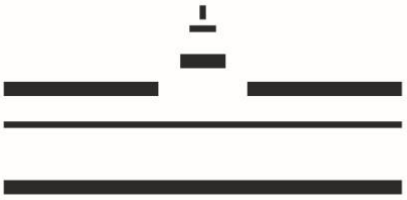

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The Impact of Election Information Shocks on Populist Party Preferences: Evidence from Germany

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The Impact of Election Information Shocks on Populist Party Preferences: Evidence from Germany

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Abstract

Despite controversial debates about the social acceptability of its nationalist program, the right-wing populist AfD has recently entered all state parliaments as well as the federal parliament in Germany. Although professed AfD voters faced a likely risk of social stigmatization, electoral support followed a clear upward trend. In order to explain these dynamics, we analyze the impact of information shocks with respect to aggregate-level AfD support on individual party choices. Unexpectedly high aggregate support for a populist party may indicate a higher social acceptance of its platform and reduce the social desirability bias in self-reported party preferences. Consequently, the likelihood to reveal an AfD preference increases. We test this mechanism in an event-study approach, exploiting quasi-random variation in survey interviews conducted closely around German state elections. We define election information shocks as deviations of actual AfD vote shares from pre-election polls and link these to the individual disposition to report an AfD preference in subsequent survey interviews. Our results suggest that exposure to higher-than expected AfD support significantly increases the individual probability to report an AfD vote intention by up to 3 percentage points.

JEL-Codes: C21, D71, D72, D83, D91
Keywords: Voting behavior, populist parties, contagion effects, information shocks, social desirability bias

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University of Münster
CIW – Center for Interdisciplinary Economics
Scharnhorststrasse 100
D-48151 Münster

phone: +49-251/83-25329 (Office)
e-Mail: clementine.kessler@uni-muenster.de
Internet: www.wiwi.uni-muenster.de/ciw

1 Introduction

Established party systems in both Europe and the U.S. have recently been rattled by the fast growing success of right-wing populist platforms in a number of elections and referendums, In Germany, the rise of the right-wing populist *Alternative for Germany* (*Alternative fuer Deutschland*, AfD) has gained momentum in a series of state elections before the party entered the federal parliament in 2017 with a vote share as high as 12.6 percent. The quick spread of right-wing populist advocacy throughout the electorate may be driven by social interdependencies in party choices. Recent research observes that the victory of Donald Trump triggered contagion effects in reported anti-immigrant attitudes both within and outside of the U.S. (Bursztyn et al., 2018; Giani and Méon, 2018). Similar effects have been found for other political movements like the Arab Spring protests or, more recently, climate movements (Barbera and Jackson, 2019; Kuran and Romero, 2019; Zeitzoff, 2017). The literature mentions two main motives for imitating one's fellow voters' behavior – cost savings in information accumulation (Bikhchandani et al., 1992, 1998) as well as reputational gains from social compliance (Coate and Conlin, 2004; Grossman and Helpman, 2001; Kuran, 1989; Morton, Muller, et al., 2015).

We analyze the existence of similar contagious dynamics in publicly stated support for the German AfD. We apply a quasi-experimental event-study design which exploits variation in vote intentions reported in surveys conducted closely around German state elections between 2013 and 2017. Individuals may falsify their true vote intention if they believe that their political attitude is not socially accepted. As a consequence, right-wing party preferences are systematically under-reported in face-to-face interviews (Creighton et al., 2018; DellaVigna et al., 2012; Hainmueller and Hangartner, 2013; Philipps and Claney, 1972). However, upon observation of larger aggregate support for originally controversial attitudes, the *social desirability bias* is expected to be reduced, encouraging AfD sympathizers to genuinely reveal their party preference. We capture new, unanticipated information about societal support levels by means of *election information shocks*, measured by the deviations of state election outcomes from pre-election polls. Our results provide systematic evidence that larger-than expected AfD vote shares in state elections raise subsequently reported AfD vote intentions among individuals living in *other* states by up to 3 percentage points.

