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Are Campaign Promises Effective?

Manuscript

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Abstract

In democracies, political parties promise to expand social benefits to attract voters in the lead-up to elections. However, we know relatively little whether such campaign promises effectively sway benefiting voters. Using a regression-discontinuity design, we estimate the causal effects of an electoral pledge made by the German conservative party to expand pension benefits ahead of the parliamentary election in 2013. The results show that the promise increased alignment with the pledge-making party by 12.2% among eligible beneficiaries. These gains originate from the re-alignment of individuals who traditionally support left-wing platforms, while it had no mobilizing effect on inactive voters. In addition, we find that the pledge effect is larger among individuals with lower economic and social security. Finally, the policy-induced alignment gain is transitory as it disappears once the pledge is fulfilled. Overall, our paper shows that electoral pledges related to social benefits are rather temporarily persuasive than permanently mobilizing.

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Introduction

Campaign promises are knowingly part and parcel of the political process. In democratic systems, candidates and platforms promise future social benefit expansions as a means to attract and influence voters, with the intent of tilting the electoral outcome in their favour. Although previous work has shown that incumbent parties largely fulfil their electoral pledges after elections (Thomson et al. 2017), we know relatively little whether voters care about campaign promises in the first place. In contrast to empirical studies in the policy feedback literature that assess the political effects of pre-election policies (Manacorda et al. 2011; Labonne 2013; De La O 2013, amongst others), the empirical evidence on the prevalence of pledge-based voting is relatively scarce.

With the present paper, we aim to overcome these empirical challenges by estimating the causal alignment effects of a campaign promise on a pension benefit expanding reform in the run-up to the 2013 federal election in Germany. Specifically, the proposal known as the *Mütterrente*, introduced by the conservative party, targeted a specific subset of voters based on a somewhat arbitrary eligibility criterion. We use this eligibility criterion to establish a causal link between this campaign promise and political behaviour among the benefiting group by employing a regression-discontinuity design (RDD). In detail, the *Mütterrente* reform sought to enhance pension benefits exclusively for individuals who had become parents to a child born before January 1, 1992 (Bach et al. 2014). As this singular criterion determined *automatic* enrolment into the program, we leverage the children's birthdates in a discontinuity-based design to isolate the effect of the promise on the alignment and turnout of mothers, mitigating potential confounding factors. Importantly, the salient and conspicuous nature of this criterion during the campaign period facilitated voters' ability to track and comprehend their eligibility status.

We present compelling evidence that the foundational assumptions underpinning discontinuity-based research designs are reasonably sound (Lee and Lemieux, 2010; Eggers et al. 2015; Skovron and Titunik 2015). A central challenge typically encountered when implementing eligibility-based designs is the risk of units self-selecting into treatment (for instance, by manipulating their eligibility status). However, by utilizing a child's birthdate – which was determined two decades before the reform's proposal even existed – we effectively mitigate this potential risk of sorting and manipulation.

In summary, this paper unveils three pivotal insights pertaining to pledge-based voting. Firstly, the electoral commitment associated with the *Mütterrente* leads to a substantial 12.2% increase in alignment with the party that made the promise, a result both of substantive importance and high statistical significance. Our analysis exposes that this alignment boost stems from a realignment of voters who traditionally adhere to left-wing platforms. In contrast, the study does not reveal a discernible effect on overall electoral participation, implying that the pledge's allure lies more in its power of persuasion than in its ability to actively mobilize voters, at least in the context of an advanced economy such as Germany. Importantly, these findings remain robust even when subjected to an array of sensitivity and validity tests, encompassing diverse considerations such as bandwidth size, higher polynomial structures, structural form, non-parametric estimation strategies, and placebo tests.

Secondly, our analysis shows that individuals with lower economic and social security exhibit notably greater responsiveness to the campaign promise, in stark contrast to their counterparts possessing higher levels of economic well-being. This observation reverberates with existing insights from the literature on political behaviour, which has long underscored the presence of an income gradient (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022). This

particular finding underscores the substantial electoral gains to be reaped from campaign promises revolving around the expansion of social benefits, particularly when directed towards voters grappling with lower economic security.

Lastly, while the initial baseline estimate underscores the role of pledges in enhancing alignment among eligible recipients with the pledge-making party, this effect experiences a temporary diminishment following the implementation of the reform. Put differently, our findings indicate that delivering on campaign promises related to welfare state expansions does not bind benefiting individuals to the pledge-making party in the long-run. Thus, the transient character of pledge-based policy feedbacks underscores that policymakers cannot ensure enduring political backing through isolated instances of benefit expansions. Rather, a continuous adoption of social benefit enhancements is requisite to maintain the ongoing support among the targeted electorate.

These findings carry profound implications for the study of political behaviour and preference formation in the electoral arena. Firstly, the study reveals that conservative parties can strategically pledge forthcoming expansions of social benefits to effectively challenge left-wing platforms. Secondly, the study illuminates the transient persuasive nature of electoral pledges, as opposed to their capacity for enduring mobilization. Lastly, the study accentuates the pivotal role of economic and social security as a safeguard against partisan endeavours seeking to exploit pocketbook-driven considerations among socioeconomically disadvantaged voters.

The remainder of the paper is structured as follows. In section II, we review the literature on the political benefits of social expansions and conceptualize pledge-based voting that enables

us to derive testable hypotheses. In section III, we discuss how we use the institutional design features of the *Mütterrente* reform to estimate the causal effect of campaign promises using individual survey data. In section IV and V, we discuss the empirical strategy and the results, respectively. Section VI concludes.

Pledge-based Voting

One fundamental premise in the policy feedback literature is to understand the reciprocal dynamics between policies and politics, elucidating how they mutually influence each other in the democratic process (Schattschneider 1935; Skocpol 1992; Pierson 1993). A prevalent approach of examining policy feedbacks involves evaluating the impact of implemented policies in the past on the subsequent electoral behaviour of individual voters during the ensuing election. Hereby, the majority of empirical analyses in the field of policy feedbacks predominantly concentrates on the effects of social policies (especially conditional cash transfer programs) on voters' political preferences and electoral choices in emerging and developing economies. Most of these studies find substantial and statistically significant effects.

For instance, Manacorda et al. (2011) adopt a discontinuity-based approach to demonstrate how an anti-poverty cash transfer program in Uruguay increases the likelihood of supporting the current government – as opposed to the previous one – by 11-13% among beneficiaries. Similarly, Labonne (2013) identifies an even larger effect through a randomized roll-out of a conditional cash transfer program in Philippine villages, indicating that recipient villages report 26% higher support for the incumbency compared to untreated ones. This high effectiveness of social transfer programs has also been observed in Mexico (De La O 2013; Cantú 2019),

Brazil (Hidalgo and Nichter 2015), Colombia (Baez et al. 2012; Gallego 2018), the Philippines (Khemani 2015), and Romania (Pop-Eleches and Pop-Elches 2012).

In contrast to retrospective voting that assess the impact of past policies, the exploration of pledge-based voting – a prospective form of performance evaluation – has been a rare object of analysis in the political economy literature (Cruz et al., 2018; Elinder et al., 2015), even though programmatic signals play arguably a key role in electoral politics. In general, pledge-based voting entails individuals aligning with a political platform based on a single electoral promise, which assures the voter of a material gain contingent upon the pledge-maker's electoral victory and subsequent policy enactment.

Pledge-based voting holds significant appeal for voters. On one hand, it offers distinct and precise insights into the intended policy direction, enabling voters to readily evaluate their potential material or ideological advantages within a complex policy landscape. On the other hand, pledge-based voting offers contingent material benefits tied to the election's outcome. In essence, self-interested voters possess a compelling motivation to endorse the pledge-maker, as the realization of the economic benefit depends on the promise-maker's attainment of the requisite power for promise fulfilment.

Hence, the phenomenon of pledge-based voting in the context of social benefit expansions can be conceptualized as a forward-looking, pocketbook-driven, and unidimensional manifestation of voter behaviour. Within this framework, voters assign considerable significance to a singular prospective programmatic signal, anticipating tangible advantages should the promise-maker attain authoritative power. Within this construct, a voter transitions from her conventional stance to the promise-making party if the anticipated material gain surpasses the ideological

costs associated with realignment. Stated differently, pledge-based voting operates on the premise that entrenched ideological affinities between voters and platforms can be alleviated through electoral promises, contingent upon the magnitude of the material incentive and the ideological disparity to the pledge-maker. This materialist interpretation of pledge-based voting within a spatial framework posits three hypotheses that we aim to examine empirically.

H1: Electoral pledges on social benefit expansions attract left-leaning voters.

Firstly, pledge-based voting related to social benefit expansions is expected to attract voters who traditionally align with left-wing platforms. This hypothesis is based on previous findings showing that left-leaning voters are more receptive to the promise of social benefit expansion, as it aligns more with their policy preferences for a larger welfare state (Campbell et al. 1960; Jacoby 1988; Alesina and Glaeser 2004; see Margalit 2013). Thus, re-aligning with a left-moving party decreases their ideological costs of switching, which makes them more likely to engage in pledge-based voting.

Concurrently, the impact on inactive voters is expected to be limited. On one hand, pledge-based voting lacks the ability to initiate resource-based mobilization at the time of the signal, in contrast to implemented policies that take effect prior to elections, as evidenced by previous research in the policy feedback literature (Clinton and Sances 2018; De La O 2013; Markovich and White 2022). On the other hand, the efficacy of programmatic signals is contingent on the accessibility of information (see Gingrich 2014). Given that electoral pledges may not reach individuals who are less politically informed or engaged, informational barriers likely restrict their mobilizing effect.

H2: Pledge-based voting on social benefit expansions is more prevalent among individuals with lower economic and social security.

Secondly, pledge-based voting is expected to be more prevalent among economically and socially vulnerable individuals which stand to gain more in relative terms from the fulfilment of the pledge compared to their more well-endowed peers. Here, one can argue that the marginal utility of the fulfilment of the pledge is higher for these individuals, which makes them more susceptible to aligning with the pledge-making party – at least in the short-run. This finding is consistent with the literature on the income gradient, showing that policy responsiveness tends to decline with higher economic security (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022).

H3: After pledge fulfilment, benefiting voters revert to their original party preference.

Firstly, since pledge-based voting is conditional on the presence of material incentives, one can expect that alignment with the pledge-maker only persists until pledge-fulfilment. Thus, pledge-based voting is expected to be a temporary phenomenon rather than a persistent one, especially when voters are aware of the transactional nature of the pledge which limits their gratitude towards the pledge-maker. Therefore, once the policy-vote exchange is completed with the fulfilment of the pledge, we expect that voters revert to their original party preference.

Despite the importance to understand the prevalence of pledge-based voting, empirical investigations into this phenomenon are challenging due to several endogeneity issues (Ferland and Dassonneville 2021). For instance, even though electoral pledges are more specific than most other programmatic signals, voters cannot always ascertain whether they can directly

benefit from the implementation of a given policy proposals. However, establishing clear and assessable eligibility criteria from programmatic signals is necessary for both voters and researchers to understand and measure the material consequences of a proposed social benefit expansion.

Furthermore, programmatic signals often encompass multiple policy proposals, each potentially impacting different groups of individuals. To isolate the effect of a single component within a broadly formulated programmatic position, researchers must meticulously control for all other issue dimensions. However, such conditioning strategies at a very granular level within party manifestos is often infeasible. Additionally, confronted with the presence of analogous policy proposals from competing parties, eligible voters may experience double treatment, whereby multiple parties offer comparable policy proposals. This phenomenon introduces bias into the estimation of treatment effects, potentially leading to the erroneous conclusion of the absence of pledge effects.

To address these empirical challenges and accurately discern the impact of electoral pledges on political behaviour of individual voters, the present paper adopts a quasi-experimental approach. By employing a regression discontinuity design in the context of the 2013 federal election in Germany, the study endeavours to illuminate the prevalence of pledge-based voting within the electoral arena, offering valuable insights into the dynamics of preference formation and the influence of single benefit-expanding campaign promises on electoral outcomes.

The *Mütterrente*

During the post-war era, the responsibility for parenting was predominantly perceived as a duty of women. Amongst other consequences, this cultural norm resulted in lower female participation in the workforce, leading to limited contributions to the pension system. In the mid-1980s, with the aim of bolstering women's social security and fostering their independence from their marital partners, the cabinet under Chancellor Kohl in West Germany introduced the transformative "Baby-Jahr" reform. At its core, this policy granted parenting individuals pension entitlements equivalent to one year of standard pension contributions per child (Rentenbescheid 2020), representing a pivotal milestone in acknowledging the value of parenting activities within the pension system.

Despite the "Baby-Jahr" reform's initial step towards recognizing the value of parenting, it proved insufficient to adequately insure parents after retirement. To further this agenda, the cabinet under Chancellor Kohl expanded the parenting-based allowance from one to three years per child with the introduction of the *Sechstes Sozialgesetzbuch* on January 1, 1992 (Rentenbescheid 2020). Although this provided a substantial increase in pension entitlements for parenting individuals, the reform suffered a significant shortcoming: it exclusively extended pension benefits to parents of children born on or after January 1, 1992, leaving parents with children born on December 31, 1991, and earlier disadvantaged. This seemingly arbitrary cut-off date led to a pronounced inequity: parents below the threshold received pension entitlement equivalent to only one year of contributions per child, while those above it enjoyed entitlements equivalent to three years.

Although the distributive consequences of this policy were well known already at the time, it wasn't until the end of 2011 that the issue gained broader political attention. Amid a growing

demand for gender equality policies, the conservative party propelled the matter to the forefront of public and political debates. The conservative party's female-only working group, known as *Frauenunion*, played a leading role, advocating for rectifying the previous inequity which disadvantaged parents of children born before January 1, 1992. Their initial proposal, which became known as the *Mütterrente* (in English: *Mother's Pension*), was presented on November 14, 2011, at the annual convention of the CDU Germany in Leipzig, offering additional pension entitlements for each child born prior to January 1, 1992 (Roßmann 2012). This reform proposal garnered favour within the wider conservative party, not solely due to its emphasis on traditional family values but also because it resonated with a crucial electoral group: mid-age women in their 40s and 50s, who have been instrumental voters contributing to the conservative party's electoral success for decades.

Over the subsequent two years, the proposal gained momentum in the broader public and it became one of the most prominent pledges of the conservative party leading up to the 2013 federal election (Bäcker 2018). Importantly, the *Mütterrente* reform was solely championed by the conservative party, while other platforms criticized it during the 2013 election campaign (Focus Online 2013). Following a successful election, Chancellor Merkel's new cabinet facilitated the reform's passage in parliament on May 23, 2014, with implementation on July 1, 2014 (Bundestag 2014). Table 1 provides an overview of key events surrounding the reform.

The reform's macro- and microeconomic impacts were significant. On an aggregated level, the *Mütterrente* emerged as the most extensive and cost-intensive German pension reform in recent decades, benefiting over 9.53 million individuals (Keck et al. 2015). At the individual level, eligible parents experienced a marked augmentation in their pension benefits. Notably, parenting mothers with one child born before 1992 witnessed a substantive increment of 90.05

Euro per month before taxes, as of July 2022. By comparison, women, regardless of parental status, receive an average of 1,001 Euros per month after health and nursing insurance contributions, as of July 2021 (Blumenroth 2022).

Table 1: Timeline of Mütterrente-Reform

Period	Event	Regulation: pension entitlements for parents per child
Jan. 1986	"Baby-Jahr"-Reform	- for every child born: parenting individuals receive 1 years of pension contributions
Jan. 1992	Introduction of <i>Sechstes Sozialgesetzbuch</i>	
Nov. 2011	At the annual convention of the CDU: Frauen-Union proposes new pension reform called <i>Mütterrente</i>	- for every child born after 1992: parenting individuals receive 3 years of pension contributions
Jan.-Sept. 2013	<i>Mütterrente</i> is a key campaign promise of conservative party (CDU/CSU) during campaign period	- for every child born before 1992: parenting individuals receive 1 years of pension contributions
Sept. 2013	CDU/CSU under Chancellor Merkel win the election and enter coalition with social democratic party (SPD)	
May 2014	<i>Mütterrente</i> reform passes parliament	- for every child born after 1992: parenting individuals receive 3 years of pension contributions
Jul. 2014	<i>Mütterrente</i> reform comes into force	- for every child born before 1992: parenting individuals receive 1 years of pension contributions + additional payment (<i>Mütterrente</i>)
until Jul. 2015	All affected parents are informed how the <i>Mütterrente</i> affects their pension entitlements via " <i>Rentenbescheid</i> " (annual information documents)	

Empirical Strategy

Treatment Assignment and Data

The *Mütterrente* is an appealing case to measure the effect of electoral pledges related to social benefits on political behaviour. On the one hand, the reform was very salient and visible to voters during the campaign/promise period. This is important as the effect of individual preferences on voting behaviour depends strongly on the visibility and informational structure of social policies (Gingrich 2014). On the other hand, the reform's design of using a child's date of birth – which is the only eligibility rule to be *automatically* enrolled into the program – creates a balanced treatment and control group that can be used in a regression-discontinuity design (RDD).

