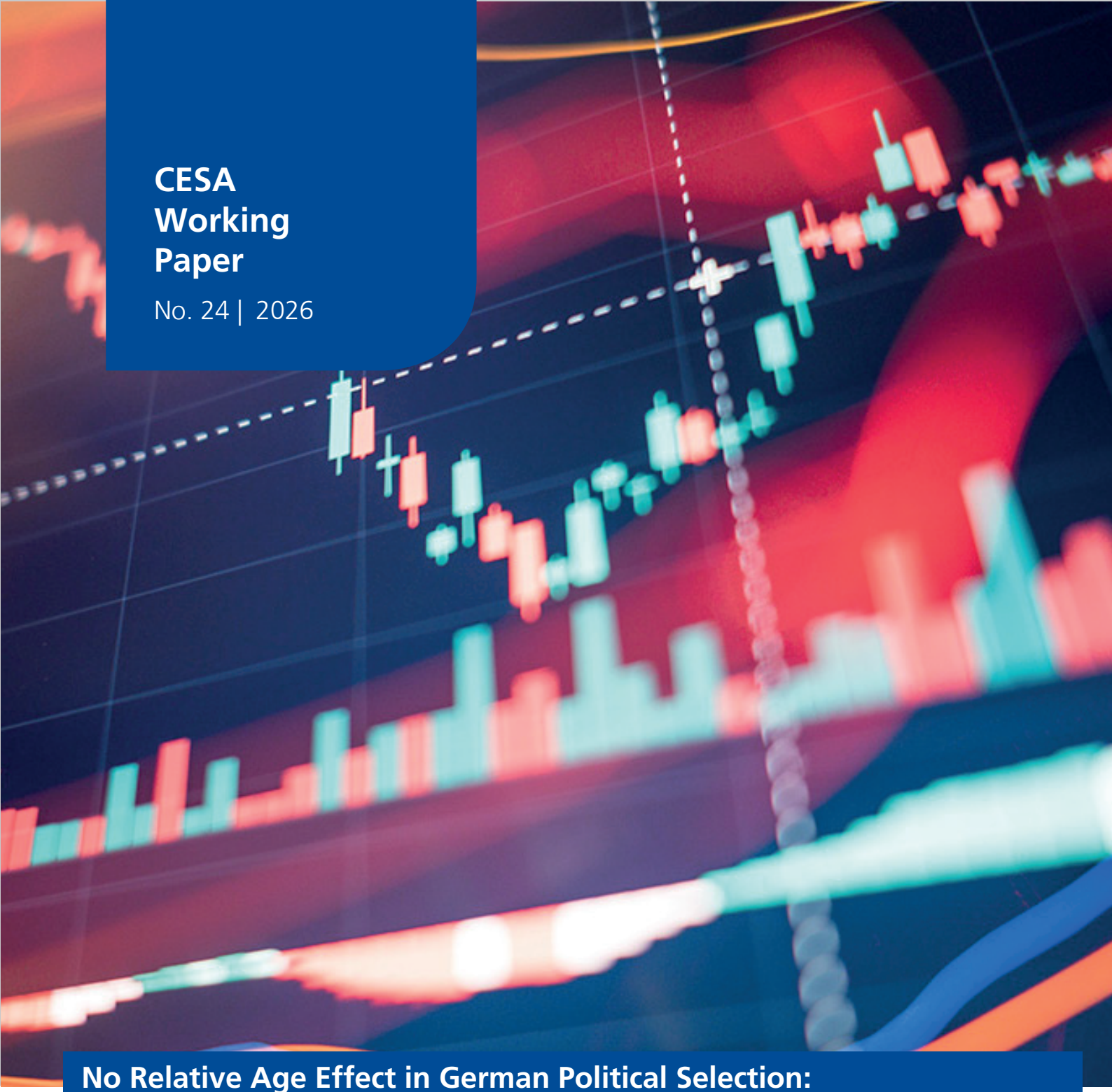

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No Relative Age Effect in German Political Selection: Evidence from Bundestag Data

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No Relative Age Effect in German Political Selection: Evidence from Bundestag Data*

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Abstract

Does relative age at school entry shape political selection? Using the exact birth dates of 1,040 members of the German Bundestag (periods 2013–2026) and school-entry cut-offs reconstructed by state and year of enrolment, we find no evidence that members born just after the cut-off are overrepresented. The estimated sawtooth parameter is close to zero ($p = 0.89$), and a local density test finds no discontinuity at the cut-off ($p = 0.39$). Results are robust across gender, mandate type and region. Relative age effects in political selection are thus not universal.

Keywords: relative age effect, political selection, school entry, Bundestag
JEL codes: D72, I21, J24, C14

*We thank Monika Hettwer for her help in collecting the data. Data and code are available from the authors upon request and will be made available in a public repository upon publication.

