738	9,738	9,738
Panel B: Girls			
$S\hat{S}A$	-0.0168***		-0.0273**
	(0.0046)		(0.0085)
Due Date		0.0285***	
$[-15, 15]$		(0.0004)	
Term year fixed effects	Yes	Yes	Yes
F-test of instrument	-	-	4954.58
Risk (%)	-42.9	-	-43.9
N	9,345	9,345	9,345

Note. Year of term recentered to run from July to June. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1st. Sample includes those with a due date between 1999 and 2004.
***p<0.001, **p<0.01, *p<0.05, †p<0.1

Table E4: School Starting Age and Academic Skills: Boys

		OLS Regression	IV-2SLS	
		(1)	(2)	(3)
			1st stage	2nd stage
National Test	Variables	Test score	SSA	Test score
Boys				
5th Grade: Math	$S\hat{S}A$	0.327***		0.332***
		(0.020)		(0.039)
	Due Date $[-15, 15]$		0.0259***	
			(0.0003)	
	F-test of instrument	-	-	6166.72
	N	**14,163**	**14,163**	**14,163**
5th Grade: Reading	$S\hat{S}A$	0.336***		0.343***
		(0.020)		(0.039)
	Due Date $[-15, 15]$		0.0259***	
			(0.0003)	
	F-test of instrument	-	-	6092.97
	N	**13,946**	**13,946**	**13,946**
9th Grade: Math	$S\hat{S}A$	0.173***		0.160***
		(0.020)		(0.038)
	Due Date $[-15, 15]$		0.0258***	
			(0.0003)	
	F-test of instrument	-	-	6187.29
	N	**14,022**	**14,022**	**14,022**
9th Grade: Reading	$S\hat{S}A$	0.187***		0.201***
		(0.020)		(0.038)
	Due Date $[-15, 15]$		0.0258***	
			(0.0003)	
	F-test of instrument	-	-	6099.18
	N	**14,029**	**14,029**	**14,029**

Note. Year of term recentered to run from July to June. Adjusted for term-year fixed effects. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1st.
***p<0.001, **p<0.01, *p<0.05, †p<0.1

Table E5: School Starting Age and Academic Skills: Girls

National Test	Variables	OLS Regression (1) Test score	IV-2SLS (2) 1st stage SSA	IV-2SLS (3) 2nd stage Test score
Girls				
5th Grade: Math	$S\hat{S}A$	0.362***		0.407***
		(0.019)		(0.034)
	Due Date $[-15, 15]$		0.0275***	
			(0.0003)	
	F-test of instrument	-	-	7329.08
	N	**13,763**	**13,763**	**13,763**
5th Grade: Reading	$S\hat{S}A$	0.353***		0.385***
		(0.019)		(0.035)
	Due Date $[-15, 15]$		0.0275***	
			(0.0003)	
	F-test of instrument	-	-	7244.51
	N	**13,669**	**13,669**	**13,669**
9th Grade: Math	$S\hat{S}A$	0.148***		0.198***
		(0.018)		(0.034)
	Due Date $[-15, 15]$		0.0276***	
			(0.0003)	
	F-test of instrument	-	-	7408.02
	N	**13,617**	**13,617**	**13,617**
9th Grade: Reading	$S\hat{S}A$	0.195***		0.276***
		(0.018)		(0.033)
	Due Date $[-15, 15]$		0.0275***	
			(0.0003)	
	F-test of instrument	-	-	7396.68
	N	**13,664**	**13,664**	**13,664**

Note. Year of term recentered to run from July to June. Adjusted for term-year fixed effects. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1st.
***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

Table E6 presents the effect of SSA and risk of ADHD diagnosis by parental education. High parental education was defined as both parents with at least an upper secondary education, while low parental education was defined as at least one parent without an upper secondary education. For those with high parental education, the IV estimate shows that a one-year increase in SSA resulted in a decrease in the probability of receiving an ADHD diagnosis of 3.80 percentage points, or 92%. Among those with low parental education, the effect of a one-year increase in school start on ADHD diagnosis is a reduction of 3.59 percentage points, corresponding to a 39% reduction in risk. However, there was no statistically significant difference in the estimates between those with low and high parental education (*p*-value for difference = 0.90).