Leveraging eligibility-based discontinuities stands as a prevailing approach to derive estimates of causal effects. Hereby, it has gained especially popularity as a form of identification in labour economics, where age-based eligibility criteria are frequently employed (Card et al. 2007; Card et al. 2009). Nonetheless, the utilization of children's birth dates as a means of identification has been relatively rare, primarily owing to data privacy concerns. However, there exist two noteworthy exceptions in the economic literature. One, Carneiro et al. (2015) employ children's birth dates as a discontinuity to estimate the causal effect of maternal leave benefits on long-term child outcomes. Two, Olafsson and Steingrimsdottir (2020) utilize children's birth dates as a discontinuity to assess the impact of paternity leave on marital stability.

Likewise, we draw upon the comprehensive *Socio-Economic Panel* (2020) (SOEP-Core, v36) dataset, compiled by the *German Institute for Economic Research* (DIW Berlin), which captures the month and year of a child's birth under the female household member. Boasting a

substantial sample size of approximately 30,000 respondents from around 15,000 households, the SOEP stands as one of the most extensive public opinion survey datasets in Germany. Distinguishing itself from many other surveys, the SOEP follows individual respondents longitudinally since 1984, gathering data on an annual basis.

To conduct our analysis, we filter the full survey dataset in three ways. First, for the initial analysis, we focus on responses from the survey waves of 2012, 2013, and 2014 (from January 1, 2012, to December 31, 2014). We chose this timeframe because voters must be aware of a policy promise for it to influence their decisions. We start the analysis from the date when the reform proposal became a topic of public debate, which was primarily after the conservative party's annual meeting at the end of 2011. We end the analysis with the year of implementation which was 2014 (Bundestag, 2014). To ensure the reliability of our findings, we also conduct robustness checks and placebo tests by shifting the start and end dates forwards and backwards. This helps us examine how the effect of the promise changes over time. Additionally, to address any potential biases, we narrow down our analysis to individuals who responded in at least two out of the three survey waves. By doing so, we aim to maintain a balanced sample over time and minimize any significant changes due to non-responses or dropouts (attrition bias).

Second, we restrict the sample to female respondents only. This is due to two reasons. On the one hand, despite men being technically eligible for the *Mütterrente*, the reform is explicitly targeted towards females, as indicated by its name (*Mother's Pension*), and this was widely acknowledged by the general public when the proposal was put forth and later adopted. Among the 9.53 million individuals eligible for the *Mütterrente* (Keck et al., 2015), only 1.4 percent are male (Stiftung Warentest 2021). On the other hand, the survey dataset lacks the capability to identify fathers since it links only female respondents to their children's records. While this

data limitation could have been concerning if men were more likely to benefit from the policy, it is unlikely to have a significant impact on our analysis as women are the primary focus group for this study.

Third, we refine the dataset by excluding all respondents with children born before 1972 and after 2012. This deliberate selection results in a dataset spanning a 40-year period, encompassing 20 years before and after the designated cut-off date. The reason for this exclusion is twofold. On the one hand, the number of mothers with children born before 1972 is notably small for each month and year, leading to relatively large variance prior to 1992. On the other hand, after 2012, the change in party alignment of mothers may have been significantly influenced by the introduction of the *Kinderförderungsgesetz* in 2008. This law grants parents the legal right to a place at a day-care centre for 1-3-year-old children starting from August 1, 2013 (Bundesministerium für Familie, Senioren, Frauen und Jugend 2018). The implementation of the *Kinderförderungsgesetz* represented a pivotal milestone in family policymaking during the Merkel era and could have led to substantial shifts in political support in favour of the conservative party. To mitigate the potential confounding impact of the 2008 reform, we exclude all mothers who did not give birth to their first child prior to 2013.

Empirical Analysis

The RDD's running variable is derived from the date of birth of the respondent's first child. Leveraging the available SOEP data, which provides precise details about the month and year of the child's birth, we convert this information into a numeric variable that quantifies the number of months prior to or after the designated cut-off date (expressed as year-month). Thus, if a respondent's first child is born in January 1992, the running variable assumes a value of 0, indicating the proximity to the cut-off date. Conversely, if the first child is born in December

1992, the running variable takes on a value of 11, signifying the months elapsed since the cut-off date. In contrast, when the first child is born in January 1991, the running variable assumes a value of -12, representing the months leading up to the cut-off date in the preceding year.

The first child's date of birth is utilized for treatment assignment as it ensures the most straightforward and unambiguous identification strategy, enabling a comparison between eligible and non-eligible mothers. While it is technically feasible to utilize the date of birth of subsequent children, this would disturb treatment assignment as the control group then includes mothers which are eligible for the benefit expansion when their first (or second, etc.) child was born prior to January 1992. Thus, to avoid these empirical pitfalls, we primarily focus on the differences between eligible and non-eligible individuals only. However, by testing for heterogenous treatment effects, we measure how the impact of the pledge varies by family size.

The primary dependent variable in our study is a measure of party alignment, derived from the multi-answer question, "Which party do you feel closest to?" From this survey question, we construct a binary variable that takes a value of 1 if a respondent aligns with the conservative party (CDU/CSU), and 0 otherwise. Although alignment measures do not equate vote choice, we use this common proxy for political preferences as it enables us to test how the effect size has evolved over time. In addition to examining party alignment, we also investigate whether the proposal had an impact on electoral participation. To assess this, we create a binary variable that takes a value of 1 if a respondent declared having voted in the 2013 federal election, and 0 otherwise.

Furthermore, we include several control variables that may influence individuals' political alignment. These variables encompass (i) age, (ii) a binary indicator for marital status, (iii) the

number of children, (iv) a binary indicator for educational attainment, (v) years of full/part-time employment experience (to capture the size of future pension entitlements), (vi) net household income, and (vii) satisfaction with household income (Weisberg 1987; Hellwig 2008; Lago and Lago 2021). While there may be other relevant control variables available, we opt not to include them due to limitations in data coverage². Summary statistics are provided in Table 2.

Table 2: Variables and summary statistics

Statistic	N	Min	Max	Mean	SD
Alignment with conservative party	3,254	0	1	0.373	0.484
Age	3,254	22	79	48.250	9.925
Binary for being married	3,254	0	1	0.757	0.429
Number of children	3,254	1	3	1.880	0.700
Binary for no degree	3,254	0	1	0.008	0.087
(log) net household income (monthly)	3,254	5.832	10.519	8.143	0.540
Years of work experience	3,254	0.000	49	19.725	10.616
Satisfaction with household income	3,254	0	10	7.047	2.141
Alignment with social democrats (SPD)	3,254	0	1	0.318	0.466
Alignment with Green party (Die Grünen)	3,254	0	1	0.240	0.427
Alignment with Liberal party (FDP)	3,254	0	1	0.017	0.128
Alignment with The Left (Die Linke)	3,254	0	1	0.048	0.214
Alignment with far-right party (AFD)	3,254	0	1	0.002	0.043
Net personal income (monthly)	2,350	0.000	11,025	1,510	1,089
Voted in federal election in 2013	4,977	0	1	0.789	0.408

The baseline specification takes the following form:

$$Y_i = \alpha + \lambda D_i + \beta_1(X_i - c) + \beta_2 D_i(X_i - c) + \delta_K K_i + \epsilon_i$$

with Y as the binary for (a) alignment with the conservative party, or (b) turnout for survey response i ; D as the treatment variable; X as the recoded first child's date-of-birth variable (running variable) represented as the number of month before/after the cut-off date; c as the

² For example, occupation, union membership, religiosity, self-placement on left-right scale, degree of political interest, and satisfaction with social security system

cut-off date value (which is equal to 0 for children born in January 1992); K as a vector of control variables with their respective coefficients (δ_K); and ϵ as the error term. The local average treatment effect (LATE) – the parameter of interest – is λ .

An essential empirical decision in the RDD approach revolves around choosing the functional form of the model. One crucial aspect pertains to determine the number of polynomial terms of the forcing variable that should be included as regressors on the right-hand side. While higher-order terms offer a better fit to the data, they also run the risk of overfitting. To strike a balance between variance and bias, it is common to select the appropriate structural form based on their respective information criteria.

To tackle this, we calculate the Bayesian Information Criteria (BIC) for both linear and quadratic polynomial models. The BIC serves as a criterion to compare the goodness of fit of different models, accounting for model complexity. After computation, we find that the linear version yields a lower BIC compared to the quadratic version. Consequently, we adopt the simpler linear model as our baseline approach. Nonetheless, we provide evidence demonstrating the robustness of our findings to the inclusion of the quadratic term of the forcing variable. We opted for the BIC over the AIC because, in explanatory exercises like ours, the BIC is considered superior, while the reverse is true for predictive tasks (Sober 2002; Shmueli 2010).

As part of our baseline approach, we incorporate all the mentioned control variables, along with the first polynomial term of the running variable. The baseline employs a parametric estimation based on a linear probability model, and it allows for different slopes on each side of the cut-off. For bandwidth selection, we utilize the method proposed by Imbens and

Kalyanaraman (2012), which enables us to determine the optimal window size around the discontinuity in a data-driven manner.

In addition to the baseline, we conduct a comprehensive set of robustness checks to estimate the treatment effect for both alignment and turnout. These checks entail (i) varying the set of control variables, (ii) experimenting with different bandwidth sizes, ranging from 5 to 20 years, (iii) fitting data via a logistic regression approach (instead of a linear probability model), (iv) applying a slope-equality restriction, (v) incorporating the second polynomial term of the running variable as additional covariate, and (vi) employing a non-parametric estimation approach (see Stigler and Quast 2016). By undertaking these tests, we aim to ensure the stability and validity of our findings.

Identifying Assumptions

The validity of regression discontinuity designs critically hinges on the continuity assumption, which cannot be statistically proven. However, in RDD applications, it is common to assess this assumption by examining continuity-in-observables and the presence/absence of sorting/manipulation around the cut-off (Lee and Lemieux 2010; Eggers et al. 2015; Skovron and Titiunik 2015).

First, to investigate sorting, we employ the McCrary density test (2008) to visually and empirically inspect whether there are significant changes in density on either side of the cut-off date. As depicted in Figure A.3, no substantial density shifts are evident, and the high p-value (0.519) supports the likelihood that sorting and/or manipulation did not take place. Notably, in the context of the *Mütterrente*, the prospect of deliberate bunching or manipulation is unlikely, if not implausible, due to the cut-off criteria being determined two decades prior to

the reform's implementation. Parents could not have foreseen that their child's birth date would bear such substantive material consequences in the distant future. Moreover, before the introduction of the *Sechstes Sozialgesetzbuch*, any incentives from the benefit system to have children would have favoured later births, after December 31, 1991, as such births would have qualified parents for additional pension entitlements. Therefore, there is a compelling case against any potential sorting into the treatment group.

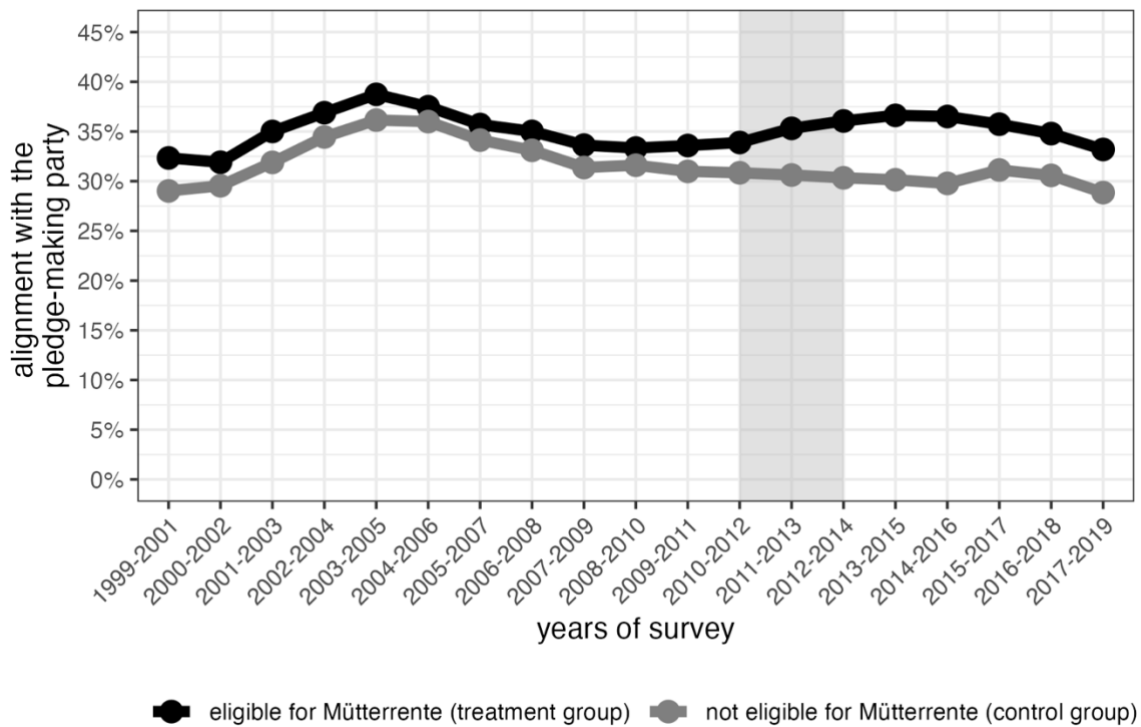
Secondly, to examine continuity-in-observables, we undertake both visual inspections and hypothesis-based tests. As depicted in Figure A.2, none of the control variables included in the baseline regression exhibit a significant discontinuity around the cut-off. Moreover, the absence of observable discontinuity is reinforced by empirical estimates presented in Table A.1, where the treatment variable shows insignificance across all considered control variables. It is pertinent to note that the treatment effect is identified for respondents who were approximately 50 years old in 2013 (and correspondingly 29 in 1992) when their first child was born. These ages align with the average age of mothers in Germany giving birth to their first child (Statistisches Bundesamt 2022).

Although both the visual and empirical tests lend support to the validity of the continuity assumption by investigating observable characteristics, they are limited in examining unobservable ones. While the timing of the reform makes it unlikely to create imbalances between the treatment and control groups, as mentioned earlier, it is conceivable that the first benefit expansion, the introduction of the *Sechstes Sozialgesetzbuch* in 1992, may have influenced incentives for giving birth after 31 December 1991. Consequently, differences in outcomes could arise due to disparities in unobservable characteristics between the treatment and control groups stemming from the earlier reform. To address this concern and ensure that

changes in political alignment (and turnout) are attributed to the *Mütterrente* rather than potential pre-existing compositional differences originating from the earlier reform, we analyse previous survey waves while applying the same sample restrictions discussed above.

As depicted in Figure 1, the disparities in outcomes between the treatment and control groups were consistently small and remained stable over time before the policy proposal for the *Mütterrente* was put forth at the end of 2011. Subsequently, these differences experienced a significant increase which were driven by higher alignment levels among those benefiting from the reform. While this descriptive evidence does not enable a causal interpretation of the reform's effect at this stage, it aims to support the conclusion that the treatment and control groups exhibited similar levels of alignment with the conservative party before the policy proposal was made. Therefore, it appears unlikely that the first reform has led to substantial differences in outcomes that would undermine the continuity assumption regarding unobservable factors in our discontinuity-based research design.

Figure 1: Alignment with the pledge-making party of eligible and non-eligible mothers over time



Note: The figure plots the 3-year-wave averages of alignment with the pledge-making party (conservative party) over time for both treatment (blue) and control group (red). The treatment group consists of mothers who gave birth to their first child between 01 January 1985 and 31 December 1991 (eligible for Mütterrente); the control group consists of mothers who gave birth to their first child between 01 January 1992 and 31 December 1999 (not eligible for Mütterrente). The grey area represents (approx.) the period when the pledge was made.

Results

Baseline

The main finding of our analysis provides evidence that campaign promises of future social benefit expansions have a large and statistically significant effect on political alignment of individual voters. Specifically, the results show that the promise of the *Mütterrente* increased alignment with the conservative party by an impressive 12.2% (Figure 2, Figure A.4, Table B.1). This substantial effect suggests that the campaign pledge of the *Mütterrente* significantly influenced the political and party preferences of eligible individuals, drawing them closer to the conservative party.

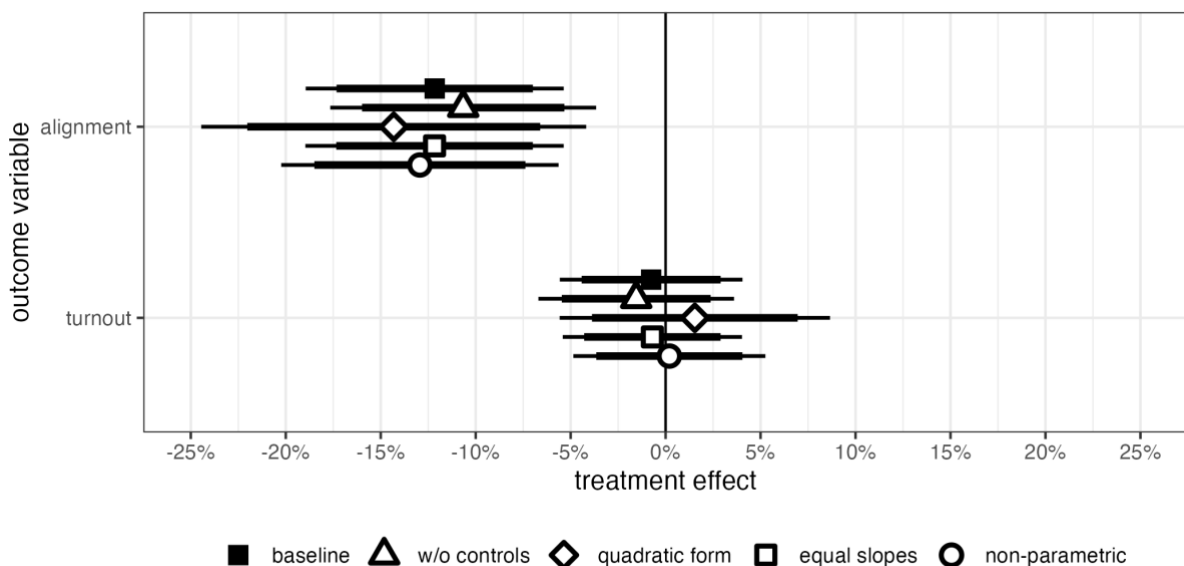
Conversely, when examining the effect on voter turnout, we found no significant impact of the campaign promise (Table C.1). In other words, despite the profound effect on alignment, the promise did not lead to notable changes in electoral participation among benefit-eligible voters. This result suggests that while the electoral pledge was effective in shaping the ideological proximity between voters and platforms, it did not mobilize inactive voters. Overall, these baseline findings confirm our key hypothesis.