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1 Introduction

Children who are among the oldest in their school cohort tend to outperform their younger classmates. This relative age effect (RAE) occurs because school cohorts are formed based on a cut-off date, meaning children born just after the date are almost a year older than those born just before it. These differences in maturity have been shown to affect test scores and educational attainment (Bedard and Dhuey, 2006), selection into youth sports (Barnsley et al., 1985; Helsen et al., 2000), leadership in high school (Dhuey and Lipscomb, 2008) and the likelihood of becoming a CEO (Du et al., 2012).

Some research has investigated whether relative age also shapes political selection. Muller and Page (2016) find that members of the US Congress born shortly after the school-entry cut-off are overrepresented by about 50% relative to those born shortly before it. In Finland, relatively old male candidates are more likely to be elected to parliament, but only in highly competitive districts (Tukiainen et al., 2019). In Belgium, members of the federal parliament born just after the cut-off are overrepresented by up to 90%, whereas no such effect is observed among local politicians (Al Yussef et al., 2023).

We examine this phenomenon in the context of the German Bundestag, analysing data on all 1,040 members from the four most recent legislative periods (2013–2026) who were born after the harmonisation of school-entry rules in the 1960s. Specifically, we ask whether the relatively old are overrepresented in parliament, which captures the combined effect of all stages of political selection. Germany offers a useful setting for two reasons. First, school-entry cut-offs are set by state law and can be reconstructed for each member from the place and date of birth. Second, the relevant cut-off date, 30 June in most cases, falls in the middle of the year. Studies that rely on calendar-year cohorts compare births in late December and early January, when the timing of births responds to fiscal incentives (Dickert-Conlin and Chandra, 1999), whereas a mid-year cut-off avoids this.

We find no evidence of a relative age effect. The distribution of relative age is almost uniform, and a sawtooth test yields a point estimate close to zero. The confidence interval rules out effects of the size reported for the US and Belgium. A local density test shows no discontinuity at the cut-off, and the actual cut-off does not stand out relative to placebo cut-offs. No subgroup shows a significant effect, whether men or women, directly elected or list members, or members born in East or West Germany.

Our findings indicate that relative age effects in political selection are not universal. They add a well-powered null result to a literature dominated by positive findings, and they call for caution in generalising the political consequences of school-entry rules across countries.

2 Data and Methods

2.1 Data

We use the official master data on all members of the German Bundestag provided by the Bundestag's open-data service (Deutscher Bundestag, 2026). The data include each member's exact date and place of birth, gender, party, and mandates. We consider all 1,498 individuals who have served in at least one of the four most recent legislative periods (2013–2026). Each individual is included in the data once. We exclude 75 members who were born abroad or whose place of birth was unclear. For all others, the federal state (*Land*) of the place of birth serves as a proxy for the state of school enrolment.

2.2 Relative age

In Germany, compulsory schooling begins at the start of the school year in which a child has turned six by a cut-off date set by state law. Until 1966, the school year began in spring in all West German states except Bavaria, and cut-off dates varied between states and over time. The Hamburg Agreement of 1964 moved the start of the school year to 1 August and set a uniform cut-off of 30 June (Kultusministerkonferenz, 1964). The change was completed in 1967 (Pischke, 2007). In the GDR, the cut-off was 31 May. After reunification, the new states adopted 30 June. From 2003 onwards, several states moved their cut-offs to later in the year (Berthold, 2008).

As cut-offs prior to the 1967 harmonisation cannot be reliably reconstructed, our main sample comprises individuals born on or after 1 July 1961, the first birth cohort to be enrolled entirely under the harmonised rules ($N = 1{,}040$). For each member, we assign the cut-off in force in the state and year of school entry: 31 May for enrolment in the GDR (148 members), 30 June otherwise, and the later, state-specific dates for the three members enrolled after 2005.[1] We measure relative age y_i as the distance between member i's birthday and the cut-off date, expressed as a share of the cohort year.[2] We centre y_i on the cut-off and fold it into the interval $[-1/2, 1/2)$. Members born just after the cut-off are the oldest in their cohort and have small positive values, while those born just before it are the youngest in the preceding cohort and have small negative values. In the absence of a relative age effect and of birth seasonality, y is uniformly distributed.

[1] Under § 187(2) of the German Civil Code, a child born on 1 July is legally deemed to be six years old on 30 June. With a 30 June cut-off, the relatively oldest children of a cohort are therefore born on 2 July. Table A.1 in the Appendix lists all cut-offs and sources.

[2] The cohort year is the period between two consecutive cut-offs. Normalising by its actual length accounts for leap years and transition cohorts.

2.3 Empirical approach

Following Muller and Page (2016), we first estimate the sawtooth density in Equation (1),

$$f(y) = 1 + d\left[\mathbf{1}(y \geq 0) - y - 1/2\right], \tag{1}$$

where d is the sawtooth parameter. A positive d indicates an overrepresentation of members born just after the cut-off that fades out linearly over the cohort. In a robustness check, we replace the uniform baseline with the monthly distribution of live births in Germany (Figure A.3 in the Appendix).