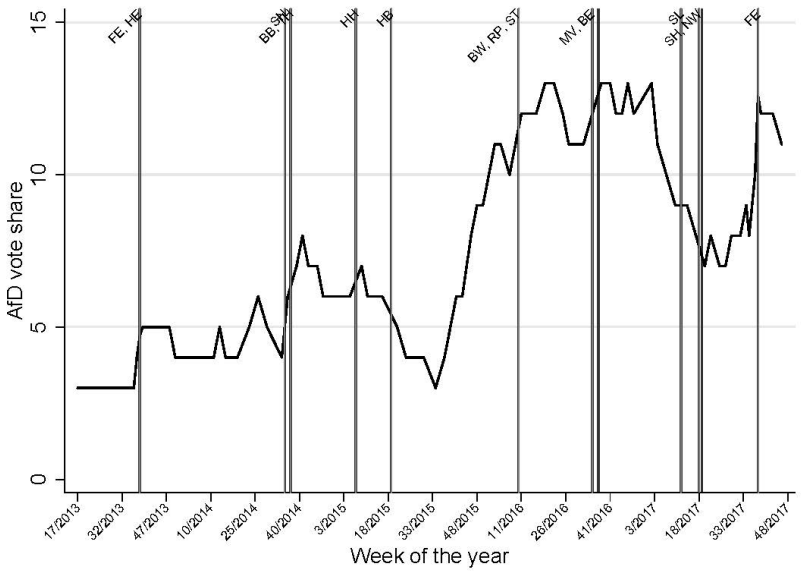
We make three key contributions to the literature. First, to the best of our knowledge, there is no study as of now which transfers the idea of interpersonal contagion to populist

voting. We provide a concise theoretical framework which illustrates how observed social preferences feedback on individual political behavior. Second, in order to empirically test this impact, we introduce and operationalize election information shocks as the vehicle of interpersonal information exchange. Third, we provide innovative evidence on a yet understudied driving force of right-wing populist voting. We thereby add to the growing body of social science literature explaining the rise of populist movements.

The German federal system in combination with the recent emergence of a far right-wing platform provides a unique and well-suited setup to analyze the effect of information shocks on individual attitudes. When founded in 2013, the AfD mainly promoted euro-scepticism as well as fiscal conservatism. Yet from 2015 onward, the party strongly shifted to the right, focusing almost exclusively on immigration with some outright xenophobic elements – a near-taboo in Germany. This programmatic transformation was electorally rewarded, especially after the peak of the European refugee crisis in 2015 (see figure 1). While voters were unsure where to place the AfD on a left-right scale in 2013, the narrow focus on immigration after 2015 has led to a public perception as a far-right party.[1] This impression has been reinforced by a respective media coverage (Cantoni et al., 2019; Salzborn, 2016). The programmatic shift implies a potential source of social stigmatization of professed AfD supporters. The AfD therefore constitutes an ideal example to test if and how shocks in aggregate support shape self-reported preferences.

For these self-reported vote intentions, we rely on individual-level data from the German *Politbarometer* survey. Importantly, we are interested in *reported* vote intentions as these are more likely subject to reputational concerns than secret ballots. Our dataset provides us a comprehensive sample of repeated cross-sections, covering vote intentions as well as politically relevant individual characteristics. To test the link between social and individual party preferences, we have to identify sudden events that reveal reliable information about shifts in aggregate preferences. Strikingly, opinion polls prior to the German state elections had pronounced difficulties in correctly predicting AfD vote shares. As reported in appendix figure B.2, pre-election polls based on interviews systematically underestimated the realized AfD vote share in all but two state elections held between 2013 and 2017 with deviations of up to 143 percent. Therefore, these deviations represent a suitable candidate to capture shocks in information about *actual* social acceptance of the AfD (Giani and Méon, 2018). We thus define an *election infor-*

[1] Figure B.1 in the appendix shows AfD ratings from the German Longitudinal Election Study.



Note: Federal-level AfD polls and dates of state elections in chronological order from 2013 to 2017. We exclude two state elections that do not fall into the respective election cycle, namely in Bavaria in 2013 and in Lower Saxony in 2017, as these were held shortly before and after the federal elections, respectively. HE = Hesse, SN = Saxony, BB = Brandenburg , TH = Thuringia , HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia, FE = Federal elections.

Figure 1: AfD vote shares and state elections.

mation shock as the deviation of the AfD vote share in a state election from the latest opinion poll at the federal level.