To ensure the reliability and robustness of our main findings, we conducted an array of tests to explore the effect of the campaign promise on both political alignment and voter turnout. Regarding the alignment effect, our robustness checks consistently yielded similar and highly statistically significant effect sizes across diverse model specifications. In particular, we explored the inclusion of a quadratic term of the running variable as additional covariate, the employment of a logistic regression setup, and the imposition of slope-equality restrictions, all of which resulted in consistent and significant alignment effect sizes (Table B.2).

Additionally, altering the control variable set and varying the bandwidth size produced no meaningful changes in the size of the coefficient and its standard error (Figure B.3, Table B.4, Table B.5). Notably, when employing a non-parametric estimation approach, specifically the non-parametric local linear regression estimator, the alignment effect remained significant and consistent (Table B.2). In contrast, when we apply these same robustness checks to turnout, we did not find any substantively meaningful and/or statistically significant treatment effects, regardless of the model specifications applied (Table C.2, Figure C.3, Table C.4, Table C.5).

In summary, our baseline analysis and the robustness checks reinforce the validity of our main findings: while the campaign promise on future social benefits did not lead to an increase in voter turnout, our investigation consistently revealed that it significantly shaped the party preferences of individual voters in terms of political alignment. These findings contribute to our understanding of the impact of campaign promises on political behaviour and underscore the distinct effects they can have on different aspects of voter engagement and party affiliation.

Figure 2: Treatment effect (ATT) of the benefit promise on alignment with the pledge-making party



Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The baseline is based on a parametric linear probability model without slope-equality restrictions (element with black filling). The remaining coefficients (robustness checks) vary one of the baseline specifications at a time (elements with white filling). Confidence intervals are based on robust standard errors. The thick and thin lines represent 95% and 99% confidence levels, respectively.

The observed alignment effect of the campaign promise on political behaviour raises an important question: if the effect is not stemming from mobilizing politically inactive individuals, where does it then originate? Upon closer examination, it becomes evident that the effect is predominantly driven by the re-alignment of individuals who traditionally support competitor platforms. Ex-ante, one might expect that the promise effect would appeal to voters

of pro-welfare platforms, as conservative parties expanding social benefits tend to shift more to the ideological terra of left-wing parties.

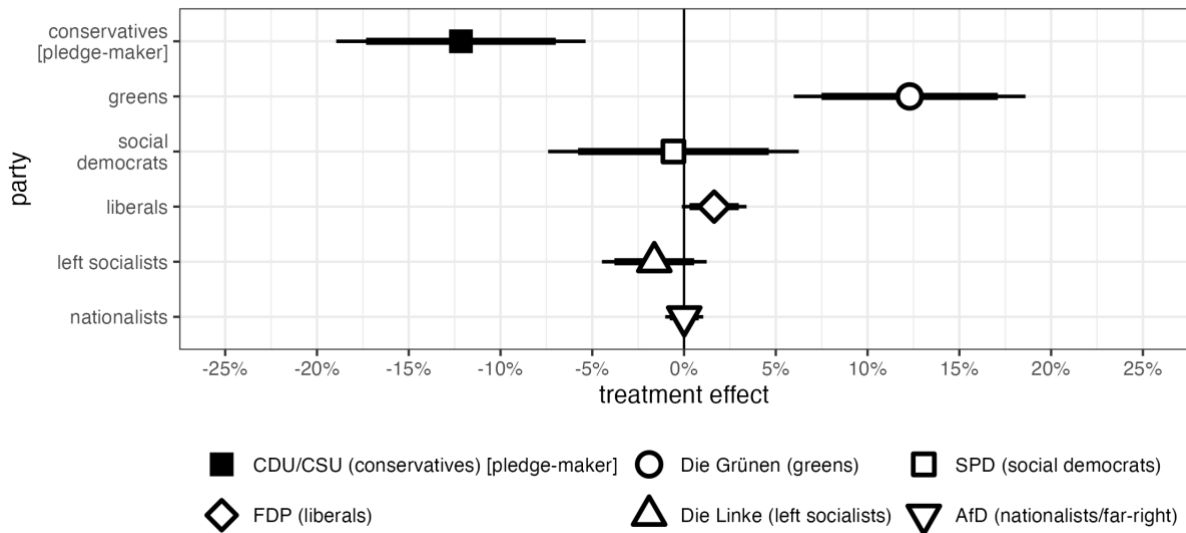
Aligned with this rationale, our findings offer evidence that the effect primarily emanates from individuals who previously aligned with the green party, a well-known left-wing platform (Figure 3, Table B.6). Notably, the decline in alignment among beneficiaries with the green party corresponds symmetrically to the alignment gains observed among recipients with the pledge-making party, underscoring that the promise predominantly attracted voters from this political competitor. Conversely, although we observe a small yet statistically significant decrease in alignment with the liberal party (FDP), no meaningful changes in alignment levels are evident among voters supporting other platforms, including the social democrats (SPD), the left socialists (Die Linke), or the far-right (AfD).

Overall, these partisan findings offer valuable insights into the dynamics of political alignment in response to campaign promises on social benefits, entailing two key implications. Firstly, conservative parties have the potential to pose a significant electoral threat to left-wing parties by strategically promoting social benefit expansions, even in the form of prospective programmatic signal communicated through electoral pledges. Our results demonstrate that such promises can effectively allure left-leaning voters, indicating that the appeal of social benefits can transcend traditional ideological boundaries.

Secondly, our findings shed light on the crucial role of pocketbook motives in shaping political preferences, even among individuals who traditionally lean towards left-wing platforms (Healy et al. 2017). This finding contributes to the ongoing debate in the literature on economic voting (see Anderson 2013), which has largely focused on retrospective considerations. By revealing

the prevalence of prospective egotropic voting behaviour – where individuals respond to future policy proposals that directly impact their own economic interests – we enrich our understanding of the factors influencing voters' decision-making processes.

Figure 3: Treatment effect (ATT) of the benefit promise on alignment with different parties



Note: The outcome variables (y-axis) are binary variables indicating alignment with different parties (coefficient with black filling refers to the pledge-making party). All estimations are based on the baseline specification, and they include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income. The thick and thin lines represent 95% and 99% confidence levels, respectively.

Heterogeneity

To understand the mechanisms how campaign promises affect alignment and turnout more closely, we conduct several tests to examine heterogenous treatment effects. Hereby, we first look at the potential mediating role of age. In the context of the reform under consideration, it is important to note that the policy impacts pension entitlements which only materialize at retirement. However, most of the treated individuals have not yet retired. This temporal aspect could potentially influence the pledge effect, assuming that mothers closer to retirement place more emphasis on programmatic signals related to pension policies during the preference

formation process than their younger peers. To examine the role of age, we narrow the sample to mothers aged above 47 (average age of the full sample of treated and control units) and estimate the treatment effect with the baseline specifications.

Although we find a slightly larger effect among older individuals, indicating that temporal distance to retirement may matter, this estimate is not statistically different from the baseline coefficient (Table 3, Table B.7, Table B.8). As such, we cannot reject the null hypothesis of estimator equality. Nonetheless, it is essential to note that studying the mediating effect of age poses an empirical challenge, given that our discontinuity-based research designs identify the treatment effect for individuals with similar ages, otherwise, the continuity assumption would be violated. Consequently, since the running variable is arguably correlated with age, subsampling along this dimension may change the compositional characteristics of treatment and control group close to the cut-off. Thus, the estimation of the pledge effect's gradient along age should be considered with caution.

Beyond age, research in the field of policy feedbacks, particularly in the context of vote buying, has found that cash transfers are more electorally beneficial when targeted at individuals with lower income (Brusco et al., 2004). However, it remains an open question whether this pattern also applies to electoral pledges concerning future social benefits. When viewing electoral politics as a form of vote-policy exchange, one can assume that the elasticity of price (here: the probability of re-alignment), is high, simply based on the idea that individuals who stand to gain more from the policy proposal in relative terms are more willing to shift their party preferences due to the campaign promise, compared to those for whom the policy proposal has only a marginal economic and/or social impact. Thus, we expect that the campaign promise

will be more effective among individuals with lower economic and social security, relative to their more secure counterparts.

To test whether economic security is a moderating factor of the pledge effect, we use different correlates of individuals' economic and social position to divide the full sample into different sub-samples. First, we compare the estimate of the treatment effect between the baseline³ and low-income individuals. Hereby, we define low-income individuals as respondents who earn 850 Euro or less per month. Employees with ≤ 850 Euro per month are on so-called "Mini-/Midi-Job" contracts which provide no or only limited compulsory social insurance contributions towards, for example, health or unemployment insurance. While these types of jobs contribute to the pension systems, they provide only narrow social insurance at retirement age, especially due to their low contribution rates.

The results show that low-income individuals are generally more sensitive to the reform promise than what the baseline estimate would imply. To be more specific, the promise effect among individuals with an income equal to/below 850 Euro per month reaches 27.8% (Table 3). This estimate is statistically significantly larger in absolute terms (p-value: 0.035) than the corresponding treatment coefficient of 10.4% among the whole sample (Table B.1). Thus, these results suggest that individuals on low-income contracts indicate larger promise-induced alignment shifts.

Second, the family/household structure may also have a large effect on individuals' economic position – both in terms of wealth and income. One aspect concerns the number of children. Hereby, mothers with more children may benefit more from parenting-based pension

³ without control variables for comparability purposes

entitlements because they have, on average, less years of employment-based pension contributions due to upbringing time. To investigate the number of children as mediating factor, we split the baseline dataset into two sample: mothers with one child and mothers with two or more children.

The estimates show that the promise effect is statistically significant, and slightly larger among respondents with two or more children compared to the baseline treatment effect (Table 3). In contrast, the estimate among mothers with one child is smaller and statistically not significant. This may suggest that the size of the promise effect indeed increases with the number of children. However, since we cannot reject the null hypothesis that the coefficient based on the more-children sample statistically significantly different from the baseline coefficient (p -values > 0.05), we cannot provide a final answer about the mediating role of family size. Nevertheless, the large difference in treatment effect size between mothers of one child and more children provides a strong signal in favour of heterogeneous treatment effects.

Third, marital status may also play a mediating role as it affects various aspects related to cost-sharing, production, and labour division within households (Häusermann et al. 2016). When we split the sample into married and non-married mothers, we find that the effect is substantially larger for non-married mothers than the baseline estimate (Table 3). In contrast, the coefficient for married mothers is smaller than the baseline treatment effect. Even though we cannot confirm that either of these sub-sample estimates is statistically different from the full sample estimator, the large difference in effect size between 20.5% among unmarried women and 7.5% among married ones suggests that marital status may also play an important mediating role for the cause-effect relationship between campaign promise and party alignment.

Table 3: Heterogeneity of the treatment effect (ATT) of the benefit promise on alignment with the pledge-making party

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.119** (0.040)	-0.028 (0.047)	-0.138*** (0.033)	-0.075* (0.032)	-0.205*** (0.047)	-0.278*** (0.061)
intercept	0.359*** (0.024)	0.282*** (0.034)	0.403*** (0.024)	0.388*** (0.023)	0.316*** (0.037)	0.525*** (0.046)
N	1473	742	1709	1854	597	560
adj. R2	0.01	0.01	0.00	0.00	0.01	0.02
BIC	2002	1039	2335	2624	687	797
Log.Lik.	-983	-503	-1149	-1293	-328	-383
p-value	0.8079	0.3169	0.5455	0.5973	0.1764	0.0335
sub-sampling variable	age	# of children	# of children	marital status	marital status	income
sub-sampling rule	older than 47	less than 2 children	more than 1 child	married	not married	less than 850€/month

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. The columns differ with respect to the sample their respective coefficients are estimated on, indicated by the sub-sampling variable and sub-sampling rule. The specification of estimation follows the baseline structure, expect the exclusion of all control variables (to ensure comparability across different sub-samples). For each estimation, a t-test is conducted to test whether the treatment effect of the sub-sample is statistically significantly different to the baseline ATT (see Table B.1 column 1). The p-value indicates whether the null hypothesis of equality between the baseline and sub-sampling estimate can be rejected. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

To provide further evidence for these results, we conducted a comprehensive set of robustness checks to validate our main findings concerning the variation in effect size, and the results consistently support the presence of effect heterogeneity with respect to economic security (Table B.7, Table B.8). Furthermore, we also explore the variation in effect size along these mediating factors with respect to voter turnout, aiming to ascertain whether the promise effects may have impacted political participation of specific demographic and socioeconomic groups differently (Table C.6, Table C.7). However, our analysis reveals no statistically significant estimates of the treatment effect, except for low-income individuals for whom we find a mobilizing pledge effect. However, we cannot confirm with sufficient statistical confidence that this estimate is different from the baseline coefficient.

In summary, our findings regarding the heterogeneous treatment effects underscore the significance of economic security in shaping the effectiveness of campaign promises in swaying voters from competitor platforms to the pledge-making one. The results suggest that economically and socially more vulnerable individuals display greater responsiveness to electoral pledges of future benefit expansions, making them potentially more susceptible targets in the realm of electoral politics and campaigning. These insights shed light on how economic factors can play a pivotal role in influencing voter responses to campaign promises.

Durability

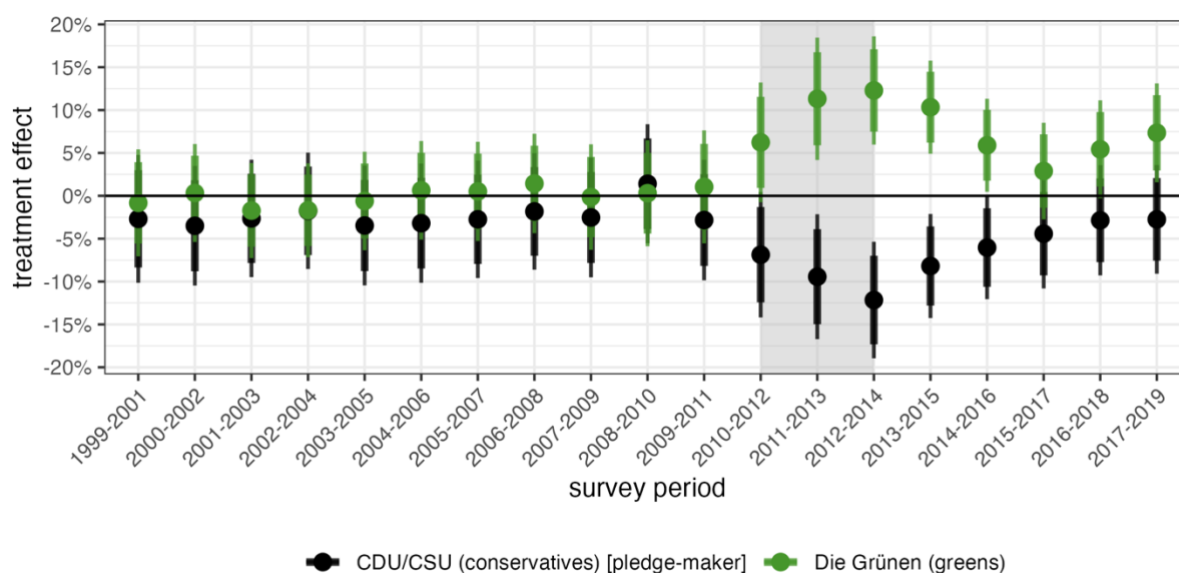
An essential inquiry in the realm of political behaviour centres around understanding the duration of policy effects. Several scholarly contributions in the policy feedback literature have investigated the enduring influence of targeted transfer programs which resulted in diverging findings, depending on the national and policy context. On the one hand, Bechtel and Hainmüller (2011) observed that disaster benefits in Germany led to a sustained increase in the vote share among recipients for the party that implemented them, a trend that persisted for two subsequent elections before diminishing. Similarly, Manacorda et al. (2011) demonstrated that cash transfers to the poor in Uruguay had a lasting impact on political support for the government among the affected individuals, even after the program's conclusion. On the other hand, Zimmermann (2021) presented contrasting results, indicating that the expiration of an anti-poverty program in India led to a substantial decline in governmental support among eligible individuals. Likewise, in Brazil and China, Zucco (2013) and Lü (2014) respectively found that conditional cash transfers and social policies did not secure long-term political support.