Second, we test for a discontinuity in the density at the cut-off, using the local polynomial density estimator of Cattaneo et al. (2020) with a local linear density approximation, a triangular kernel and MSE-optimal bandwidths chosen separately on each side. We report the estimated jump $\hat{f}(0^+) - \hat{f}(0^-)$ with robust bias-corrected inference. As local density estimates are imprecise in samples of this size, we compare the estimated jump with those obtained at placebo cut-offs placed at half-month intervals throughout the year. We also estimate a donut specification that excludes members born within three days of the cut-off. Results of the McCrary test used by Muller and Page (2016) are reported in the Appendix.

3 Results

Figure 1(a) shows the distribution of relative age in the main sample. There is no visible concentration of members born shortly after the cut-off, and the estimated sawtooth density is almost flat. The point estimate is $\hat{d} = 0.01$, with a 95% confidence interval of $[-0.19, 0.22]$ ($p = 0.89$; see Table 1). Since Equation (1) implies that the ratio of the densities just after and just before the cut-off is $f(0^+)/f(0^-) = (1 + d/2)/(1 - d/2)$, the upper bound of the 95% confidence interval corresponds to an overrepresentation of 25% of members born just after the cut-off relative to those born just before it. For comparison, the estimate of $d = 0.42$ for the US Congress (Muller and Page, 2016) corresponds to an overrepresentation of 53%. We reject an effect of this size at $p < 0.001$. In 1,000 samples of $N = 1{,}040$ drawn from Equation (1) with $d = 0.42$, the test rejects $d = 0$ at the 5% level in 97% of cases. Allowing for the seasonality in births leaves the estimate virtually unchanged ($\hat{d} = 0.03$), and a χ^2 test across the twelve monthly bins of relative age does not reject a uniform distribution ($p = 0.37$; Table A.2).

The null result holds across subgroups (see Table 1). None of the estimates are significant. This includes men, for whom relative age effects in political selection have been found to be concentrated (Tukiainen et al., 2019; Al Yussef et al., 2023): here, $\hat{d} = -0.10$, with an upper bound of 0.15. Estimates for smaller groups, notably women, are less precise. Directly elected members, who face the most individual competition, show no sign

of an effect either ($\hat{d} = 0.04$).

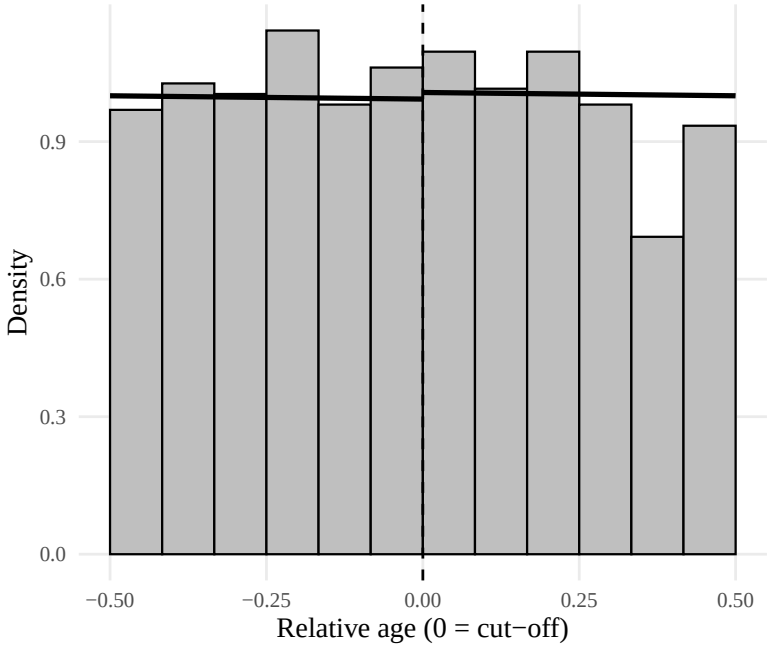
The local density test shows that the density is virtually continuous at the cut-off (Figure A.2). The estimate is 1.07 just before and 1.05 just after the cut-off, representing a jump of -0.01 ($p = 0.39$). Since the robust 95% confidence interval is wide ($[-0.39, 0.99]$), the local test alone cannot rule out a jump of the size estimated for the US Congress (0.34; Muller and Page, 2016). The placebo cut-offs, however, support the null result: the actual cut-off does not stand out, and 20 out of 22 placebo cut-offs produce a larger jump in absolute value (see Figure 1(b)). The result is also robust to excluding members born within three days of the cut-off (jump of -0.11, $p = 0.90$) and to restricting the sample to directly elected members (-0.07, $p = 0.72$; Table A.3). The estimated jump only turns positive for narrow bandwidths, reaching 0.68 for a bandwidth of one month ($p = 0.10$). For bandwidths of three months or more, it is again close to zero (Table A.3 and Figure A.1). The McCrary test, with its narrower automatic bandwidth, yields a larger estimate that is significant only at the 10% level ($\hat{\theta} = 0.44$, $p = 0.07$; Table A.3, Panel B).