Election outcomes and individual preferences within the same constituency are obviously not independent, giving rise to the so-called *reflection problem* (Manski, 1993, 2000). Apart from purposely relate state-level election outcomes to *federal* vote intentions, we further address this issue by neglecting vote intentions of interviewees residing in the state where the election in question is held. We control for additional, potential confounders effects at the national or state level by exploiting variation in reported preferences in the two polls closest to the election date. We compare a treatment group of respondents interviewed in the first survey after the election to a control group interviewed right before the election. Within this narrow *election window*, structural factors can be assumed sufficiently constant. Applying entropy balancing and including state-of-living as well as election-window fixed effects, we show that our treatment effect is robust to several econometric specifications. In addition, we find no significant effects when applying placebo tests for comparable time windows without elections, suggesting

that our treatment effect is not driven by a general trend in AfD vote shares. Our estimation strategy benefits from the fact that state elections in Germany are staggered, providing us with multiple treatment events which are random with respect to the date of closely surrounding polls. This strengthens our supposition that the observed effect of election information shocks on self-reported individual attitudes is in fact causal.

The remainder of this paper is organized as follows. Section 2 provides an overview of the related literature and lays out our theoretical framework. Section 3 elaborates on our identification strategy and sample. Estimation results are presented in Section 4. Section 5 concludes.

2 Case selection and theoretical framework

2.1 The AfD as a right-wing populist party

The AfD shows some characteristics, both with regard to its political platform and its public behavior, which lead scholars to classify it as a populist right-wing party. Populism in general is characterized by a normative separation of society into two antagonistic groups, mostly the *common people* and a *political elite* (Golder, 2003; Mudde, 2004). The main feature of right-wing populist parties is their immigration-critical attitude. They strongly oppose immigration, focus on the preservation of national cultural values and commonly grant the native population a preferential access to jobs and public goods. In that light, disproportionate electoral support for right-wing populist parties is registered among voters who have been negatively affected by the socio-economic upheaval of global integration and demographic changes during the past decades (Dülmer and Klein, 2005; Funke et al., 2016; Morton and Ou, 2015; Oesch, 2008; Rooduijn and Akkerman, 2017; Rooduijn, de Lange, et al., 2014).

The AfD was originally founded in 2013 as a special-issue party opposing the EU's financial-support policies in the aftermath of the euro crisis. Therefore, typical right-wing and nationalist tendencies as well as populist rhetorics were rather unincisive (Arzheimer, 2015). However, the party underwent a fundamental transformation in the course of the so-called *refugee crisis* in 2014/15 when party leadership was demised to the nationalist wing. In the time following, the AfD emphasized its well-defined position against the immigration-friendly policies of the Merkel administration. Instead,

the party pressed for strictly regulating immigration, especially from muslim countries. The AfD has drawn near to other European parties with a similarly pronounced anti-immigration platform, the then-called *Front National* in France, the Dutch *PVV* or the Austrian *FPÖ*. Some AfD members have even been linked to right-wing extremist organizations (Berbuir et al., 2015; Salzborn, 2016).

Various party officials have caused controversies by joining rallies organized by anti-immigration movements[2] or using xenophobic language in public statements. Party leader Alexander Gauland sparked a severe debate as he advanced his opinion that then-Minister of State Aydan Özuguz, who was born in Germany to Turkish parents, should be "disposed in Anatolia". Gauland formulated this statement as a reaction to Özuguz's public claim that an essentially German culture was "hardly identifiable". He was also among the AfD members casting doubts about Germany's responsibility to preserve the memory of victims murdered during the Nazi era. As to Gauland, "Hitler and the Nazis are just a bird dropping in a thousand years of successful German history". Similar statements were made by party member Björn Höcke, who is usually considered to be the leading figure of the extreme-right wing of the AfD. Höcke attracted massive public criticism when referring to the Holocaust memorial in Berlin as a "memorial of shame in the heart of the German capital".