However, the longevity of policies that have been implemented following electoral promises is expected to differ compared to policies that are not directly tied to electoral processes. This distinction arises from the fact that once a policy is implemented, and the pledge is fulfilled, the initial economic incentive that initially motivated voters to align with the pledge-maker may lose its potency. As a consequence, voters who were initially influenced by the promise might gradually revert to their pre-existing political preferences. Essentially, the fulfilment of the promise marks the culmination of the policy-vote exchange, and in the absence of ongoing incentives, the effect of the promise on political alignment is likely to fade over time. Consequently, investigating the durability of pledge-based policy effects offers valuable insights into the complex relationship between voters and political platforms, shedding light on whether alignment motives, such as voters' gratitude and resource-based rationales, extend to policies that were previously pledged for electoral purposes.

To assess the lasting impact of pledge-based policies in the context of expanding social benefit entitlements, we employ the same sample restrictions and baseline specification as in the earlier analysis described above in order to keep sampling uncertainty at comparable levels. The only variation is that we shift the survey time both forward and backward to examine whether conservative alignment between the treatment and control groups varies over time. Specifically, the survey period encompasses all responses recorded between 01 January in year k and 31 December in year $k+2$, where k ranges from 1999 to 2017. During the pre-treatment period, we expect to obtain insignificant estimates, as the promise cannot have any effects before it has been made (pre-treatment placebo test). During the post-treatment period, we expect that the alignment difference between the treatment and control group gradually diminishes, reflecting the completion of the policy-vote exchange once the electoral pledge is fulfilled.

In addition to successfully passing the pre-treatment placebo tests, our analysis provides two key findings about the temporal persistence of the treatment effect. First, the post-promise estimates reveal that the policy proposal had its most substantial impact on attracting political support from benefiting voters between 2012 and 2014, which encompasses (approx.) the campaign and immediate post-policy period (Figure 4, Figure D.1, Table D.4, Table D.5, Table D.6). This indicates that the electoral pledge had a significant effect in influencing alignment during this critical time of the reform when it was part of the public discourse.

Figure 4: Treatment effect (ATT) of the benefit promise on alignment over time



Note: The outcome variables are binary variables indicating alignment with the pledge-making (conservative party, coloured in black) and one non-pledge-making (the greens, coloured in green) party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Each estimate is based on the baseline specification and uses a 3-year survey period (x-axis). The grey area represents (approx.) the period when the pledge was made. Confidence intervals are based on robust standard errors. The thick and thin lines represent 95% and 99% confidence levels, respectively.

Second, after the implementation of the reform in mid-2014, we observe a gradual reduction in the alignment with the conservative party over time, leading to insignificant differences between treatment and control group before the first post-reform federal election in 2017. To

test whether these voters reverted to their previous electoral platform, we assess the alignment trends with the green party which experienced the most significant political costs due to the reform (proposal) (Figure 3). As the results illustrates, the losses in alignment of the green party were gradually compensated after pledge fulfilment, suggesting that traditionally green-aligned recipients indeed returned to their previous ideological home (Figure 4, Figure D.2, Table D.7, Table D.8, Table D.9; see also Figure D.3).

The implications of these findings align with our initial expectations, indicating that pledge-based policies are unable to maintain electoral effects in the long run, even when the pledge-making party delivers on its promise. This finding highlights that policy proposals crafted for electoral gain follow a different logic than the traditional rationales that are commonly used in the policy feedback literature, such as resource-based considerations or temporary/enduring gratitude of voters, which are more in line with effect persistency.

These findings underscore the significance of considering pocketbook motives in shaping political preferences, particularly in the arena of electoral politics. The observed pattern, wherein alignment with the pledge-maker increases steadily before policy adoption and gradually declines thereafter, suggests that a substantial portion of voters can be incentivized to temporarily traverse ideological boundaries until they receive their desired ideological compensation in the form of personal economic benefits. Interestingly, the results demonstrate that right-wing parties can entice even left-leaning voters through campaign and policy strategies that capitalize on pocketbook considerations.

Pocketbook-driven re-alignment through single policies is, however, not sustainable in the long-run. We interpret this finding by noting that voters are cognizant of the transactional

aspect of pledge-making, wherein the campaign promise itself acts as an offer and the implementation as the fulfilment of the exchange. As a result, voters gradually return to their previously held ideological platforms, especially when there is a limited risk of policy repeal by the same party, and enduring alignment with the pledge-maker is not deemed necessary to preserve the benefits of the adopted policy. Therefore, our analysis seems to suggest that activating pocketbook motives in the electoral arena only temporarily steers voters away from their political position.

Conclusion

In conclusion, this paper has aimed to improve our understanding of pledge-based voting within democratic systems. By employing a regression-discontinuity design that utilizes a unique eligibility criterion of a benefit-expanding electoral pledge in the context of the 2013 federal election in Germany, our findings provide several key insights on pledge-based voting.

In general, the paper has shown that campaign promises on social benefit expansions substantively increase alignment with the pledge-making party. Notably, this alignment stems from a recalibration of voters traditionally inclined towards left-wing platforms. In contrast, we find no discernible impact on overall electoral participation, suggesting the persuasive rather than mobilizing nature of electoral pledges related to social benefit expansions.

Furthermore, our investigation highlights the differential sensitivity of individuals to campaign promises. To be more specific, individuals with lower economic and social security exhibit a considerably heightened responsiveness to benefit-related pledges, underscoring the pronounced electoral dividends accrued from social benefit expansion promises targeted at

economically vulnerable voters. In addition, the analysis has also highlighted the transitory nature of pledge-based policy feedbacks. Although initial effects enhance alignment with the pledge-making party, these effects dissipate shortly after reform implementation.

Overall, these findings aim to enrich our comprehension of political behaviour and preference formation in the electoral arena. They emphasize the strategic potency of pledge-based tactics by conservative parties to challenge left-wing platforms, underscore the temporary presence of pledge effects, and emphasize the role of economic and social security as a bulwark against partisan manoeuvres targeted towards socioeconomically disadvantaged voters.

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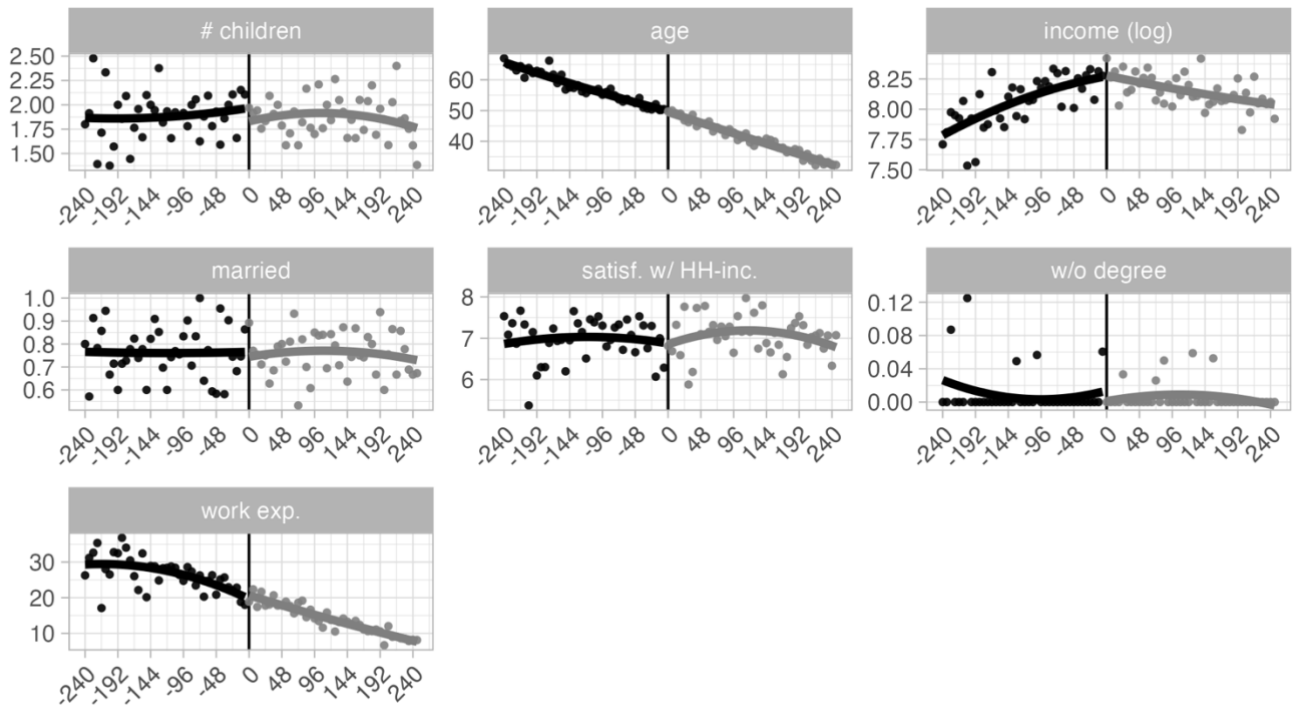
Part A: Identifying Assumptions

Table A.1: Empirical test for continuity in observables

	age	married	# children	w/o degree	income (log)	work exp.	satisf. w/ HH-inc.
ATT	0.063	0.002	-0.015	-0.010	0.032	0.642	0.133
	(0.329)	(0.030)	(0.049)	(0.008)	(0.030)	(0.587)	(0.153)
age		-0.004** (0.001)	-0.030*** (0.002)	0.000 (0.001)	0.010*** (0.002)	0.608*** (0.025)	0.010 (0.007)
married	-0.525** (0.161)		-0.004 (0.025)	0.006 (0.003)	0.415*** (0.015)	-1.718*** (0.271)	-0.151* (0.075)
# children	-1.287*** (0.095)	-0.001 (0.009)		0.007*** (0.002)	0.112*** (0.009)	-3.118*** (0.169)	-0.150*** (0.042)
w/o degree	-0.689 (1.184)	0.107 (0.058)	0.352*** (0.070)		-0.372*** (0.070)	-9.228*** (0.956)	0.168 (0.396)
income (log)	1.063*** (0.165)	0.376*** (0.014)	0.281*** (0.023)	-0.017*** (0.004)		1.538*** (0.287)	2.326*** (0.065)
work exp.	0.199*** (0.009)	-0.005*** (0.001)	-0.024*** (0.001)	-0.001*** (0.000)	0.005*** (0.001)		0.005 (0.004)
satisf. w/ HH-inc.	0.050 (0.036)	-0.007* (0.003)	-0.019*** (0.005)	0.000 (0.001)	0.114*** (0.003)	0.084 (0.062)	
intercept	40.111*** (1.222)	-1.958*** (0.117)	1.737*** (0.189)	0.179*** (0.037)	6.364*** (0.082)	-17.141*** -	-12.898*** -
N	2715	2715	2715	2715	2715	2715	2715
adj. R2	0.71	0.21	0.20	0.04	0.45	0.45	0.32
BIC	15457	2525	5286	-5522	2791	18494	10969
Log.Lik.	-7677	-1211	-2592	2812	-1344	-9195	-5433
Polyn. form	quadratic	quadratic	quadratic	quadratic	quadratic	quadratic	quadratic

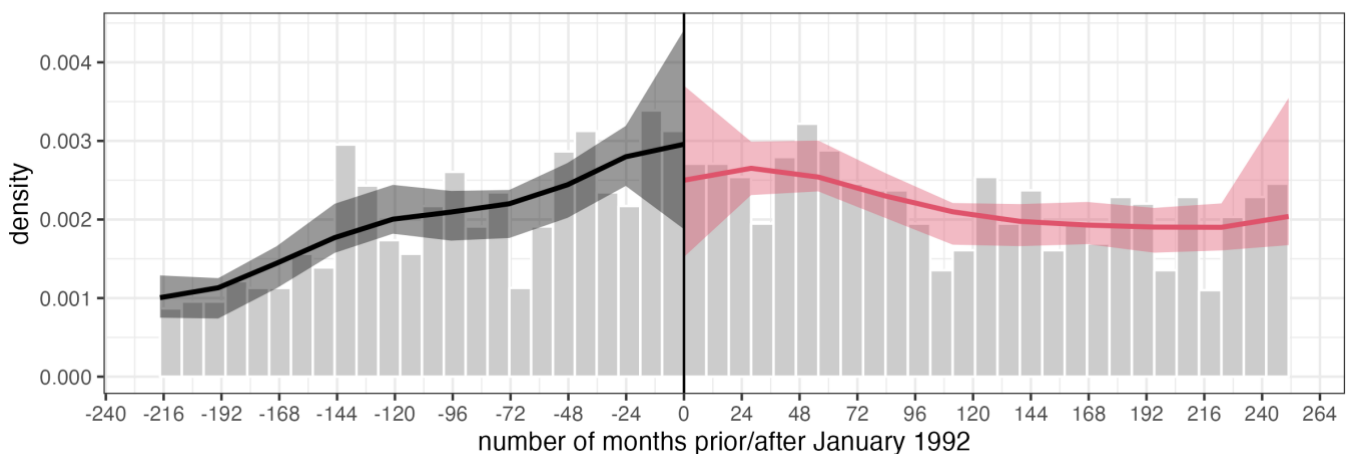
Note: The outcome variable is one of the respective observables indicated at the top of each column. All estimations include the following set of control variables excluding the variable that is used as dependent variable: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable (1st and 2nd polynomial terms). The estimation specification is based on the baseline approach, except a quadratic fit is used due to superior goodness of fit (based on Bayesian Information Criterion) compared to the linear fit. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Figure A.2: Visual test for continuity in observables around the cut-off date



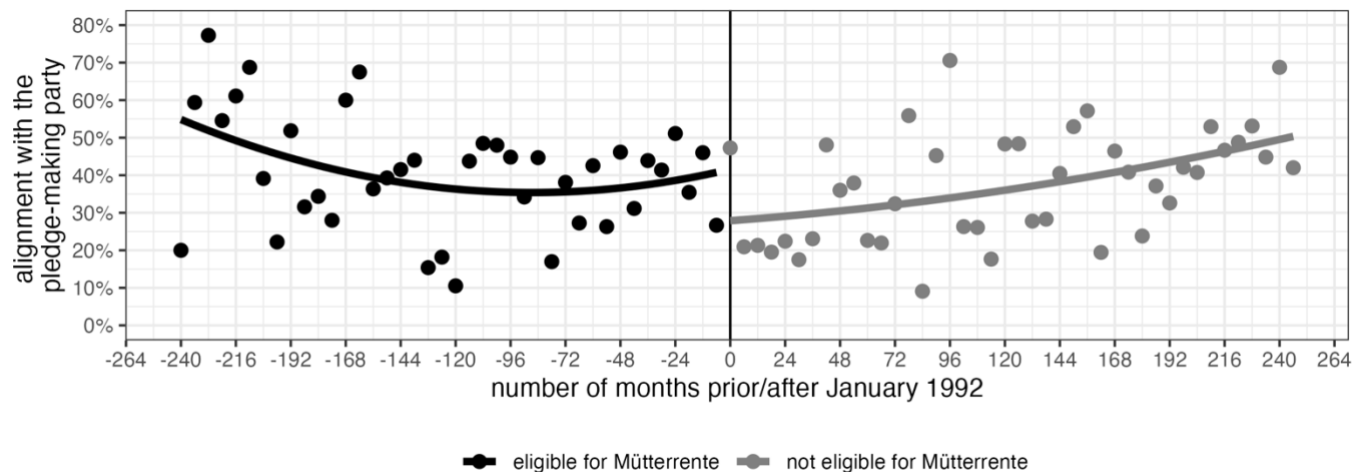
Note: The figure plots the biannual averages of different observable characteristics of mothers by the date of birth (year-month) of the first child. Black dots consist of mothers who gave birth to their first child before January 1992 (eligible for Mütterrente); grey dots consist of mothers who gave birth to their first child on or after January 1992 (not eligible for Mütterrente). The lines are based on a quadratic fit.

Figure A.3: Visual test for sorting – density plot along the running variable



Note: The figure plots the density along the running variable (number of months prior to/since January 1992). The black line is based on mothers who gave birth to their first child before January 1992 (eligible for Mütterrente); the red line is based on mothers who gave birth to their first child on or after January 1992 (not eligible for Mütterrente). The p-value of the McCrary density test is 0.519.

Figure A.4: Alignment with the pledge-making party along the running variable



Note: The figure plots the biannual averages of alignment level with the pledge-making party (conservative party) of mothers by the date of birth (year-month) of the first child. Black dots consist of mothers who gave birth to their first child before January 1992 (eligible for Mütterrente); grey dots consist of mothers who gave birth to their first child on or after January 1992 (not eligible for Mütterrente). The lines are based on a quadratic fit.