These positive estimates at narrow bandwidths, including the weakly significant McCrary estimate, are driven by a small cluster of births right after the cut-off rather than by a systematic advantage of the relatively old: within ten days on either side, 38 members are born after and 20 before it. In an exact binomial test, this imbalance is significant ($p = 0.025$), and only 2% of the 364 placebo cut-offs yield an equally low p-value. After adjusting for the choice of window, the evidence is weak (p between 0.04 and 0.12; Table A.4). Moreover, this excess disappears beyond three weeks (95 versus 87 members within 30 days), is absent among directly elected members (14 versus 11), and involves fewer than 20 individuals. It is therefore inconsistent with the gradual advantage of the relatively old that a relative age effect implies.

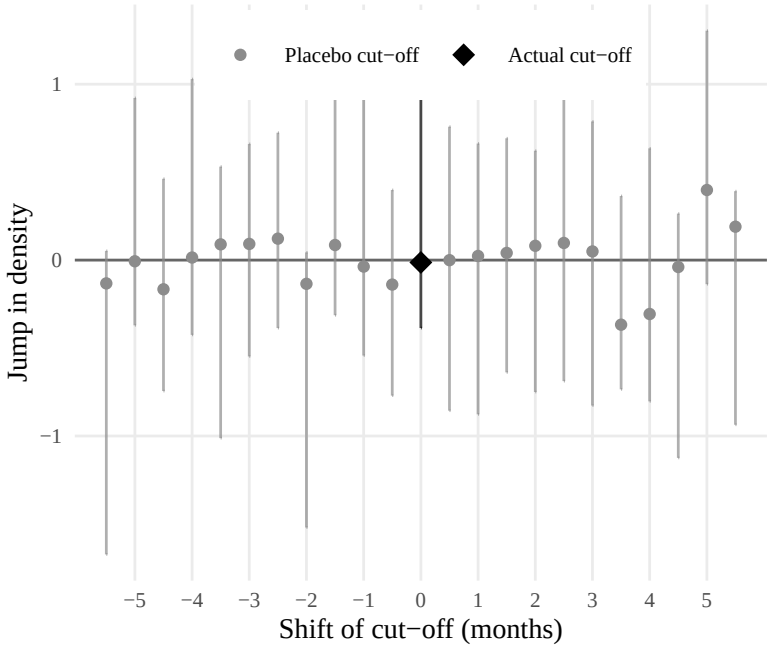
Table 1: Sawtooth estimates

	N	$\hat{d}$	95% CI	$p\ (d = 0)$
Main sample	1,040	0.01	$[-0.19,\ 0.22]$	0.89
Seasonal baseline[a]	1,040	0.03	$[-0.18,\ 0.23]$	0.79
Men	677	-0.10	$[-0.35,\ 0.15]$	0.42
Women	363	0.25	$[-0.11,\ 0.60]$	0.17
Direct mandate[b]	383	0.04	$[-0.29,\ 0.37]$	0.82
List mandate[b]	657	-0.00	$[-0.26,\ 0.26]$	0.99
West Germany[c]	856	0.07	$[-0.16,\ 0.30]$	0.56
East Germany[c]	184	-0.23	$[-0.70,\ 0.26]$	0.36

Notes: Estimates of the sawtooth parameter d in Equation (1). The main sample comprises members of the 18th to 21st Bundestag born on or after 1 July 1961. [a] Seasonal baseline: density relative to the monthly distribution of live births in Germany, 1961–2002. [b] Mandate type refers to the most recent legislative period. [c] East/West refers to the place of birth. p-values are not adjusted for multiple testing.



(a) Distribution of relative age



(b) Actual vs. placebo cut-offs

Figure 1: Relative age of members of the Bundestag. Panel (a): histogram of relative age in the main sample ($N = 1{,}040$) with the estimated sawtooth density. Panel (b): estimated jump in the density at the actual and at placebo cut-offs, with robust 95% confidence intervals.

4 Discussion and conclusion

We find no systematic advantage of the relatively old in the German Bundestag. This is not because relative age is irrelevant in German schools: it does affect track choice in secondary education (Mühlenweg and Puhani, 2010). However, this advantage does not carry over to adult wages and occupations, as the German school system allows students to switch tracks later on (Dustmann et al., 2017). Three features of the setting may further weaken its impact on political selection.