Given these public statements, it does not come as a surprise that the legitimacy of openly supporting the AfD has been controversially discussed. The party is often stigmatized as anti-democratic or unelectable (Berbuir et al., 2015). What is more, due to its specific national history, Germany's political landscape reacts very sensitively to movements featuring nationalist, xenophobic or even racist attitudes (Cantoni et al., 2019; Mudde, 2004; Rydgren, 2005). Openly sympathizing with these movements is likely associated with social proscription. In economic terms, the social cost of being a professed AfD supporter could be prohibitively high. The party's shift with regard to program components made it even harder for the voting population to evaluate the political agenda of the AfD. In order to correctly assess its social acceptability, voters were in permanent need of information about the actual AfD reputation among the electorate.

Despite the above-mentioned boundary crossings, AfD election results followed a pronounced upward trend. In the 2013 general election, the AfD just failed to pass the

[2] Such as the far-right, islamophobic PEGIDA movement which originated in East Germany in 2014. The acronym PEGIDA translates as *Patriotic Europeans against the Islamization of the Occident.*

threshold of 5 percent of valid votes in order to enter the federal parliament. The year after, the AfD achieved great successes in the state elections in Brandenburg, Saxonia and Thuringia and entered all three state parliaments with vote shares of roughly 10 percent and higher.[3] Until 2017, the party had step by step won seats in 14 out of 16 German state parliaments. It finally entered the federal parliament in 2017 with a vote share of 12.6 percent where it is now the largest opposition party. Given this pattern, the steadily growing AfD electorate could, partly, be explained by a self-reinforcing process of intersubjective contagion.

2.2 Interdependencies of individual political behaviors

Yet, what reason should individuals have to comply with their fellow voters' behavior? The related literature can be condensed to identify two main explanations. First, as set out in the essential economic theory of voting, collecting encompassing information about the quality of political contestors involves prohibitively high costs (Downs, 1957b). Yet, these can dramatically be reduced by simply adopting other individuals' choices, assuming that they collected and correctly evaluated the necessary information on political competitors. In this sense, voters side with a party for which they observe a high level of aggregate support from earlier decision makers, yielding to an *informational cascade*. Support decisions only build upon the *presumption* that other voters attest a sufficient quality to a political party, not on a party's actual aptitude for political offices. Consequently, even low-quality parties may receive high vote shares due to behavioral imitation (Bikhchandani et al., 1992, 1998; Cao et al., 2011).

Information costs are virtually zero in times of digital information flows so that the second explanation for behavioral compliance is of higher relevance in our setting. Voters may want to reveal a certain party preference in order to receive utility from being publicly known as a professed supporter. The amount of this *reputational utility* to be gained positively depends on the social standing of the party which support is expressed for. Following the argumentation of Granovetter (1978) and Kuran (1987, 1989), individuals adopt a socially observable behavior once the number of fellow citizens exhibiting this behavior exceeds an individual *threshold level*. Beyond this, the individual utility from being in line with society exceeds his or her instrumental utility from the

[3] Official election results at the federal and the state level are obtained from the German Federal Statistical Office

behavioral outcome. Consequently, individuals would even be willing to falsify their true preferences to avoid social rejection by the majority (Granovetter, 1978; Kaempfer and Lowenberg, 1992; Kuran, 1989; Morton and Ou, 2015). Similarly, elections are subject to the so-called *bandwagon effect*. In order not to "waste" their vote for an underperforming party, which can thus be considered unpopular with the majority, individuals decide to vote for the likely winner of an election (Dahlgaard et al., 2016; Klor and Winter, 2018; Mehrabian, 1998; Obermaier et al., 2017).

The argumentation can intuitively be transferred to the issue of right-wing populist voting. Low societal support for a party with controversial positions is associated with a likewise low social reputation. Professed supporters are thus likely subject to social rejection. The more fellow voters stick with the mainstream parties, the more inclined are AfD sympathizers to conceal their true preference. However, growing AfD support signals a change in public perception of the party's electability. A larger supporter group serves as a protective cloak under which individuals can freely voice their advocacy at a lower risk of reputation losses. Consequently, the individual readiness to openly express an AfD preference increases.