Part B: Alignment

Table B.1: Treatment effect (ATT) of the benefit promise on alignment by different control variable sets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.104*** (0.027)	-0.108*** (0.027)	-0.106*** (0.026)	-0.109*** (0.026)	-0.110*** (0.026)	-0.116*** (0.026)	-0.117*** (0.026)	-0.122*** (0.026)
age		-0.006*** (0.002)	-0.005*** (0.002)	-0.007*** (0.002)	-0.008*** (0.002)	-0.010*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)
married			0.155*** (0.015)	0.163*** (0.015)	0.162*** (0.015)	0.108*** (0.018)	0.106*** (0.018)	0.106*** (0.018)
# children				-0.041*** (0.011)	-0.040*** (0.011)	-0.048*** (0.011)	-0.052*** (0.011)	-0.048*** (0.011)
w/o degree					-0.119 (0.079)	-0.066 (0.078)	-0.076 (0.079)	-0.077 (0.080)
income (log)						0.096*** (0.016)	0.098*** (0.016)	0.058** (0.019)
work exp.							-0.001 (0.001)	-0.001 (0.001)
satisf. w/ HH-inc.								0.017*** (0.004)
intercept	0.366*** (0.019)	0.667*** (0.079)	0.524*** (0.080)	0.692*** (0.092)	0.700*** (0.092)	0.061 (0.137)	0.041 (0.138)	0.257 (0.149)
N	2500	2500	2500	2500	2500	2472	2472	2451
adj. R2	0.00	0.01	0.03	0.03	0.03	0.04	0.04	0.04
BIC	3420	3419	3377	3376	3382	3329	3336	3309
Log.Lik.	-1690	-1686	-1661	-1657	-1656	-1626	-1625	-1608

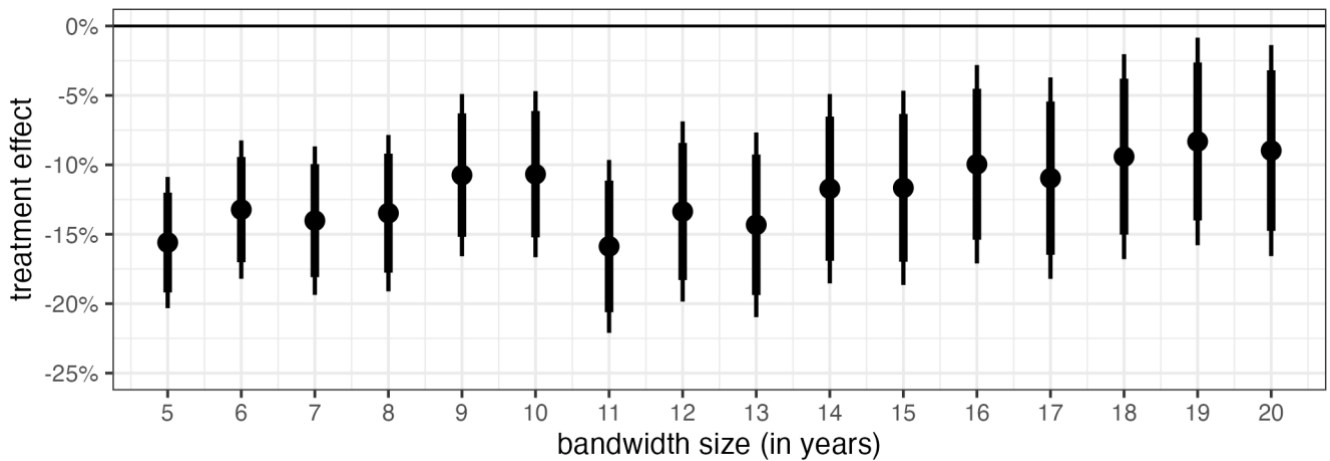
Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the running variable as control variable. Columns differ with respect to the control variable set. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.2: Treatment effect (ATT) of the benefit promise on alignment by different model specifications

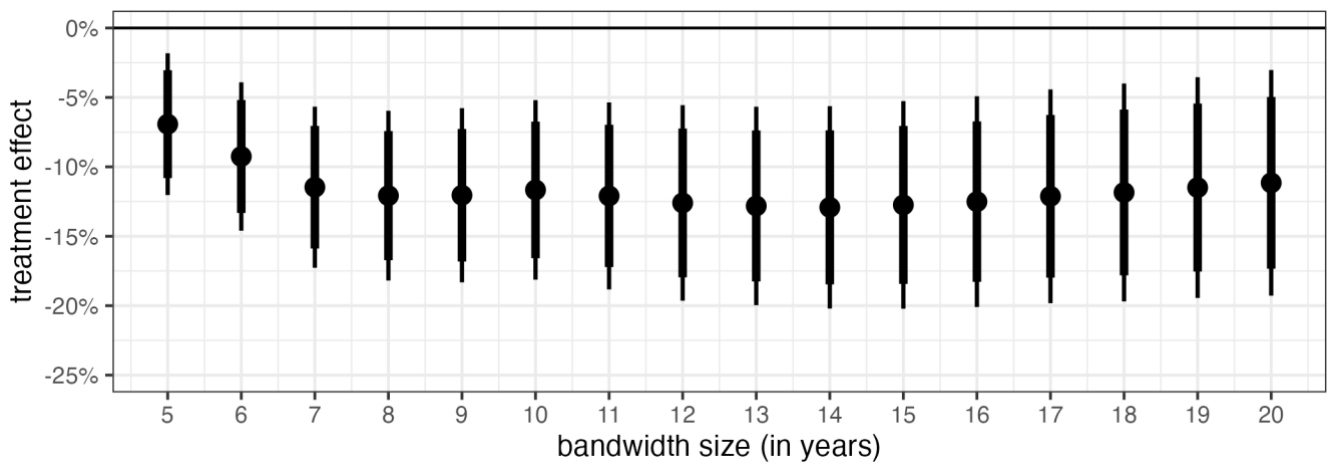
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.122*** (0.026)	-0.130*** (0.028)	-0.129*** (0.028)	-0.143*** (0.039)	-0.120*** (0.026)	0.558*** (0.070)	0.565*** (0.069)	0.560*** (0.070)
age	-0.009*** (0.002)	-0.010*** (0.002)	-0.010*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)	0.958*** (0.008)	0.957*** (0.008)	0.957*** (0.008)
married	0.110*** (0.018)	0.080*** (0.020)	0.080*** (0.020)	0.104*** (0.018)	0.105*** (0.018)	1.705*** (0.157)	1.732*** (0.159)	1.695*** (0.156)
# children	-0.048*** (0.011)	-0.031* (0.013)	-0.031* (0.013)	-0.049*** (0.011)	-0.049*** (0.011)	0.802*** (0.042)	0.804*** (0.042)	0.800*** (0.042)
w/o degree	-0.079 (0.080)	-0.074 (0.093)	-0.074 (0.093)	-0.077 (0.082)	-0.077 (0.080)	0.666 (0.292)	0.658 (0.288)	0.666 (0.294)
income (log)	0.049** (0.018)	0.091*** (0.021)	0.089*** (0.020)	0.058** (0.019)	0.060** (0.019)	1.310** (0.114)	1.257** (0.108)	1.322** (0.115)
work exp.	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.994 (0.005)	0.994 (0.005)	0.995 (0.005)
satisf. w/ HH-inc.	0.019*** (0.004)	0.017*** (0.004)	0.018*** (0.004)	0.018*** (0.004)	0.017*** (0.004)	1.089*** (0.021)	1.097*** (0.021)	1.088*** (0.021)
intercept	0.358* (0.145)	0.072 (0.165)	0.089 (0.162)	0.326* (0.151)	0.253 (0.148)	0.349 (0.238)	0.548 (0.363)	0.343 (0.233)
N	2451	2451	2451	2451	2451	2451	2451	2451
adj. R2	0.04	0.05	0.05	0.04	0.04	-	-	-
BIC	3306	3920	3913	3319	3306	3144	3141	3141
Log.Lik.	-1610	-1913	-1913	-1605	-1606	-1529	-1531	-1528
estimation method	parametric	non- parametric	non- parametric	parametric	parametric	parametric	parametric	parametric
model	linear	linear	linear	linear	linear	logit	logit	logit
order	1	1	1	2	2	1	1	2
slope	same slope	separate slope	same slope	separate slope	same slope	separate slope	same slope	separate slope

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Columns differ with respect to the statistical specifications they are estimated on. The estimates differ with respect to the estimation method (parametric vs. non-parametric), the measurement of the outcome variable (linear vs. logistic), the inclusion of high-order polynomial terms of the running variable, and the imposition of the slope-equality restrictions. The estimates of the logistic regressions represent odds ratios. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

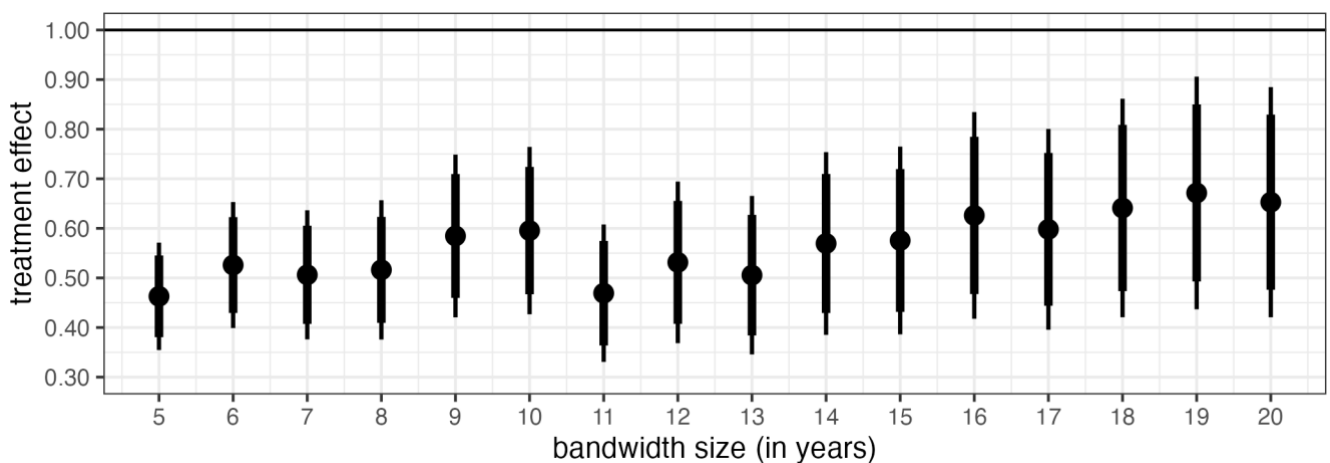
Figure B.3: Treatment effect (ATT) of the benefit promise on alignment by bandwidth size
(A)



(B)



(C)



Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates in panel A are based on the baseline specification; the estimates of panel B and C use the specification of the baseline with the only exception that they are based non-parametric (panel B) and logistic estimations (panel C), respectively. The estimates of the logistic regressions (panel C) represent odds ratios. Confidence intervals are based on robust standard errors. The thick (thin) lines represent 95% (99%) confidence levels, respectively.

Table B.4: Treatment effect (ATT) of the benefit promise on alignment by bandwidth size (I)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.156***	-0.132***	-0.140***	-0.135***	-0.107***	-0.107***	-0.159***	-0.134***
	(0.018)	(0.019)	(0.021)	(0.022)	(0.023)	(0.023)	(0.024)	(0.025)
age	-0.013***	-0.016***	-0.012***	-0.012***	-0.010***	-0.009***	-0.008***	-0.010***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
married	0.054***	0.048***	0.058***	0.066***	0.069***	0.083***	0.090***	0.089***
	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)
# children	-0.028***	-0.001	-0.033***	-0.040***	-0.042***	-0.038***	-0.042***	-0.043***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)
w/o degree	-0.245***	-0.270***	-0.029	-0.116	-0.041	-0.027	-0.036	-0.003
	(0.056)	(0.063)	(0.080)	(0.069)	(0.068)	(0.074)	(0.072)	(0.078)
income (log)	0.184***	0.137***	0.111***	0.115***	0.086***	0.053**	0.039*	0.061***
	(0.012)	(0.013)	(0.015)	(0.016)	(0.016)	(0.016)	(0.017)	(0.018)
work exp.	-0.002*	0.000	-0.001	-0.002*	-0.003***	-0.002*	-0.002	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
satisf. w/ HH-inc.	0.009***	0.013***	0.014***	0.012***	0.019***	0.022***	0.022***	0.018***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
intercept	-0.463***	-0.064	0.073	0.037	0.143	0.321*	0.405**	0.286*
	(0.096)	(0.104)	(0.114)	(0.122)	(0.126)	(0.130)	(0.135)	(0.142)
N	1081	1219	1422	1605	1736	1870	2039	2235
Bandwidth	5 years	6 years	7 years	8 years	9 years	10 years	11 years	12 years
adj. R2	0.07	0.07	0.05	0.05	0.05	0.04	0.04	0.04
BIC	1453	1624	1906	2148	2335	2501	2730	2995
Log.Lik.	-685	-770	-909	-1030	-1123	-1206	-1319	-1451

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on the baseline specification. Columns differ with respect to the bandwidth size. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.5: Treatment effect (ATT) of the benefit promise on alignment by bandwidth size (II)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
ATT	-0.143*** (0.026)	-0.117*** (0.026)	-0.117*** (0.027)	-0.100*** (0.028)	-0.110*** (0.028)	-0.094** (0.029)	-0.083** (0.029)	-0.090** (0.030)
age	-0.010*** (0.002)	-0.009*** (0.002)	-0.008*** (0.002)	-0.006** (0.002)	-0.006** (0.002)	-0.005** (0.002)	-0.007*** (0.002)	-0.006** (0.002)
married	0.100*** (0.018)	0.103*** (0.018)	0.089*** (0.019)	0.084*** (0.019)	0.096*** (0.019)	0.101*** (0.020)	0.112*** (0.020)	0.112*** (0.020)
# children	-0.043*** (0.011)	-0.049*** (0.011)	-0.044*** (0.012)	-0.045*** (0.012)	-0.046*** (0.012)	-0.048*** (0.013)	-0.047*** (0.013)	-0.045*** (0.013)
w/o degree	-0.029 (0.082)	-0.078 (0.081)	-0.075 (0.084)	-0.069 (0.088)	-0.116 (0.081)	-0.125 (0.085)	-0.138 (0.082)	-0.137 (0.085)
income (log)	0.058** (0.018)	0.058** (0.019)	0.054** (0.019)	0.060** (0.019)	0.051** (0.020)	0.051* (0.020)	0.048* (0.020)	0.051* (0.020)
work exp.	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.001)	-0.002* (0.001)	-0.003* (0.001)	-0.003* (0.001)	-0.002* (0.001)
satisf. w/ HH-inc.	0.017*** (0.004)	0.016*** (0.004)	0.017*** (0.004)	0.016*** (0.004)	0.016*** (0.004)	0.015*** (0.004)	0.015*** (0.004)	0.014** (0.004)
intercept	0.297* (0.146)	0.288 (0.149)	0.238 (0.154)	0.123 (0.156)	0.193 (0.160)	0.182 (0.163)	0.237 (0.166)	0.185 (0.168)
N	2349	2471	2606	2738	2835	2945	3050	3177
Bandwidth	13 years	14 years	15 years	16 years	17 years	18 years	19 years	20 years
adj. R2	0.04	0.04	0.03	0.03	0.03	0.03	0.04	0.04
BIC	3154	3340	3534	3719	3854	4010	4159	4344
Log.Lik.	-1531	-1623	-1720	-1812	-1879	-1957	-2031	-2124

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on the baseline specification. Columns differ with respect to the bandwidth size. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.6: Treatment effect (ATT) of the benefit promise on alignment with different parties

	(1)	(2)	(3)	(4)	(5)
ATT	0.123*** (0.025)	-0.006 (0.027)	0.016* (0.007)	-0.016 (0.011)	0.000 (0.004)
age	0.015*** (0.002)	0.000 (0.002)	0.000 (0.000)	-0.005*** (0.001)	0.000 (0.000)
married	-0.077*** (0.017)	-0.018 (0.019)	0.008 (0.004)	-0.037*** (0.009)	0.003 (0.002)
# children	0.060*** (0.011)	-0.016 (0.011)	0.003 (0.002)	-0.005 (0.006)	-0.001 (0.001)
w/o degree	-0.196*** (0.024)	0.427*** (0.076)	-0.023*** (0.006)	-0.085*** (0.011)	-0.002 (0.001)
income (log)	0.047** (0.015)	-0.086*** (0.017)	0.008 (0.005)	-0.006 (0.008)	-0.002 (0.002)
work exp.	-0.001 (0.001)	0.003** (0.001)	-0.001* (0.000)	0.001 (0.000)	0.000* (0.000)
satisf. w/ HH- inc.	0.003 (0.003)	-0.004 (0.004)	-0.002 (0.001)	-0.010*** (0.002)	0.000 (0.000)
intercept	-0.981*** (0.123)	1.039*** (0.145)	-0.069 (0.042)	0.448*** (0.069)	0.030 (0.016)
N	2451	2451	2451	2451	2451
adj. R2	0.05	0.02	0.00	0.04	0.00
BIC	2819	3278	-3092	-616	-8150
Log.Lik.	-1363	-1592	1593	355	4122
Party	Die Grünen (greens)	SPD (social dem.)	FDP (liberals)	Die Linke (left social.)	AFD (nationalists)

Note: The outcome variables are binary variables indicating alignment with a competitor party of the pledge-maker. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on the baseline specification. Columns differ with party under consideration. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table B.7: Heterogeneity of the treatment effect (ATT) of the benefit promise on alignment with the pledge-making party (non-parametric)