Tables E7 and E8 present the results for academic skills by parental education. Overall, the effect of a one-year increase in SSA on national test scores is slightly larger for those with lower parental education than those with higher parental education in both math and reading tests in 5th, however these estimates were not statistically significantly different (*p*-value for difference in 5th grade math = 0.27, and *p*-value for difference in 5th grade reading = 0.09).

For 9th grade national tests in math and reading, the effect of a one-year increase in SSA was slightly smaller for those with lower parental education than for those with higher parental education, although again these estimates were not statistically significantly different (*p*-value for difference in 9th grade math = 0.62, and *p*-value for difference in 9th grade reading = 0.35). Overall, there did not appear to be any statistically significant differences in the effect of SSA on ADHD or national test scores between those with high and low parental education.

Table E6: School Starting Age and ADHD Diagnosis by Parental Education

	OLS Regression	**IV-2SLS**	
	(1)	(2)	(3)
		1^{st} stage	2^{nd} stage
Variables	ADHD Diagnosis	SSA	ADHD Diagnosis
Panel A: High Parental Education			
$S\hat{S}A$	-0.0177***		-0.0380***
	(0.0043)		(0.0079)
Due Date		0.0286***	
$[-15, 15]$		(0.0004)	
Term year fixed effects	Yes	Yes	Yes
F-test of instrument	-	-	5879.68
Risk (%)	-42.9	-	-92.3
N	11,322	11,322	11,322
Panel B: Low Parental Education			
$S\hat{S}A$	-0.0153*		-0.0359*
	(0.0077)		(0.0149)
Due Date		0.0271*	
$[-15, 15]$		(0.0005)	
Term year fixed effects	Yes	Yes	Yes
F-test of instrument	-	-	3280.32
Risk (%)	-16.6	-	-38.8
N	7,671	7,671	7,671

Note. Year of term recentered to run from July to June. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1^{st}. Sample includes those with a due date between 1999 and 2004. High parental education was defined as both parents with at least an upper secondary education and low parental education was defined as at least 1 parent with less than an upper secondary education.
***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

Table E7: School Starting Age and Academic Skills: High Parental Education

		OLS Regression	IV-2SLS	
		(1)	(2)	(3)
			1st stage	2nd stage
National Test	Variables	Test score	SSA	Test score
High Parental Education				
5th Grade: Math	$S\hat{S}A$	0.377***		0.351***
		(0.017)		(0.030)
	Due Date $[-15, 15]$		0.0272***	
			(0.0003)	
	F-test of instrument	-	-	8984.31
	N	**17,418**	**17,418**	**17,418**
5th Grade: Reading	$S\hat{S}A$	0.354***		0.338***
		(0.016)		(0.030)
	Due Date $[-15, 15]$		0.0271***	
			(0.0003)	
	F-test of instrument	-	-	8898.55
	N	**17,246**	**17,246**	**17,246**
9th Grade: Math	$S\hat{S}A$	0.203***		0.190***
		(0.016)		(0.030)
	Due Date $[-15, 15]$		0.0271***	
			(0.0003)	
	F-test of instrument	-	-	8943.95
	N	**17,210**	**17,210**	**17,210**
9th Grade: Reading	$S\hat{S}A$	0.208***		0.261***
		(0.016)		(0.030)
	Due Date $[-15, 15]$		0.0271***	
			(0.0003)	
	F-test of instrument	-	-	8958.14
	N	**17,241**	**17,241**	**17,241**

Note. Year of term recentered to run from July to June. Adjusted for term-year fixed effects. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1st. High parental education was defined as both parents with at least an upper secondary education.
***p<0.001, **p<0.01, *p<0.05, †p<0.1