First, compliance with the cut-off is imperfect. Children born shortly before the cut-off date can be deferred, while those born shortly after it can start school early, blurring the link between birth date and relative age. Our estimates, like those for the US and Belgium, measure the effect of being born after the cut-off rather than of being relatively old. Nevertheless, attenuation would have to be substantial to conceal an effect of the size documented by Muller and Page (2016). Second, candidates for the Bundestag are selected by party bodies, and many of its members enter parliament via party lists. Individual competition, which appears to drive relative age effects in Finland (Tukiainen et al., 2019), may therefore matter less. However, the absence of an effect among directly elected members, and its presence in the Belgian list system (Al Yussef et al., 2023), suggest that electoral rules alone do not explain the difference. Third, overrepresentation in parliament combines selection into candidacy with electoral success. In Finland, the relatively old are less likely to run but more likely to be elected (Tukiainen et al., 2019). Offsetting effects of this kind cannot be ruled out, given that official candidate lists only report the year of birth.

Overall, our findings indicate that the relative age effects documented for other parliaments do not generalise to Germany. Whether early advantages in school translate into political careers appears to depend on the context.

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Appendix

A.1 School-entry cut-offs

Table A.1 lists the cut-off dates used to compute relative age. A child is subject to compulsory schooling in a given school year if it has completed its sixth year by the cut-off date. Under German law, a child completes its sixth year on the day before its sixth birthday. With a 30 June cut-off, a child born on 1 July is thus the youngest of its enrolment cohort, and the relatively oldest are born from 2 July. For the GDR cut-off of 31 May, the relatively oldest are born from 2 June.

Members are assigned to the state of their place of birth. Members born in the territory of the GDR are assigned the GDR cut-off if they were enrolled up to 1990 (born before 2 June 1984), and the cut-off of their state otherwise. The post-2003 changes affect only three members of the main sample, born in 2001 and 2002 in Baden-Württemberg, Bavaria and Rhineland-Palatinate. Cohorts enrolled in a year in which the cut-off moved are longer than one year. Relative age is therefore normalised by the actual length of each cohort.

Table A.1: School-entry cut-offs by state and year of enrolment

State	Year of enrolment	Child must be six years old by
West German states	1967 onwards	30 June, unless listed below
East German states	until 1990	31 May (GDR)
	1991 onwards	30 June, unless listed below
Baden-Württemberg	2005 / 2006	31 July / 31 August
	2007–2019	30 September
	2020 / 2021	31 August / 31 July
	2022 onwards	30 June
Bavaria	2005 / 2006 / 2007	31 July / 31 August / 30 September
	2008 / 2009	31 October / 30 November
	2010 onwards	30 September (from 2019, parents of children born 1 July–30 September may defer enrolment)
Berlin	2005–2016	31 December
	2017 onwards	30 September
Brandenburg	2005 onwards	30 September
North Rhine-Westphalia	2007–2008	31 July
	2009–2010	31 August
	2011 onwards	30 September
Rhineland-Palatinate	2008 onwards	31 August
Thuringia	2003 onwards	1 August

Notes: Before the Hamburg Agreement of 1964 (Kultusministerkonferenz, 1964), the school year began in spring in all West German states except Bavaria; the switch to a school year starting on 1 August was completed in 1967 (Pischke, 2007). Members born before 1 July 1961 are therefore excluded from the main sample. Later changes follow Berthold (2008), Landtag von Baden-Württemberg (2003), Landtag Nordrhein-Westfalen (2015), Bayerischer Landtag (2018), Freistaat Bayern (2026) and Kultusministerkonferenz (2023).

A.2 Additional sawtooth results

Table A.2 reports sawtooth estimates for further samples together with χ^2 goodness-of-fit tests across the twelve monthly bins of relative age, against a uniform distribution and against the monthly distribution of live births in Germany (1961–2002). Panel A repeats the samples of Table 1. Panel B reports two robustness checks: members with at least one direct mandate in the 18th to 21st Bundestag, and the main sample excluding six members whose place of birth could not be assigned to a state with certainty.

With $N = 1{,}040$, the minimum detectable effect at 80% power and a 5% significance level is $d = (1.96 + 0.84)\sqrt{12/N} = 0.30$, where $\sqrt{12/N}$ is the asymptotic standard error of $\hat{d}$ under $d = 0$. Simulations from the sawtooth density in Equation (1) (1,000 replications per value of d, likelihood-ratio test of $d = 0$ at the 5% level) give a power of 17%, 48%, 79% and 97% for $d = 0.1$, 0.2, 0.3 and 0.42, respectively; for directly elected members ($N = 383$), the power against $d = 0.42$ is 69%.