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.111** (0.043)	-0.038 (0.051)	-0.142*** (0.036)	-0.087* (0.035)	-0.208*** (0.048)	-0.259*** (0.066)
intercept	0.388*** (0.026)	0.272*** (0.037)	0.430*** (0.026)	0.422*** (0.025)	0.278*** (0.038)	0.530*** (0.049)
N	1473	742	1709	1854	597	560
adj. R2	0.01	0.01	0.01	0.00	0.02	0.02
BIC	2303	1206	2794	3123	819	930
Log.Lik.	-1133	-586	-1379	-1543	-393	-449
p-value	0.9469	0.2656	0.5789	0.5762	0.166	0.0492
sub-sampling variable	age	# of children	# of children	marital status	marital status	income
sub-sampling rule	older than 47	less than 2 children	more than 1 child	married	not married	less than 850€/month

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. The columns differ with respect to the sample their respective coefficients are estimated on, indicated by the sub-sampling variable and sub-sampling rule. The specification of estimation follows the baseline structure, expect the exclusion of all control variables (to ensure comparability across different sub-samples). The coefficients represent non-parametric estimates. For each estimation, a t-test is conducted to test whether the treatment effect of the sub-sample is statistically significantly different to the baseline ATT. The p-value indicates whether the null hypothesis of equality between the baseline and sub-sampling estimate can be rejected. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.8: Heterogeneity of the treatment effect (ATT) of the benefit promise on alignment with the pledge-making party (logistic)

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.565** (0.113)	0.871 (0.191)	0.538*** (0.081)	0.723* (0.100)	0.302*** (0.084)	0.303*** (0.082)
intercept	0.559*** (0.059)	0.401*** (0.062)	0.676*** (0.068)	0.634*** (0.060)	0.469*** (0.086)	1.117 (0.211)
N	1473	742	1709	1854	597	560
BIC	1903	986	2221	2497	663	755
Log.Lik.	-937	-480	-1096	-1234	-319	-365
p-value	0.7266	0.3298	0.5609	0.5397	0.0836	0.0449
sub-sampling variable	age	# of children	# of children	marital status	marital status	income
sub-sampling rule	older than 47	less than 2 children	more than 1 child	married	not married	less than 850€/month

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. The columns differ with respect to the sample their respective coefficients are estimated on, indicated by the sub-sampling variable and sub-sampling rule. The specification of estimation follows the baseline structure, expect the exclusion of all control variables (to ensure comparability across different sub-samples). The coefficients represent the odds ratio of a logistic regression. For each estimation, a t-test is conducted to test whether the treatment effect of the sub-sample is statistically significantly different to the baseline ATT. The p-value indicates whether the null hypothesis of equality between the baseline and sub-sampling estimate can be rejected. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Part C: Turnout

Table C.1: Treatment effect (ATT) of the benefit promise on turnout by different control variable sets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.018	-0.026	-0.020	-0.021	-0.025	-0.009	-0.008	-0.008
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
age		0.023***	0.022***	0.023***	0.022***	0.019***	0.017***	0.017***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
married			0.117***	0.113***	0.110***	0.031*	0.035**	0.036**
			(0.010)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
# children				0.009	0.010	-0.008	0.000	0.002
				(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
w/o degree					-0.271***	-0.231***	-0.187**	-0.184**
					(0.061)	(0.061)	(0.064)	(0.065)
income (log)						0.148***	0.143***	0.129***
						(0.011)	(0.010)	(0.012)
work exp.							0.003***	0.003***
							(0.001)	(0.001)
satisf. w/ HH-inc.								0.005*
								(0.003)
intercept	0.837***	-0.270***	-0.339***	-0.376***	-0.339***	-1.286***	-1.212***	-1.141***
	(0.015)	(0.051)	(0.051)	(0.057)	(0.057)	(0.090)	(0.090)	(0.096)
N	3604	3604	3600	3600	3595	3476	3459	3448
adj. R2	0.01	0.08	0.10	0.10	0.11	0.13	0.13	0.13
BIC	3707	3419	3346	3353	3339	3151	3134	3126
Log.Lik.	-1833	-1685	-1644	-1644	-1633	-1534	-1522	-1514

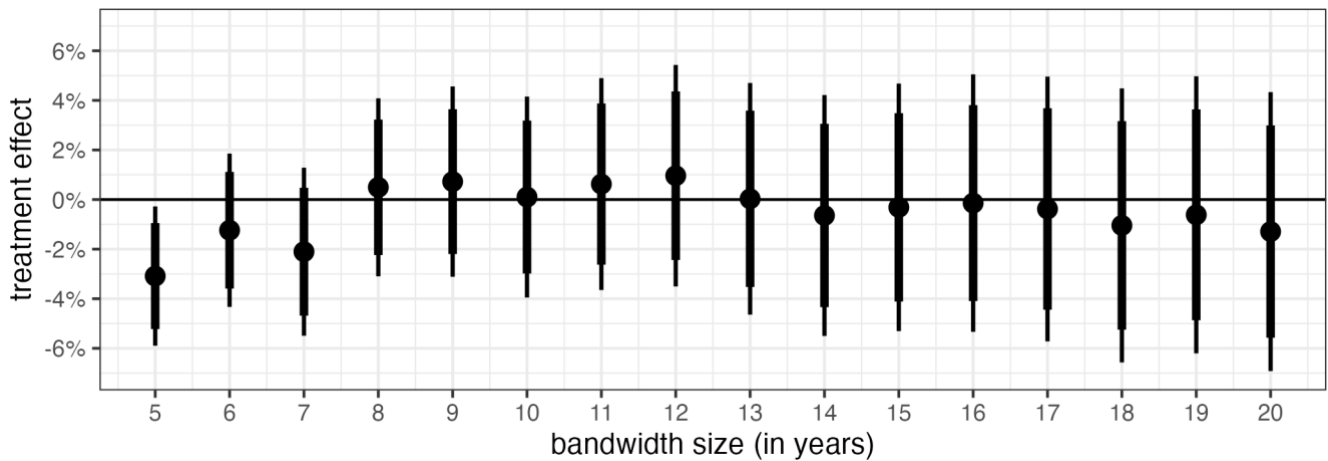
Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Columns differ with respect to the control variable set. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.2: Treatment effect (ATT) of the benefit promise on turnout by different model specifications

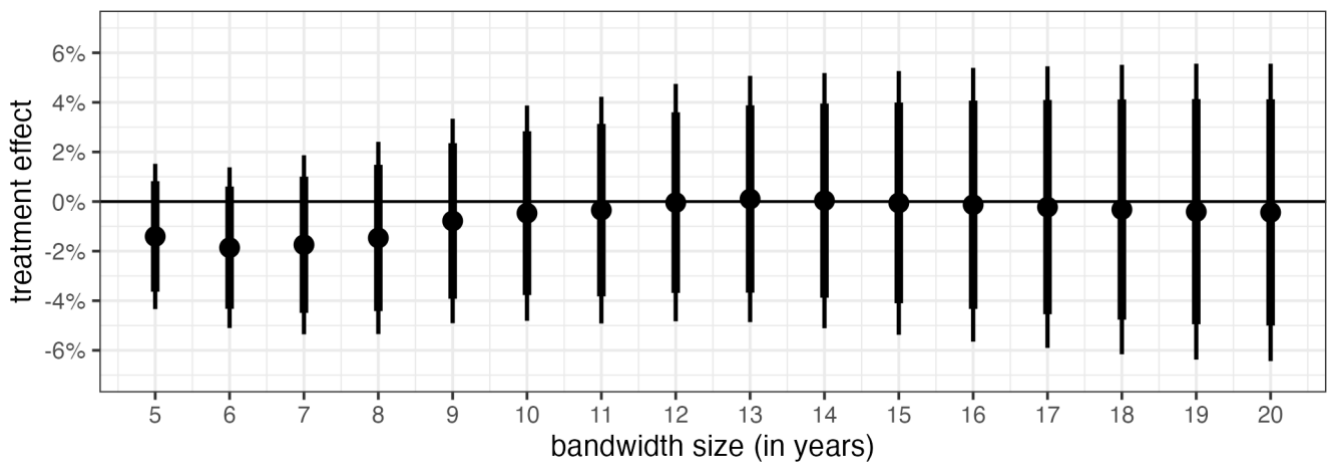
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.007 (0.018)	0.001 (0.020)	0.002 (0.020)	0.015 (0.028)	-0.007 (0.019)	0.892 (0.130)	0.888 (0.124)	1.120 (0.244)
age	0.017*** (0.001)	0.016*** (0.001)	0.016*** (0.001)	0.017*** (0.001)	0.017*** (0.001)	1.129*** (0.010)	1.129*** (0.010)	1.129*** (0.010)
married	0.035** (0.012)	0.029* (0.014)	0.029* (0.014)	0.035** (0.012)	0.035** (0.012)	1.175* (0.096)	1.176* (0.095)	1.166 (0.096)
# children	0.002 (0.007)	0.000 (0.008)	0.000 (0.008)	0.002 (0.007)	0.002 (0.007)	0.981 (0.050)	0.981 (0.050)	0.984 (0.050)
w/o degree	-0.184** (0.065)	-0.256*** (0.073)	-0.256*** (0.073)	-0.183** (0.065)	-0.184** (0.065)	0.574 (0.180)	0.574 (0.180)	0.579 (0.181)
income (log)	0.129*** (0.012)	0.133*** (0.014)	0.133*** (0.013)	0.129*** (0.012)	0.129*** (0.012)	2.738*** (0.259)	2.733*** (0.254)	2.740*** (0.259)
work exp.	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	1.012* (0.006)	1.012* (0.006)	1.012* (0.006)
satisf. w/ HH-inc.	0.005* (0.003)	0.007* (0.003)	0.007* (0.003)	0.005* (0.003)	0.005* (0.003)	1.028 (0.017)	1.028 (0.017)	1.028 (0.017)
intercept	-1.144*** (0.092)	-1.148*** (0.109)	-1.156*** (0.102)	-1.148*** (0.098)	-1.145*** (0.094)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
N	3448	3448	3448	3448	3448	3448	3448	3448
adj. R2	0.13	0.14	0.14	0.13	0.13	-	-	-
BIC	3118	4012	4004	3142	3126	3101	3092	3116
Log.Lik.	-1514	-1957	-1957	-1514	-1514	-1506	-1506	-1505
estimation method	parametric	non- parametric	non- parametric	parametric	parametric	parametric	parametric	parametric
model	linear	linear	linear	linear	linear	logit	logit	logit
order	1	1	1	2	2	1	1	2
slope	same slope	separate slope	same slope	separate slope	same slope	separate slope	same slope	separate slope

Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Columns differ with respect to the statistical specifications they are estimated on. The estimates differ with respect to the estimation method (parametric vs. non-parametric), the measurement of the outcome variable (linear vs. logistic), the inclusion of high-order polynomial terms of the running variable, and the imposition of the slope-equality restrictions. The estimates of the logistic regressions represent odds ratios. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

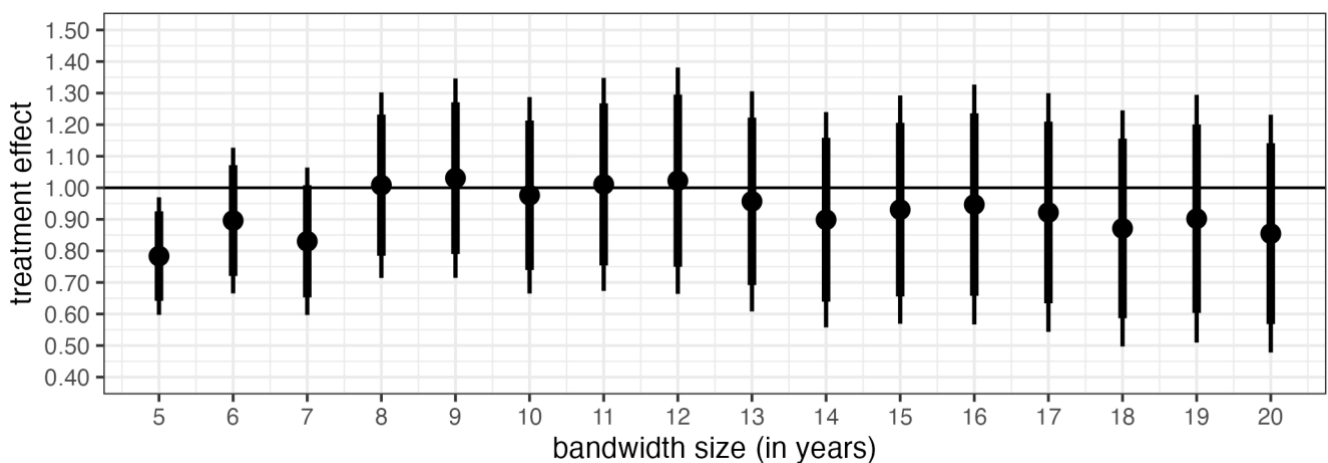
Figure C.3: Treatment effect (ATT) of the benefit promise on turnout by bandwidth size
(A)



(B)



(C)



Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates in panel A are based on the baseline specification; the estimates of panel B and C use the specification of the baseline with the only exception that they are based non-parametric (panel B) and logistic estimations (panel C), respectively. The estimates of the logistic regressions (panel C) represent odds ratios. Confidence intervals are based on robust standard errors. The thick (thin) lines represent 95% (99%) confidence levels, respectively.

Table C.4: Treatment effect (ATT) of the benefit promise on turnout by bandwidth size (I)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.031**	-0.012	-0.021	0.005	0.007	0.001	0.006	0.010
	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)
age	0.013***	0.015***	0.017***	0.017***	0.017***	0.017***	0.016***	0.016***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
married	0.047***	0.038***	0.032***	0.032***	0.028**	0.030**	0.023*	0.029*
	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)	(0.012)
# children	-0.003	0.000	-0.003	-0.001	0.003	0.003	0.002	0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
w/o degree	-0.283***	-0.237***	-0.289***	-0.272***	-0.294***	-0.311***	-0.312***	-0.240***
	(0.043)	(0.043)	(0.043)	(0.046)	(0.048)	(0.051)	(0.051)	(0.059)
income (log)	0.125***	0.132***	0.136***	0.128***	0.125***	0.121***	0.126***	0.132***
	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)
work exp.	0.003***	0.003***	0.002***	0.003***	0.003***	0.003***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
satisf. w/ HH-inc.	0.015***	0.013***	0.008***	0.006**	0.005*	0.005*	0.005*	0.005*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
intercept	-0.997***	-1.103***	-1.203***	-1.177***	-1.146***	-1.085***	-1.094***	-1.144***
	(0.058)	(0.064)	(0.068)	(0.073)	(0.078)	(0.080)	(0.087)	(0.090)
N	1248	1483	1769	2020	2280	2529	2790	3049
Bandwidth	5 years	6 years	7 years	8 years	9 years	10 years	11 years	12 years
adj. R2	0.14	0.14	0.15	0.14	0.14	0.13	0.13	0.13
BIC	1051	1263	1529	1742	1996	2238	2506	2748
Log.Lik.	-483	-588	-719	-825	-952	-1072	-1205	-1326

Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on the baseline specification. Columns differ with respect to the bandwidth size. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.5: Treatment effect (ATT) of the benefit promise on turnout by bandwidth size (II)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
ATT	0.000 (0.018)	-0.006 (0.019)	-0.003 (0.019)	-0.001 (0.020)	-0.004 (0.021)	-0.010 (0.021)	-0.006 (0.022)	-0.013 (0.022)
age	0.016*** (0.001)	0.017*** (0.001)	0.016*** (0.001)	0.016*** (0.001)	0.016*** (0.001)	0.017*** (0.001)	0.017*** (0.001)	0.017*** (0.001)
married	0.035** (0.012)	0.034** (0.012)	0.029* (0.013)	0.031* (0.013)	0.032* (0.014)	0.034* (0.014)	0.032* (0.014)	0.031* (0.014)
# children	0.003 (0.007)	0.004 (0.007)	0.003 (0.008)	0.003 (0.008)	0.002 (0.008)	0.005 (0.008)	0.006 (0.008)	0.005 (0.009)
w/o degree	-0.196** (0.063)	-0.202** (0.063)	-0.188** (0.066)	-0.203** (0.067)	-0.180** (0.068)	-0.172* (0.070)	-0.190** (0.066)	-0.197** (0.067)
income (log)	0.129*** (0.012)	0.129*** (0.012)	0.134*** (0.013)	0.127*** (0.013)	0.133*** (0.013)	0.136*** (0.014)	0.138*** (0.014)	0.140*** (0.014)
work exp.	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003** (0.001)
satisf. w/ HH-inc.	0.005 (0.003)	0.006* (0.003)	0.006* (0.003)	0.007* (0.003)	0.006* (0.003)	0.006* (0.003)	0.006* (0.003)	0.006* (0.003)
intercept	-1.119*** (0.094)	-1.148*** (0.096)	-1.165*** (0.099)	-1.133*** (0.101)	-1.173*** (0.103)	-1.216*** (0.107)	-1.241*** (0.108)	-1.258*** (0.109)
N	3272	3500	3721	3974	4185	4416	4582	4685
Bandwidth	13 years	14 years	15 years	16 years	17 years	18 years	19 years	20 years
adj. R2	0.13	0.13	0.13	0.13	0.13	0.14	0.14	0.14
BIC	2968	3185	3398	3652	3835	4025	4160	4237
Log.Lik.	-1435	-1544	-1650	-1776	-1868	-1962	-2029	-2068

Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on the baseline specification. Columns differ with respect to the bandwidth size. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.6: Heterogeneity of the treatment effect (ATT) of the benefit promise on turnout (non-parametric)

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.024 (0.026)	-0.014 (0.037)	-0.016 (0.024)	0.008 (0.021)	-0.054 (0.042)	-0.096* (0.041)
intercept	0.895*** (0.018)	0.870*** (0.028)	0.818*** (0.018)	0.858*** (0.016)	0.789*** (0.033)	0.863*** (0.032)
N	1486	941	2507	2242	1206	979
adj. R2	0.01	0.00	0.01	0.01	0.00	0.01
BIC	942	938	2624	1894	1530	1140
Log.Lik.	-453	-452	-1292	-928	-747	-553
p-value	0.3392	0.9594	0.9666	0.5431	0.5705	0.2402
sub-sampling variable	age	# of children	# of children	marital status	marital status	income
sub-sampling rule	older than 47	less than 2 children	less than 1 child	married	not married	less than 850€/month

Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. The columns differ with respect to the sample their respective coefficients are estimated on, indicated by the sub-sampling variable and sub-sampling rule. The specification of estimation follows the baseline structure, expect the exclusion of all control variables (to ensure comparability across different sub-samples). For each estimation, a t-test is conducted to test whether the treatment effect of the sub-sample is statistically significantly different to the baseline ATT. The p-value indicates whether the null hypothesis of equality between the baseline and sub-sampling estimate can be rejected. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.7: Heterogeneity of the treatment effect (ATT) of the benefit promise on turnout (logistic)

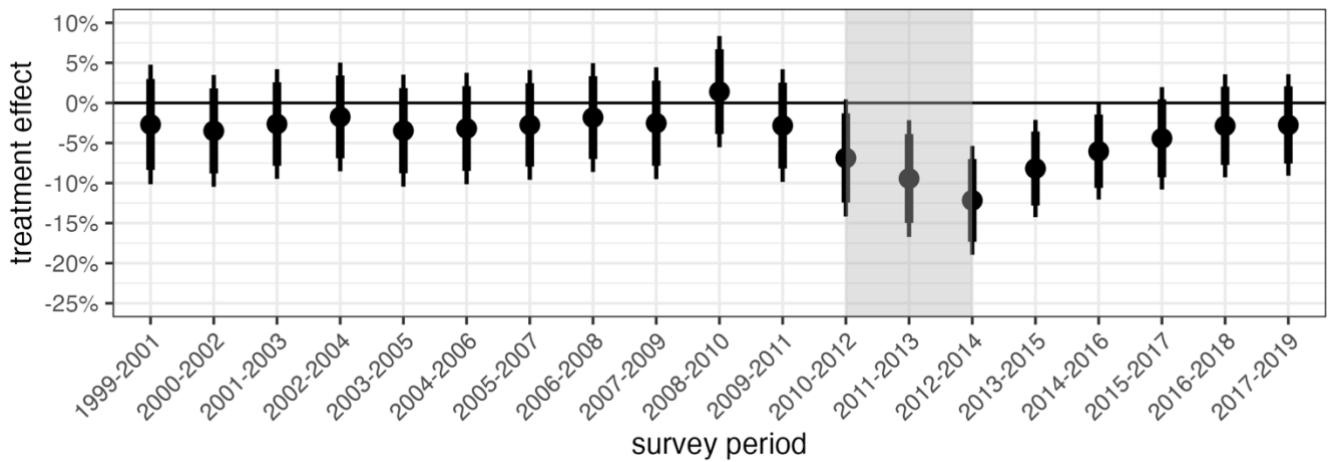
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	1.347 (0.371)	0.871 (0.238)	0.901 (0.142)	1.056 (0.188)	0.757 (0.169)	0.527* (0.149)
intercept	8.310*** (1.382)	6.562*** (1.454)	4.487*** (0.579)	5.967*** (0.865)	3.665*** (0.666)	6.230*** (1.545)
N	1486	941	2507	2242	1206	979
BIC	1131	937	2584	1989	1461	1098
Log.Lik.	-551	-455	-1276	-979	-716	-535
p-value	0.2455	0.9999	0.9054	0.5386	0.6835	0.2258
sub-sampling variable	age	# of children	# of children	marital status	marital status	income
sub-sampling rule	older than 47	less than 2 children	less than 1 child	married	not married	less than 850€/month

Note: The outcome variable is a binary variable indicating whether an individual has voted in the federal election in 2013. The columns differ with respect to the sample their respective coefficients are estimated on, indicated by the sub-sampling variable and sub-sampling rule. The specification of estimation follows the baseline structure, expect the exclusion of all control variables (to ensure comparability across different sub-samples). The coefficients represent odds ratios of a logistic regression. For each estimation, a t-test is conducted to test whether the treatment effect of the sub-sample is statistically significantly different to the baseline ATT. The p-value indicates whether the null hypothesis of equality between the baseline and sub-sampling estimate can be rejected. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

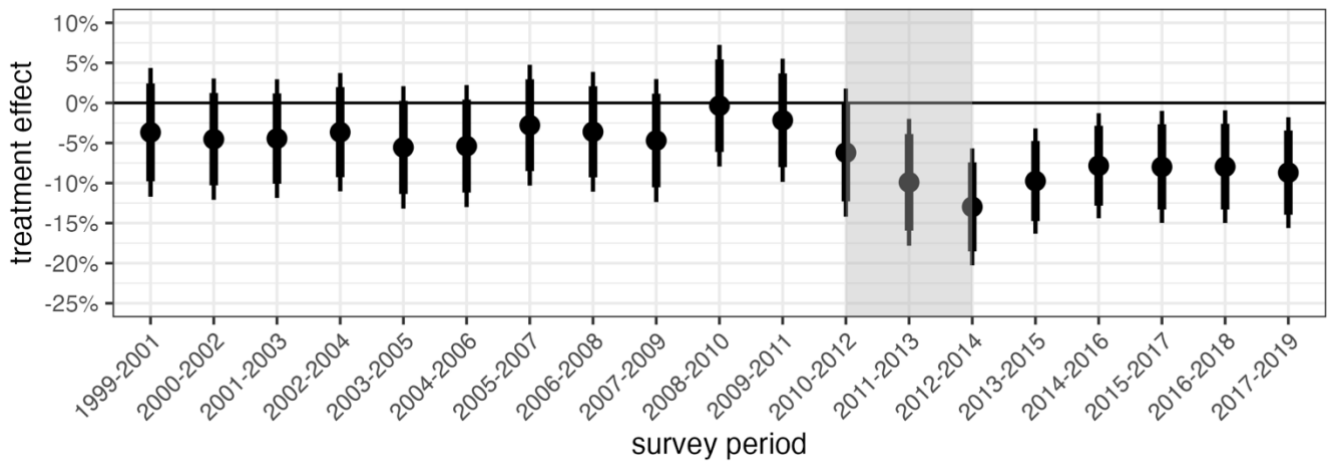
Part D: Durability

Figure D.1: Treatment effect (ATT) of the benefit promise on alignment with the pledge-making party over time

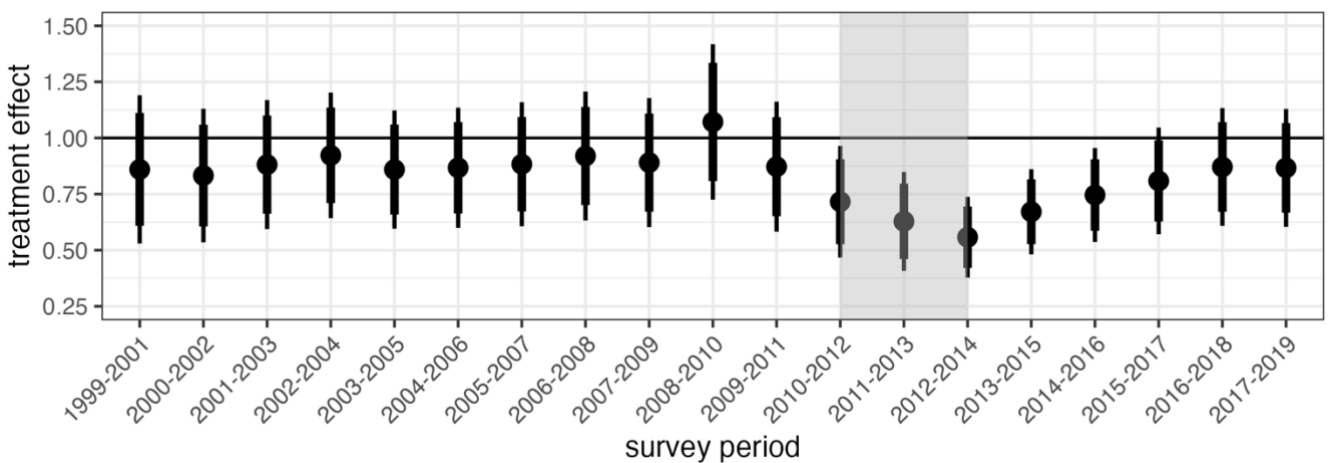
(A)



(B)



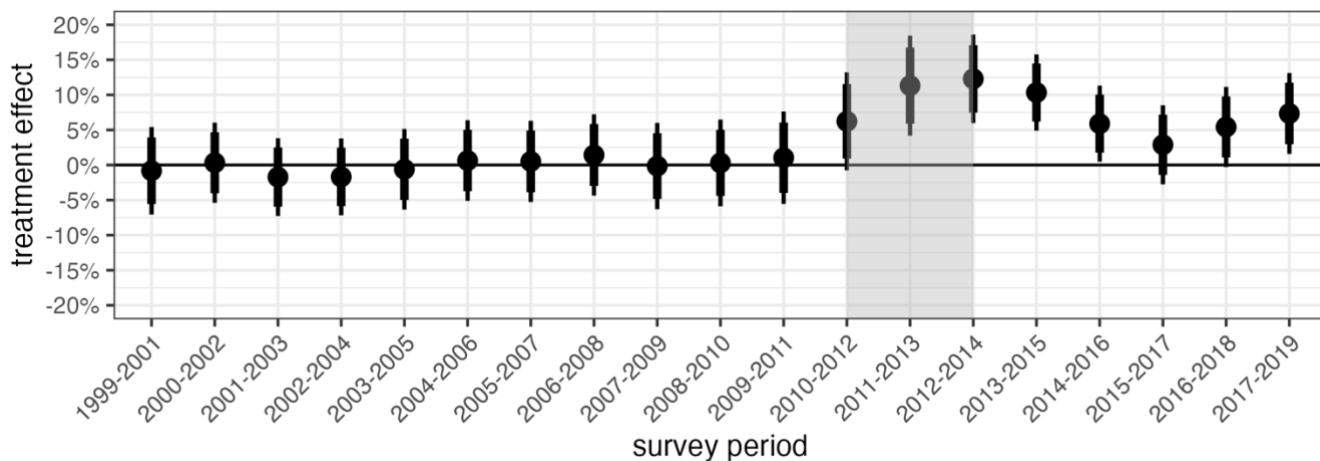
(C)



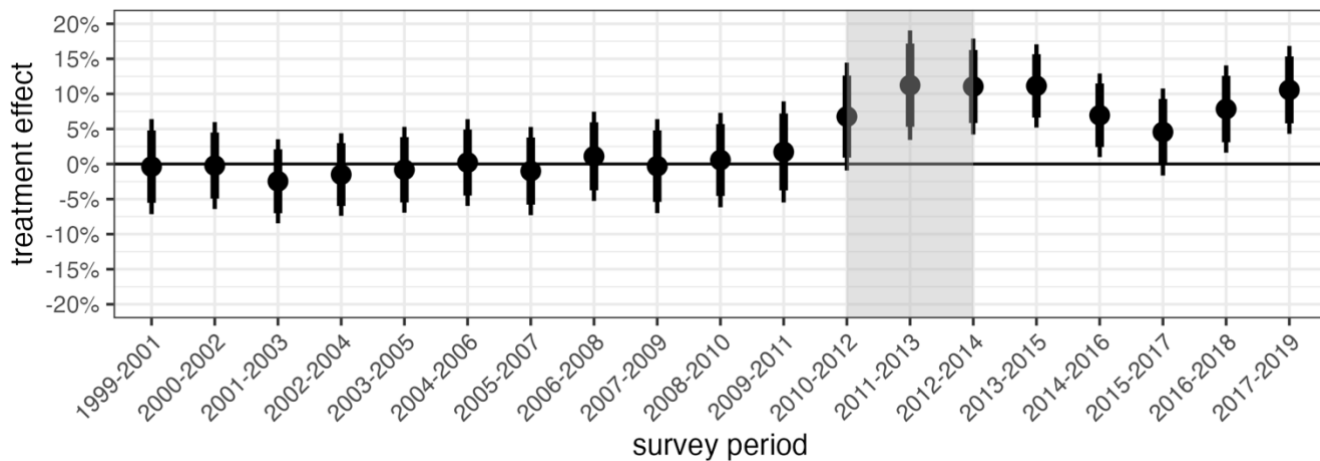
Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Each estimate is based on the baseline specification (Panel A) and uses a 3-year survey period (x-axis). Panel B and C use a non-parametric and logistic estimation approach, respectively. The grey area represents (approx.) the period when the pledge was made. Confidence intervals are based on robust standard errors. The thick and thin lines represent 95% and 99% confidence levels, respectively.

Figure D.2: Treatment effect (ATT) of the benefit promise on alignment with the green party over time

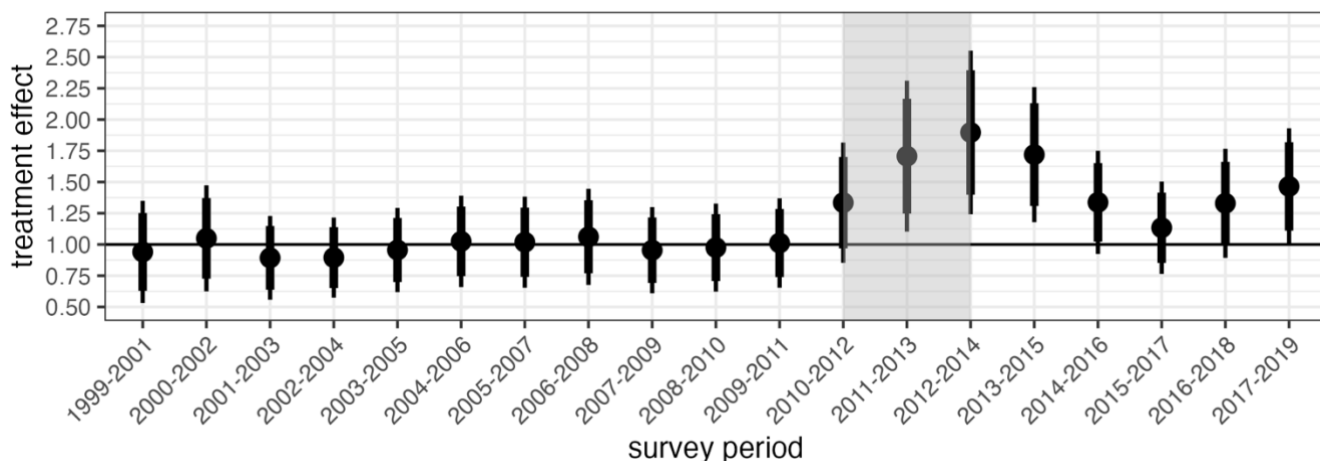
(A)



(B)



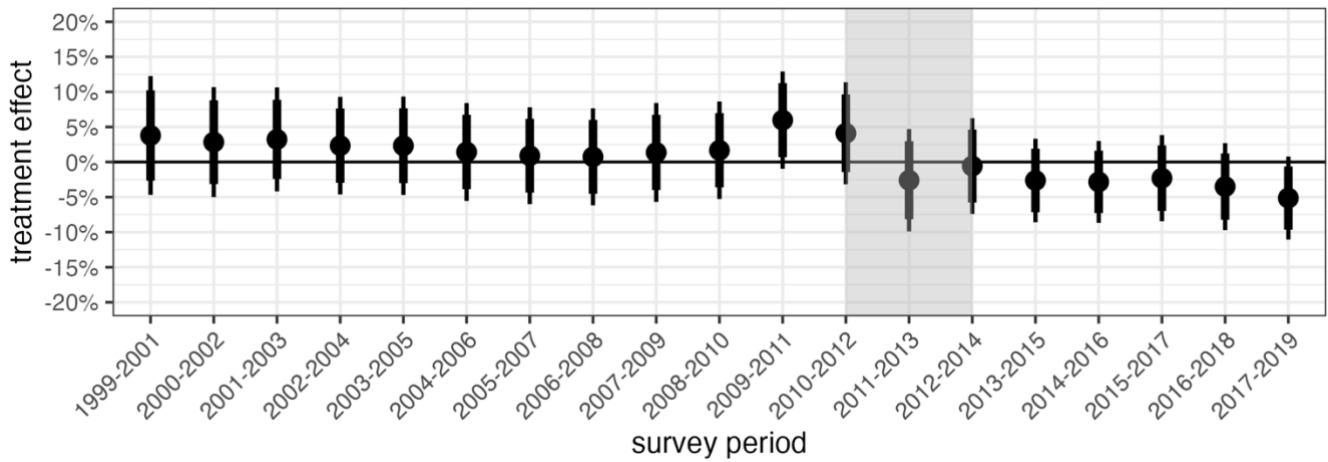
(C)



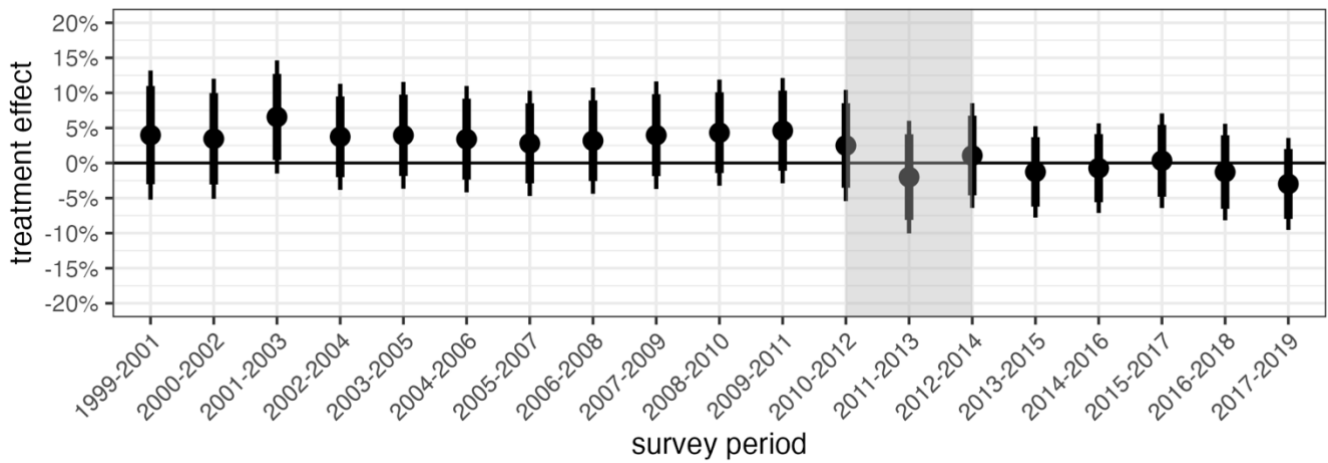
Note: The outcome variable is a binary variable indicating alignment with the green party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Each estimate is based on the baseline specification (Panel A) and uses a 3-year survey period (x-axis). The grey area represents (approx.) the period when the pledge was made. Panel B and C use a non-parametric and logistic estimation approach, respectively. Confidence intervals are based on robust standard errors. The thick and thin lines represent 95% and 99% confidence levels, respectively.