Table E8: School Starting Age and Academic Skills: Low Parental Education

National Test	Variables	OLS Regression	IV-2SLS	
		(1)	(2)	(3)
			1st stage	2nd stage
		Test score	SSA	Test score
Low Parental Education				
5th Grade: Math	$S\hat{S}A$	0.305***		0.410***
		(0.023)		(0.044)
	Due Date $[-15, 15]$		0.0259***	
			(0.0004)	
	F-test of instrument	-	-	4358.21
	N	**10,359**	**10,359**	**10,359**
5th Grade: Reading	$S\hat{S}A$	0.305***		0.430***
		(0.023)		(0.046)
	Due Date $[-15, 15]$		0.0259***	
			(0.0004)	
	F-test of instrument	-	-	4304.73
	N	**10,223**	**10,223**	**10,223**
9th Grade: Math	$S\hat{S}A$	0.110***		0.163***
		(0.022)		(0.043)
	Due Date $[-15, 15]$		0.0259***	
			(0.0004)	
	F-test of instrument	-	-	4463.83
	N	**10,286**	**10,286**	**10,286**
9th Grade: Reading	$S\hat{S}A$	0.121***		0.211***
		(0.022)		(0.044)
	Due Date $[-15, 15]$		0.0258***	
			(0.0004)	
	F-test of instrument	-	-	4361.09
	N	**10,309**	**10,309**	**10,309**

Note. Year of term recentered to run from July to June. Adjusted for term-year fixed effects. Robust standard errors in parentheses. Robust first stage F statistic. Sample includes only observations with a due date of ±15 days around January 1st. Low parental education was defined as at least 1 parent with less than an upper secondary education. ***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

F Temporal trends in birth shifting

To examine whether this shifting of births is relevant to cohorts prior to 1995, I analyze birth shifting across the cutoff from 1950 until 2018. Figure F5 shows the share of births shifted from equation 1 estimated separately for each 5-year period within a ±15 day window around the cutoff. Estimates for each period are adjusted for the recentered years of birth within that period. In addition, Figure F4 shows the average daily births in 10-year periods from 1950-2018 for births ±30 days around January 1st.