Table A.2: Sawtooth estimates and χ^2 tests, all samples

				p-value		χ^2, p-value	
	N	$\hat{d}$	95% CI	$d = 0$	$d = 0.42$	uniform	births[e]
Panel A: samples of Table 1							
Main sample	1,040	0.01	[−0.19, 0.22]	0.89	<0.001	0.37	0.73
Men	677	−0.10	[−0.35, 0.15]	0.42	<0.001	0.01	0.08
Women	363	0.25	[−0.11, 0.60]	0.17	0.350	0.86	0.69
Direct mandate[a]	383	0.04	[−0.29, 0.37]	0.82	0.023	0.19	0.31
List mandate[a]	657	−0.00	[−0.26, 0.26]	0.99	0.002	0.45	0.61
West Germany[b]	856	0.07	[−0.16, 0.30]	0.56	0.002	0.24	0.38
East Germany[b]	184	−0.23	[−0.70, 0.26]	0.36	0.008	0.22	0.33
Panel B: further samples							
Ever directly elected[c]	437	0.12	[−0.19, 0.43]	0.45	0.056	0.30	0.49
Excluding ambiguous birthplaces[d]	1,034	0.03	[−0.18, 0.23]	0.80	<0.001	0.36	0.72

Notes: Estimates of the sawtooth parameter d in Equation (1). The main sample comprises members of the 18th to 21st Bundestag born on or after 1 July 1961. $d = 0.42$ is the estimate for the US Congress (Muller and Page, 2016). χ^2 tests across the twelve monthly bins of relative age. [a] Mandate type refers to the most recent legislative period. [b] East/West refers to the place of birth. [c] At least one direct mandate in the 18th to 21st Bundestag. [d] Excluding six members whose place of birth could not be assigned to a state with certainty. [e] Monthly distribution of live births in Germany, 1961–2002. p-values are not adjusted for multiple testing; after a Holm correction across all tests of $d = 0$ and all χ^2 tests in the table, none is significant at the 10% level.

A.3 Local density tests

Table A.3 reports the local density test for alternative specifications and, for comparison with Muller and Page (2016), the test of McCrary (2008). Figure A.1 plots the estimated jump against the bandwidth. Figure A.2 shows the estimated density on both sides of the cut-off.

Table A.3: Local density tests at the cut-off

	Bandwidth	Estimate	95% CI	p
Panel A: local polynomial density estimator, jump $\hat{f}(0^+) - \hat{f}(0^-)$				
Data-driven (main specification)	0.21 / 0.27	−0.01	[−0.39, 0.99]	0.39
Locally constant density	0.21 / 0.18	−0.04	[−0.46, 0.53]	0.88
Fixed bandwidth, 1 month	0.083	0.68	[−0.17, 2.06]	0.10
Fixed bandwidth, 2 months	0.167	0.16	[−0.11, 1.50]	0.09
Fixed bandwidth, 3 months	0.250	−0.01	[−0.50, 0.84]	0.62
Fixed bandwidth, 4 months	0.333	−0.06	[−0.54, 0.64]	0.86
Fixed bandwidth, 6 months	0.500	−0.01	[−0.58, 0.38]	0.68
Donut, ±3 days excluded	0.27 / 0.23	−0.11	[−0.92, 0.81]	0.90
Direct mandate	0.20 / 0.15	−0.07	[−1.59, 1.09]	0.72
Panel B: McCrary (2008) test, log difference $\hat{\theta}$				
Automatic bin size and bandwidth	0.17	0.44	[−0.04, 0.91]	0.07
Bin size 7 days	0.083	0.97	[0.20, 1.73]	0.01
Bin size 7 days	0.167	0.44	[−0.04, 0.92]	0.07
Bin size 7 days	0.25	0.27	[−0.11, 0.65]	0.17
Bin size 7 days	0.50	0.13	[−0.13, 0.38]	0.33
Bin size 14 days	0.25	0.21	[−0.16, 0.59]	0.27
Bin size 14 days	0.50	0.11	[−0.14, 0.37]	0.39
Automatic, direct mandate	0.18	0.14	[−0.53, 0.82]	0.68

Notes: Bandwidths as shares of the cohort year (left / right of the cut-off where they differ). Panel A: estimator of Cattaneo et al. (2020) with local linear density approximation (unless stated otherwise), triangular kernel and MSE-optimal bandwidths; conventional point estimates, robust bias-corrected confidence intervals and p-values (the intervals are therefore not centred on the point estimates). Panel B: histogram-based estimator with local linear smoothing and triangular kernel; automatic bin size (6.4 days), bandwidth and standard errors following McCrary (2008). The corresponding estimates for the US Congress are a jump of 0.34 and $\hat{\theta} = 0.36$ (Muller and Page, 2016).