Figure D.3: Treatment effect (ATT) of the benefit promise on alignment with the social democratic party over time

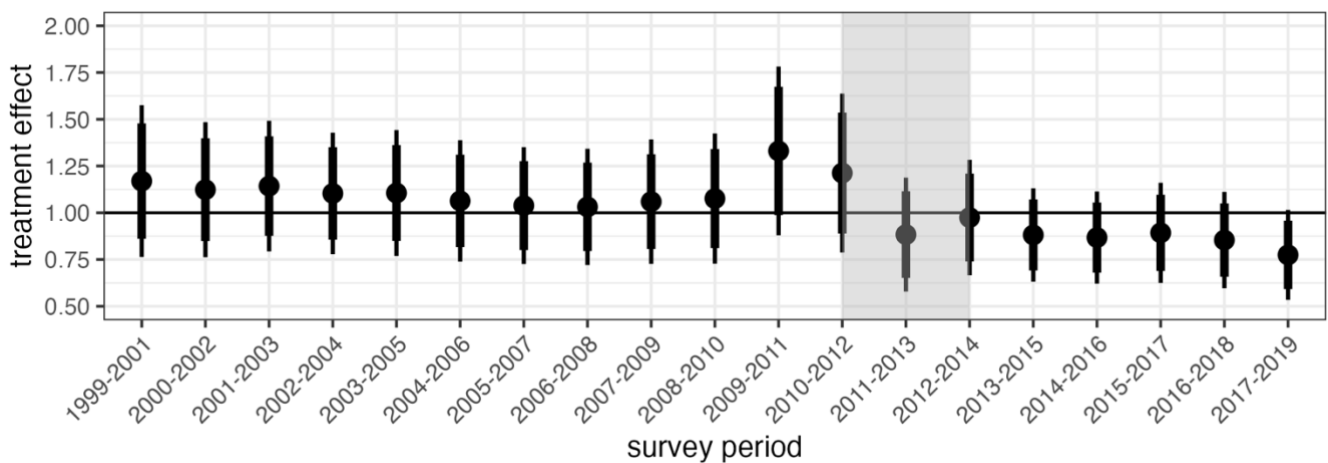
(A)



(B)



(C)



Note: The outcome variable is a binary variable indicating alignment with the social democratic party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. Each estimate is based on the baseline specification (Panel A) and uses a 3-year survey period (x-axis). The grey area represents (approx.) the period when the pledge was made. Panel B and C use a non-parametric and logistic estimation approach, respectively. Confidence intervals are based on robust standard errors. The thick and thin lines represent 95% and 99% confidence levels, respectively.

Table D.4: Treatment effect (ATT) of the benefit promise on alignment with the pledge-making party over time (I)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.027	-0.035	-0.026	-0.017	-0.035	-0.032	-0.027
	(0.029)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)
age	-0.017***	-0.017***	-0.017***	-0.015***	-0.016***	-0.013***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
married	0.125***	0.119***	0.147***	0.107***	0.092***	0.088***	0.081***
	(0.019)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)
# children	0.055***	0.048***	0.025*	0.016	-0.005	0.009	0.007
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
w/o degree	-0.106	-0.172*	0.076	0.015	-0.104	-0.082	-0.136
	(0.073)	(0.074)	(0.113)	(0.103)	(0.093)	(0.100)	(0.096)
income (log)	-0.017	-0.005	0.007	0.029	0.032	0.043*	0.051**
	(0.022)	(0.020)	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)
work exp.	0.009***	0.008***	0.006***	0.004**	0.003*	0.004**	0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
satisf. w/ HH-inc.	0.014***	0.012***	0.010**	0.009*	0.010**	0.016***	0.022***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
intercept	0.638***	0.630***	0.607***	0.495***	0.634***	0.373**	0.254
	(0.155)	(0.146)	(0.131)	(0.126)	(0.133)	(0.131)	(0.131)
Period	1999-2001	2000-2002	2001-2003	2002-2004	2003-2005	2004-2006	2005-2007
N	2314	2748	3083	3252	3077	2934	2882
adj. R2	0.04	0.04	0.03	0.02	0.03	0.03	0.04
BIC	2968	3477	4061	4445	4228	4007	3858
Log.Lik.	-1438	-1691	-1982	-2174	-2066	-1956	-1881

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table D.5: Treatment effect (ATT) of the benefit promise on alignment with the pledge-making party over time (II)

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
ATT	-0.018 (0.026)	-0.025 (0.027)	0.014 (0.027)	-0.028 (0.027)	-0.069* (0.028)	-0.094*** (0.028)	-0.122*** (0.026)
age	-0.010*** (0.002)	-0.010*** (0.002)	-0.012*** (0.002)	-0.013*** (0.002)	-0.012*** (0.002)	-0.011*** (0.002)	-0.009*** (0.002)
married	0.095*** (0.018)	0.085*** (0.019)	0.106*** (0.019)	0.082*** (0.019)	0.071*** (0.019)	0.070*** (0.019)	0.106*** (0.018)
# children	0.017 (0.011)	0.025* (0.011)	0.024* (0.011)	0.015 (0.011)	0.007 (0.012)	-0.023 (0.012)	-0.048*** (0.011)
w/o degree	-0.136 (0.112)	-0.061 (0.139)	-0.183 (0.125)	-0.051 (0.151)	-0.110 (0.125)	-0.070 (0.129)	-0.077 (0.080)
income (log)	0.022 (0.018)	0.028 (0.018)	0.007 (0.019)	0.037 (0.019)	0.049* (0.020)	0.073*** (0.020)	0.058** (0.019)
work exp.	0.001 (0.001)	0.002 (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)
satisf. w/ HH-inc.	0.023*** (0.003)	0.017*** (0.004)	0.021*** (0.004)	0.021*** (0.004)	0.024*** (0.004)	0.016*** (0.004)	0.017*** (0.004)
intercept	0.324* (0.130)	0.301* (0.140)	0.528*** (0.143)	0.354* (0.148)	0.248 (0.156)	0.190 (0.158)	0.257 (0.149)
Period	2006-2008	2007-2009	2008-2010	2009-2011	2010-2012	2011-2013	2012-2014
N	2733	2456	2422	2361	2285	2230	2451
adj. R2	0.03	0.03	0.04	0.04	0.04	0.04	0.04
BIC	3661	3301	3250	3159	3041	2962	3309
Log.Lik.	-1783	-1604	-1578	-1533	-1474	-1435	-1608

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table D.6: Treatment effect (ATT) of the benefit promise on alignment with the pledge-making party over time (III)

	(15)	(16)	(17)	(18)	(19)
ATT	-0.082*** (0.024)	-0.060** (0.023)	-0.044 (0.025)	-0.029 (0.025)	-0.027 (0.025)
age	-0.010*** (0.001)	-0.009*** (0.001)	-0.007*** (0.001)	-0.008*** (0.001)	-0.007*** (0.001)
married	0.101*** (0.015)	0.122*** (0.015)	0.112*** (0.016)	0.123*** (0.015)	0.118*** (0.015)
# children	-0.060*** (0.009)	-0.055*** (0.009)	-0.042*** (0.009)	-0.023* (0.009)	-0.022* (0.009)
w/o degree	-0.098 (0.063)	0.035 (0.084)	-0.046 (0.073)	0.136 (0.097)	0.244** (0.082)
income (log)	0.058*** (0.016)	0.059*** (0.017)	0.062*** (0.017)	0.038* (0.017)	0.042** (0.015)
work exp.	-0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.002* (0.001)	0.001 (0.001)
satisf. w/ HH-inc.	0.011*** (0.003)	0.009** (0.003)	0.010** (0.003)	0.011*** (0.003)	0.007* (0.003)
intercept	0.364** (0.124)	0.316* (0.127)	0.162 (0.136)	0.278* (0.132)	0.263* (0.123)
Period	2013-2015	2014-2016	2015-2017	2016-2018	2017-2019
N	3290	3642	3329	3209	3204
adj. R2	0.04	0.04	0.04	0.03	0.03
BIC	4375	4785	4428	4262	4167
Log.Lik.	-2139	-2343	-2166	-2083	-2035

Note: The outcome variable is a binary variable indicating alignment with the pledge-making party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table D.7: Treatment effect (ATT) of the benefit promise on alignment with the green party over time (I)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.008 (0.024)	0.003 (0.022)	-0.017 (0.022)	-0.017 (0.021)	-0.006 (0.022)	0.006 (0.022)	0.005 (0.022)
age	0.022*** (0.002)	0.021*** (0.002)	0.019*** (0.002)	0.017*** (0.001)	0.018*** (0.002)	0.018*** (0.002)	0.017*** (0.002)
married	-0.138*** (0.017)	-0.155*** (0.016)	-0.146*** (0.016)	-0.110*** (0.016)	-0.088*** (0.016)	-0.081*** (0.016)	-0.087*** (0.016)
# children	0.017 (0.009)	0.033*** (0.009)	0.044*** (0.009)	0.043*** (0.009)	0.049*** (0.009)	0.041*** (0.010)	0.037*** (0.010)
w/o degree	-0.178*** (0.026)	-0.164*** (0.026)	-0.162*** (0.034)	-0.134*** (0.016)	-0.199*** (0.029)	-0.183*** (0.037)	-0.192*** (0.042)
income (log)	0.025 (0.016)	0.031* (0.015)	0.016 (0.013)	0.005 (0.012)	-0.006 (0.013)	0.000 (0.013)	0.012 (0.013)
work exp.	-0.013*** (0.001)	-0.011*** (0.001)	-0.007*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)
satisf. w/ HH-inc.	-0.008* (0.003)	-0.003 (0.003)	0.006* (0.003)	0.005 (0.003)	0.006 (0.003)	0.003 (0.003)	0.005 (0.003)
intercept	-0.518*** (0.121)	-0.661*** (0.112)	-0.579*** (0.098)	-0.469*** (0.094)	-0.444*** (0.100)	-0.495*** (0.100)	-0.584*** (0.101)
Period	1999-2001	2000-2002	2001-2003	2002-2004	2003-2005	2004-2006	2005-2007
N	2314	2748	3083	3252	3077	2934	2882
adj. R2	0.07	0.07	0.06	0.04	0.04	0.04	0.05
BIC	1856	2256	2688	3014	3127	2912	2925
Log.Lik.	-881	-1081	-1296	-1458	-1515	-1408	-1415

Note: The outcome variable is a binary variable indicating alignment with the green party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table D.8: Treatment effect (ATT) of the benefit promise on alignment with the green party over time (II)

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
ATT	0.014 (0.023)	-0.001 (0.024)	0.003 (0.024)	0.010 (0.026)	0.062* (0.027)	0.113*** (0.028)	0.123*** (0.025)
age	0.016*** (0.002)	0.016*** (0.002)	0.018*** (0.002)	0.021*** (0.002)	0.019*** (0.002)	0.017*** (0.002)	0.015*** (0.002)
married	-0.102*** (0.016)	-0.073*** (0.017)	-0.043*** (0.017)	-0.050** (0.017)	-0.059** (0.018)	-0.073*** (0.019)	-0.077*** (0.017)
# children	0.026** (0.009)	0.030** (0.010)	0.033*** (0.010)	0.050*** (0.011)	0.055*** (0.011)	0.055*** (0.012)	0.060*** (0.011)
w/o degree	-0.198*** (0.042)	-0.219*** (0.041)	-0.223*** (0.046)	-0.295*** (0.047)	-0.304*** (0.038)	-0.322*** (0.038)	-0.196*** (0.024)
income (log)	0.033* (0.013)	0.017 (0.014)	0.022 (0.014)	0.003 (0.015)	0.012 (0.016)	0.023 (0.017)	0.047** (0.015)
work exp.	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.004*** (0.001)	-0.003** (0.001)	-0.001 (0.001)
satisf. w/ HH-inc.	0.004 (0.003)	0.008* (0.003)	0.009** (0.003)	0.011*** (0.003)	0.009* (0.004)	0.007 (0.004)	0.003 (0.003)
intercept	-0.668*** (0.101)	-0.604*** (0.106)	-0.748*** (0.106)	-0.779*** (0.113)	-0.797*** (0.128)	-0.793*** (0.136)	-0.981*** (0.123)
Period	2006-2008	2007-2009	2008-2010	2009-2011	2010-2012	2011-2013	2012-2014
N	2733	2456	2422	2361	2285	2230	2451
adj. R2	0.05	0.05	0.06	0.07	0.06	0.05	0.05
BIC	2714	2545	2514	2639	2673	2764	2819
Log.Lik.	-1309	-1226	-1210	-1273	-1290	-1336	-1363

Note: The outcome variable is a binary variable indicating alignment with the green party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table D.9: Treatment effect (ATT) of the benefit promise on alignment with the green party over time (III)

	(15)	(16)	(17)	(18)	(19)
ATT	0.103*** (0.021)	0.059** (0.021)	0.029 (0.022)	0.054* (0.022)	0.073** (0.022)
age	0.016*** (0.001)	0.018*** (0.001)	0.016*** (0.001)	0.016*** (0.001)	0.018*** (0.001)
married	-0.065*** (0.014)	-0.070*** (0.013)	-0.067*** (0.013)	-0.072*** (0.013)	-0.072*** (0.014)
# children	0.068*** (0.009)	0.071*** (0.008)	0.072*** (0.008)	0.069*** (0.008)	0.083*** (0.009)
w/o degree	-0.153*** (0.019)	-0.188*** (0.026)	-0.134** (0.045)	-0.258*** (0.021)	-0.201*** (0.049)
income (log)	0.040** (0.013)	0.035** (0.012)	0.039** (0.012)	0.052*** (0.013)	0.043** (0.014)
work exp.	-0.001 (0.001)	-0.002** (0.001)	-0.002** (0.001)	-0.002* (0.001)	0.000 (0.001)
satisf. w/ HH-inc.	0.001 (0.003)	0.007* (0.003)	0.008** (0.003)	0.008** (0.003)	0.012*** (0.003)
intercept	-1.013*** (0.100)	-1.072*** (0.096)	-1.046*** (0.100)	-1.164*** (0.106)	-1.266*** (0.111)
Period	2013-2015	2014-2016	2015-2017	2016-2018	2017-2019
N	3290	3642	3329	3209	3204
adj. R2	0.06	0.06	0.05	0.05	0.06
BIC	3716	4170	3738	3666	3850
Log.Lik.	-1810	-2036	-1820	-1785	-1877

Note: The outcome variable is a binary variable indicating alignment with the green party. All estimations include the following set of control variables: age, binary for being married, number of children in household, binary for no educational degree, personal income, years of work experience, satisfaction with household income, as well as the running variable. The estimates are based on a parametric linear probability model without slope-equality restrictions (baseline specification). The columns differ with respect to the survey period they are estimated on. Robust standard errors in parenthesis. * p < 0.05, ** p < 0.01, *** p < 0.001.