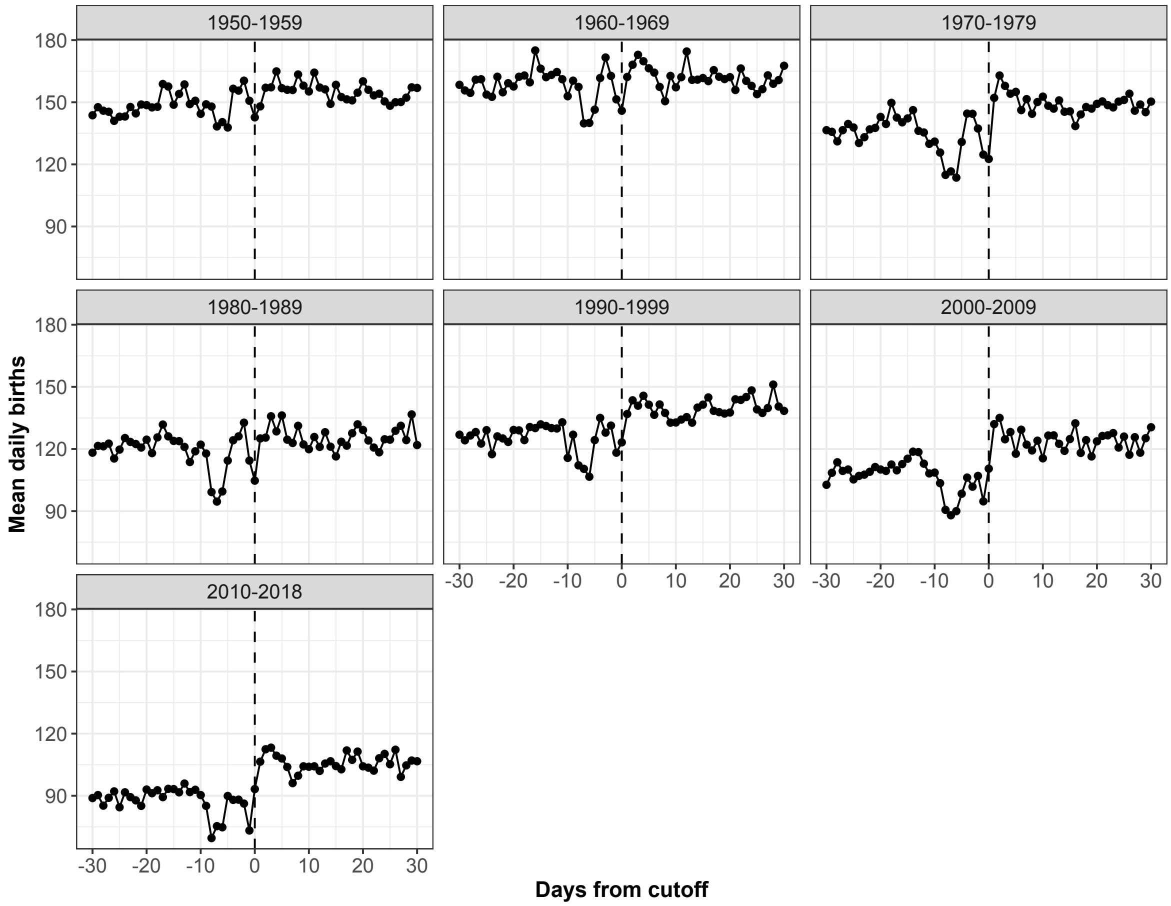

Figure F4: Mean daily births from 1950-2018 in 10-year periods. *Note.* Date of birth recentered around January 1st cutoff.

From figure F5, we see that the shifting of births around the cutoff is not restricted to the most recent cohorts. There appears to be a clear trend of births being shifted from the last 15 days of December to the 15 days of January from the period 1960-1965 until the most recent period of 2015-2018. The first shift towards January births appears to occur during the late 1960's through the 1970's and 1980's. Prior to the late 1960's and 1970's, delayed and early school start was a common occurrence, with parents having much more freedom in this decision. Following a reform in 1969, school psychology services were established to coordinate and assist with special education schools, which were segregated from the general public schools (NOU 1983: 4, 1983). However, a reform in 1976, which had a significant focus on integrating special education schools and general public schools, expanded the responsibilities of the school psychology offices. During this time, the opportunity for children to delay school start was considerably restricted. A standard of children beginning school in the year they turned seven was set, with those receiving a delayed school start the exception. The school psychological services were instructed to grant a delayed school start only in the most exceptional circumstances (NOU 1983: 4, 1983, p.65). In order to receive such exception, parents had to apply to the municipality and the child must be determined to be not developmentally ready to begin school by expert assessment from the school psychology office (Gabrielsen & Lundetræ, 2017).

From the late 1990's, the share of births shifted into January increased again, to around 8%. During the same time, a reform to the educational system was introduced in 1997, with debate and smaller changes already occurring in 1994 (Gabrielsen & Lundetræ, 2017). The reform in 1997 increased compulsory schooling to 10 years length through reducing the age at school start by one year, with children now entering school the year they turn six. This reform was met with resistance and sparked much debate around the age of school start (Gabrielsen & Lundetræ, 2017). To address the concerns surrounding the decreasing age at school start, the reform stipulated that the first year of compulsory school should be seen as an extension of kindergarten rather than an extra year of school, with play and informal learning the focus (Gabrielsen & Lundetræ, 2017). In the national education plan, it was stated that there should not be any formal requirements regarding reading proficiency for the first year of compulsory schooling, and that formal instruction should rather begin in the second year. The increase in the share of births shifted to January may be associated with the changes in the age at school start and the following public debate, which brought attention to the implications of a December birth date for SSA. However, during the same period, the share of births via cesarean-section increased from 2% in 1970 to 8% in 1985 (see Appendix G). Therefore, this shifting of births may also be related to the increase in the use of cesarean-sections which made the shifting of births more feasible.