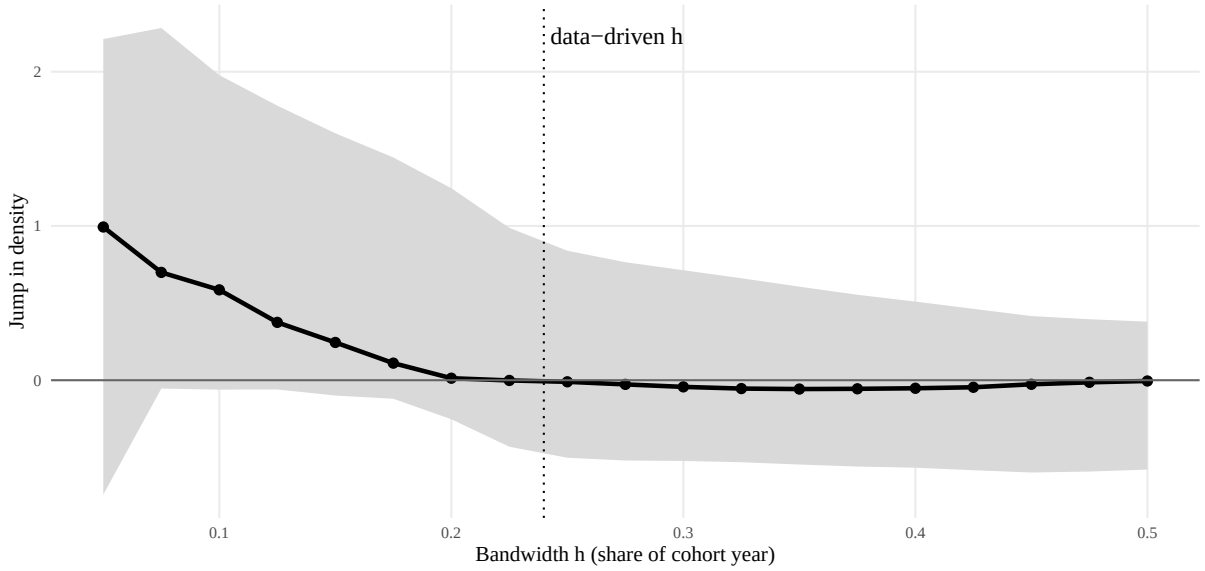

Figure A.1: Estimated jump in the density by bandwidth

Notes: Local polynomial density estimator (Cattaneo et al., 2020) with local linear density approximation and fixed bandwidths h (share of the cohort year); shaded area: 95% confidence intervals from robust bias-corrected inference. The dotted vertical line marks the data-driven bandwidth of the main specification.

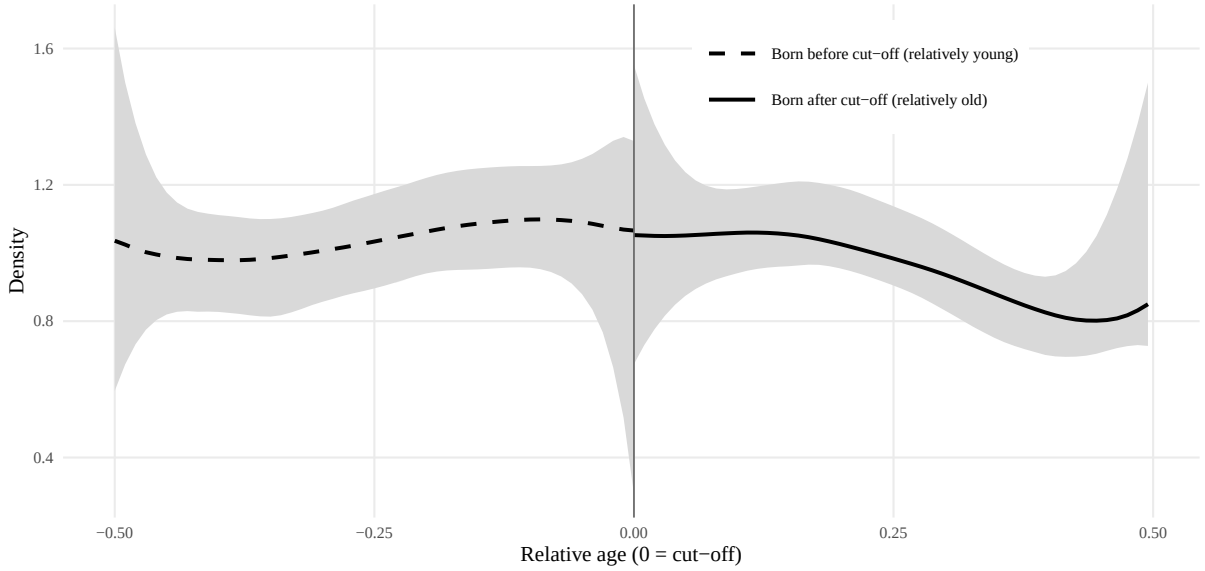

Figure A.2: Estimated density of relative age around the cut-off

Notes: Local polynomial density estimates on either side of the cut-off (dashed: born before, solid: born after the cut-off); shaded areas: 95% confidence intervals from robust bias-corrected inference. Main specification of Table A.3, Panel A.