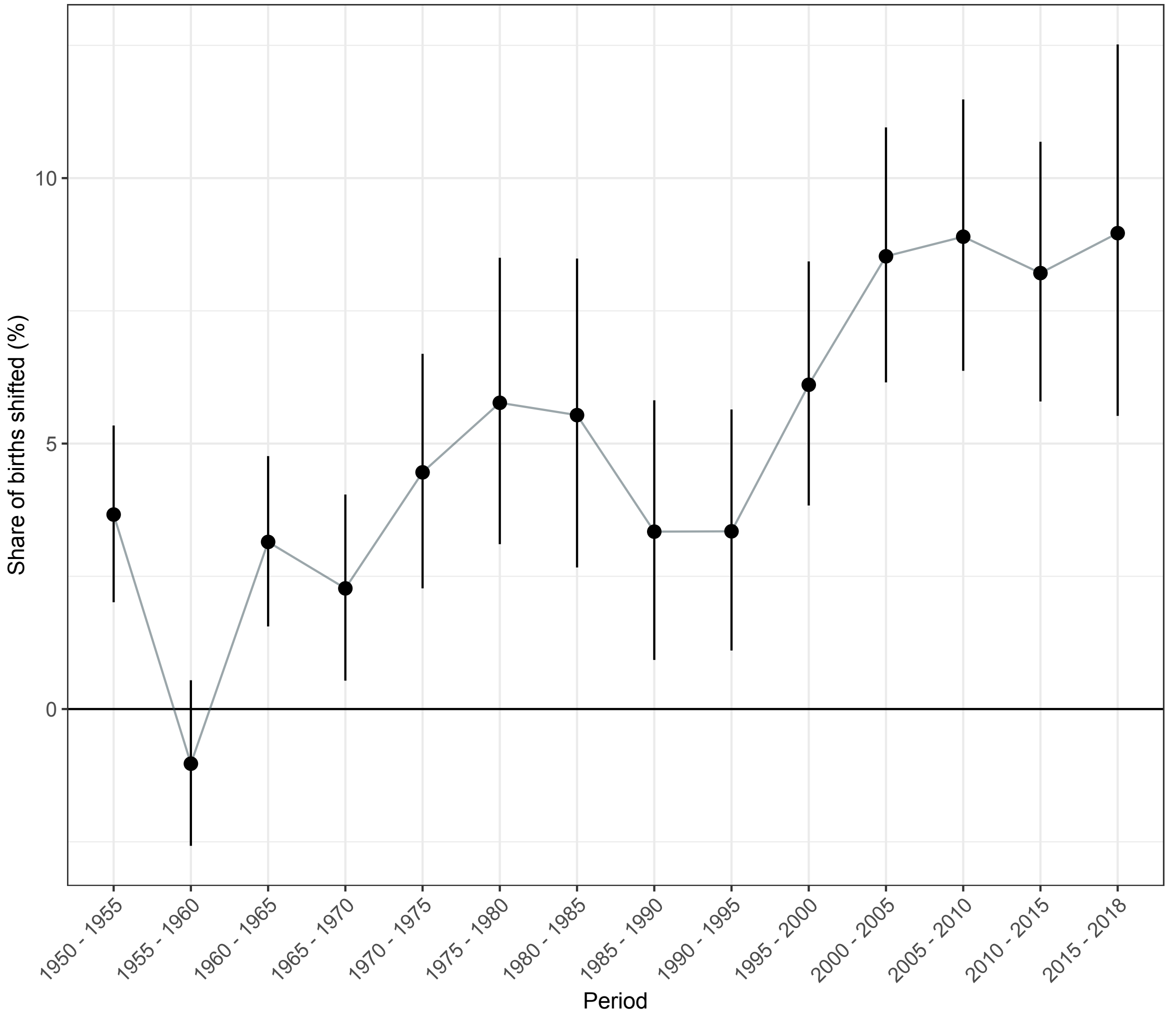

Figure F5: Share of births shifted from 1950-2018. *Note.* Share of births shifted across cutoff from 1950-2018 in 5-year periods with 95% confidence intervals. Date of birth recentered around January 1st cutoff. Sample includes only observations ±15 days around the cutoff. Estimates in each period are adjusted for year of birth recentered to run from July to June.