A.4 Births in narrow windows around the cut-off

Table A.4 compares the number of members born shortly before and shortly after the cut-off. For each window of $\pm w$ days, covering one to three weeks and one month ($w = 7, 10, 14, 21, 30$), an exact binomial test checks whether members are equally likely to be born before and after the cut-off. To assess how unusual these imbalances are, given that

birthdays cluster by chance over the year, we repeat each test at each of the 364 other days of the year as placebo cut-offs. The last column reports the share of placebo cut-offs with a p-value at least as low as at the actual cut-off.

The smallest p-value across the five windows is 0.025 ($\pm$10 days). Since we report the most pronounced of five windows, this p-value overstates the evidence. A Bonferroni correction multiplies it by the number of windows, which yields 0.12. This correction is conservative, because the windows are nested and the tests are therefore highly correlated. Alternatively, we determine the smallest p-value across the five windows at each placebo cut-off and compute the share of placebo cut-offs at which this minimum is at least as low as at the actual cut-off. This accounts for the correlation between windows and yields an adjusted p-value of 0.04.

Table A.4: Members born within narrow windows around the cut-off

	Members born			Share of placebo
Window	before	after	p	cut-offs with p as low
Panel A: main sample				
$\pm$7 days	11	25	0.029	0.016
$\pm$10 days	20	38	0.025	0.019
$\pm$14 days	32	48	0.093	0.066
$\pm$21 days	51	69	0.120	0.041
$\pm$30 days	87	95	0.604	0.382
Panel B: subgroups, $\pm$10 days				
Men	14	26	0.081	
Women	6	12	0.238	
Direct mandate	11	14	0.690	
List mandate	9	24	0.014	
West Germany	16	34	0.015	
East Germany	4	4	1.000	

Notes: Exact binomial tests of equal numbers of members born before and after the cut-off. Placebo cut-offs: each of the 364 other days of the year. Exploratory analysis; p-values not adjusted for multiple testing.

A.5 Birth months

Figure A.3 compares the birth months of members in the main sample with the monthly distribution of live births in Germany, which serves as the seasonal baseline in Section 2. Apart from fewer members born in November, the birth months of members are broadly in line with the seasonal pattern of the population. For the main sample, the χ^2 tests in Table A.2 reject neither a uniform distribution nor the distribution of live births.

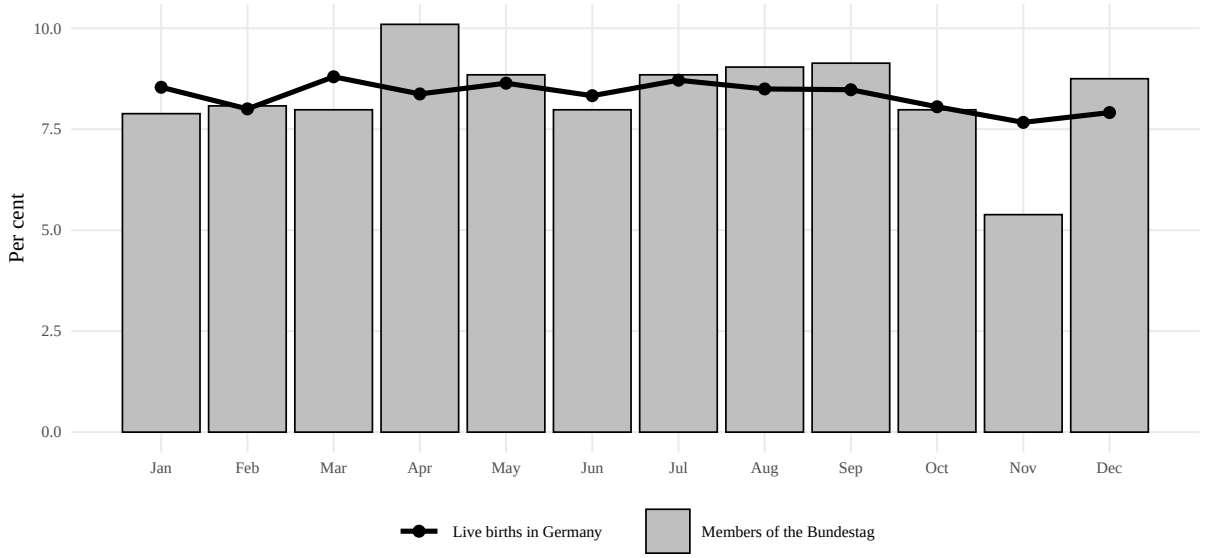

Figure A.3: Birth months of members of the Bundestag and of the population

Notes: Main sample ($N = 1{,}040$). Live births in Germany by month, 1961–2002 (Federal Statistical Office).

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