G Characteristics and Trends by Birth Type

Figure G6 shows the proportion of births born via cesarean-section, induction, and spontaneous births from 1967 to 2018 in Norway. Table G9 presents the descriptive characteristics of births born by cesarean-section, induction, and spontaneous births from 1995 to 2018, including by elective or emergent cesarean-section. Overall, about 70% of births are spontaneous, 17% are induced, and 12.6% are born via cesarean-section. Births via cesarean-section to second-borns or later are more often elective procedures, while for first births born via cesarean-section are more often emergent procedures. More first births are induced compared to second or higher births, and mothers over 30 are more often induced than births to mothers below 30 years old. There are similar proportion of births born via induction and cesarean-section between those of higher maternal or paternal education and those of lower maternal or paternal education. However, births to those with missing education, either maternal or paternal, are more likely to be induced or born via cesarean-section.

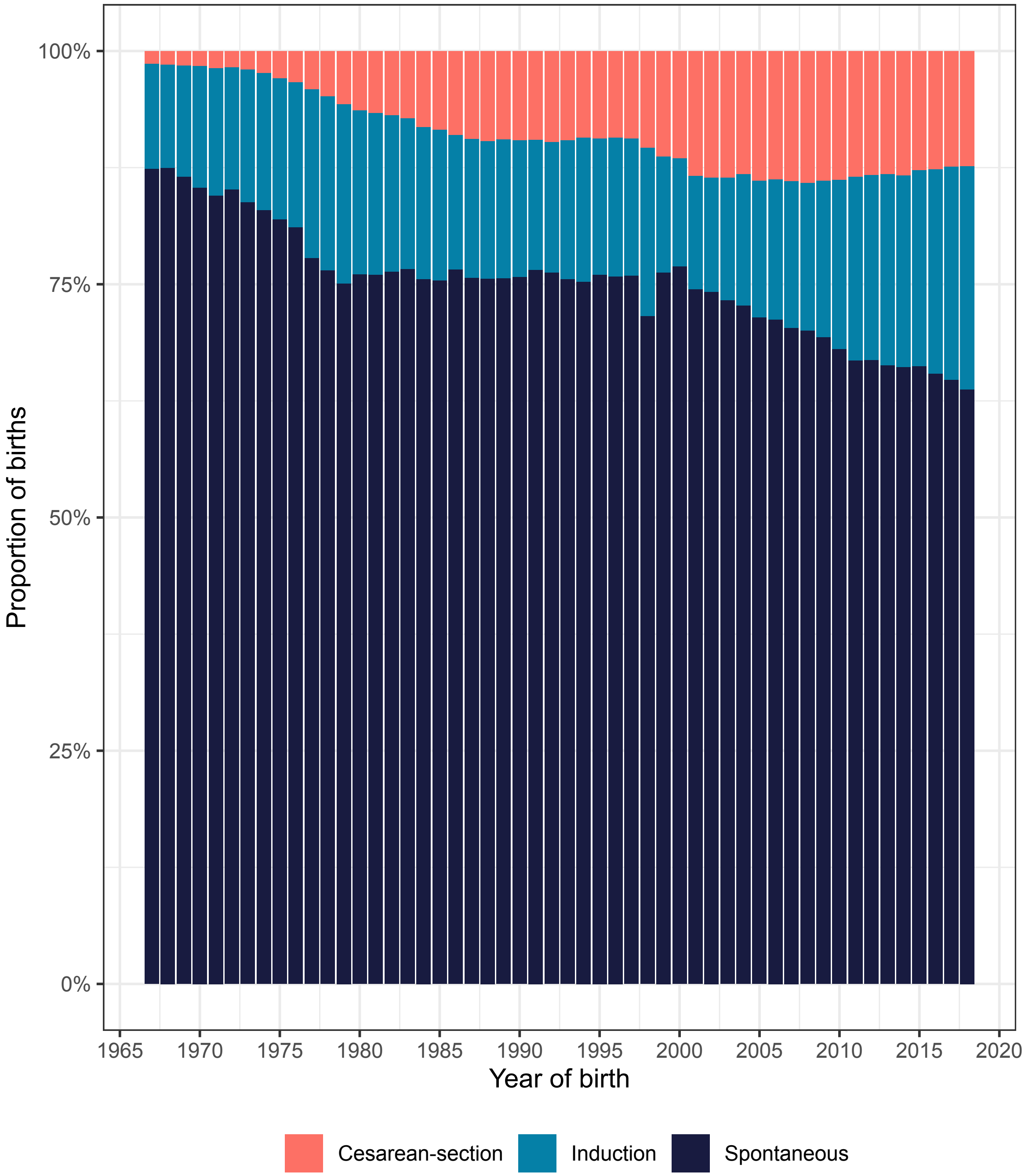

Figure G6: Proportion of births in Norway by type from 1967-2018. *Note.* Data from the Medical Birth Register of Norway (MBRN).

Table G9: Descriptive Characteristics of Births by Type

	(1)	(2)	(3)	(4)	(5)
			Cesarean-Section		
	Spontaneous	**Induction**	Overall	Emergent	Elective
Gender					
Boy	0.694	0.178	0.129	0.070	0.058
Girl	0.718	0.159	0.123	0.064	0.058
Birth Order					
First-born	0.680	0.189	0.132	0.091	0.040
Second-born or later	0.724	0.154	0.122	0.050	0.071
Mother's Age					
Over 30	0.669	0.180	0.151	0.071	0.079
Under 30	0.742	0.157	0.101	0.063	0.038
Mother's Education					
Upper secondary or higher	0.707	0.168	0.125	0.065	0.059
Less than upper secondary	0.703	0.170	0.127	0.069	0.058
Missing	0.694	0.173	0.133	0.081	0.052
Father's Education					
Upper secondary or higher	0.708	0.167	0.125	0.065	0.060
Less than upper secondary	0.708	0.166	0.126	0.070	0.055
Missing	0.675	0.188	0.137	0.081	0.055
Overall	0.706	0.169	0.126	0.067	0.058
N	994,990	237,814	177,510	94,592	82,235

Note. From 1995-2018 birth cohorts. A small number of births via cesarean-section are missing information on whether the procedure was elective or emergent.

H Birth Shifting by Child Characteristics

Table H10 examines the shifts in births by the newborn's gender and birth order. There is no significant difference in the share of boy and girl births shifted across the cutoff. However, second and later births are significantly shifted more often across the cutoff compared to first-borns. Results show that 9.3% of second or higher-order births are shifted compared to 4.6% of first births. This implies that parents may have learned from their first births the potential advantages of their child being born after January 1st as opposed to late December. This may not be concentrated among parents with a first birth in December, but could also be among all parents as they interact with other parents, children, and the school system. Parents may have also learned from the first birth methods for delaying the birth, for example by requesting a delivery scheduled at the end of December to be moved until after the cutoff date, when possible. This may also be related to the tendency that second and higher births born via cesarean-section are more often elective procedures, while first-births via cesarean-section are more often emergent procedures (see Appendix G).

Table H10: Shifts in Birth Timing by Child Characteristics

	Gender		Birth Order	
	(1) Boy	(2) Girl	(3) First-born	(4) Second-born or later
A: Number of births				
January	8.05***	7.16***	4.20***	11.01***
	(0.66)	(0.63)	(0.55)	(0.76)
Births shifted within window (n)	*60*	*54*	*31*	*83*
Adj. R^2	0.57	0.58	0.41	0.65
B: Log number of births				
January	0.144***	0.137***	0.090***	0.178***
	(0.012)	(0.012)	(0.012)	(0.012)
Share shifted within window	*7.5%*	*7.1%*	*4.6%*	*9.3%*
Adj. R^2	0.58	0.59	0.42	0.66
p-value for difference	0.664		<0.001***	
Mean daily births	56	54	47	63
Observations	720	720	720	720

Note. Adjusted for year of birth fixed effects (redefined as running from July to June), weekday, and holiday fixed effects. Robust standard errors in parentheses. Number of daily births come from pooled 1995-2018 birth cohorts and sample includes births within ±15 days around the school entry cutoff date. *January* is a dummy variable for whether or not a birth date is after December 31st. The number of births moved is calculated as $W \times \beta/2$ where W is the number of days in the window (i.e. 15). The share of births shifted is calculated as exp(β/2)-1. Observations are days per year.
***$p<0.001$, **$p<0.01$, *$p<0.05$, †$p<0.1$

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