Empirical studies find corroborating evidence of social compliance driving political behavior in various settings. Nickerson (2008) observes individuals whose partner was subject to a campaign designed to increase voter turnout to be more likely to participate in local elections. Not only was the campaign successful with regard to the target person, the effect extends to their partner due to behavioral imitation. The literature on *spousal voting* and *family voting* has long been providing corresponding evidence on close individuals influencing each other's turnout decisions and party choices (Glaser, 1959; Huckfeld and Sprague, 1991; Straits, 1990). McClurg (2004) reasons this behavioral convergence with frequent personal exchange of information as well as a high level of mutual trust within these relationships.

Apart from personal interaction, behavioral interdependencies are also observed at an aggregate level. Voters may use opinion polls or election results at other administrative levels to learn about general societal preferences. Related empirical studies find election polls to influence individual voting decisions by stimulating the bandwagon effect. Bandwagoning is observed with respect to expressing sympathy for a candidate (Dahlgaard et al., 2016), actual election choices in real-world or experimental settings (Khalil et al., 2019; Mehrabian, 1998; Morton, Muller, et al., 2015; Obermaier et al., 2017) as well as

turning out to cast a ballot for the party leading the polls (Bursztyn et al., 2018; Klor and Winter, 2018).

Considering the fact that behavioral spillovers without face-to-face communication exist, an important mediator of voting contagion is media consumption. Although Dahlgaard et al. (2016) state that "it is almost impossible to avoid polls", Faas et al. (2008) emphasize that information generated by polls can only influence voting decisions if individuals actually receive this information. In addition, Durante, Pinotti, et al. (2019), Durante and Knight (2012) as well as Dewenter et al. (2018) show for several settings that biase media reports actually affect political behavior and vote intentions. In that sense, a positive presentation of populist leaders promotes sympathy among the electorate (Bos et al., 2011; Durante, Pinotti, et al., 2019; Lubbers et al., 2002; van der Brug et al., 2000). In line with this, Boomgarden and Vliegenthart (2009) observe support for nationalist policies to increase if media outlets refer to immigration issues in a negative way. More recent studies also examine a similar effect of media coverage on immigration worries (Benesch et al., 2019). Consequently, media coverage matters as one way of disseminating biased or unbiased information about societal party preferences.

2.3 Theoretical mechanism

To structure the above-discussed mechanism by which we expect election information shocks to affect individual party support, we present a simple formalization based on Bikhchandani et al. (1992, 1998) and Kuran (1987, 1989) as well as Lindbeck and Weibull (1987).

Let $i = 1, 2, ...N$ be an eligible voter in a spatial unit r. It is $r \in R$ where R denotes the total of spatial units all of which are administratively linked at a higher level (e. g. federal states or provinces). Staggered and periodical elections are held at every lower level r as well as at the higher level. There is a number $J \geq 2$ of political parties, which compete for electoral support from a number of voters n_r. Each voter i is allowed to cast one ballot in elections held at the national level as well as one ballot in elections held in his region of living r but not in any other region $-r$. Election results in r are observed by all voters in another spatial unit $-r$.

Taking into account the earlier economic theory of voting (Brennan and Lomasky, 1993; Downs, 1957a; Riker and Ordeshook, 1968), we model individual utility from publicly

stating support for a certain party as being purely expressive.[4] Consequently, the only motivation for voters to reveal their political preferences is to gain reputational utility among their peers.

Generally, an individual supports the party whose promoted canon of values is closest to her own personal values (Kuran, 1989). Denote by $p_{i,r}$ a vector of fixed political views of individual i living in region r. Among these are opinions on concrete policy measures as well as attitudes towards more broadly defined issues such as immigration or nationalism. We model $p_{i,r}$ as being dependent on a vector of individual characteristics $x_{i,r}$ such as age, gender, employment status, and the like. Let p^j denote a respective vector of political values represented by party j. The function $d^j_{i,r}\Big(p_{i,r}(x_{i,r}), p^j\Big) \geq 0$ then returns the utility from proximity in values between i and j, By definition, $d^j_{i,r}$ takes up its maximum if $p_{i,r}$ and p^j exactly coincide and equals 0 if $p_{i,r}$ and p^j do not share a single common value. A single individual cannot impact on a party platform p^j, so that $d^j_{i,r}$ is treated as a random variable.

When making party choices, i weighs this proximity in values by the party's standing in society. Party j may promote socially controversial elements such as nationalism or authoritarianism. Based on these components, j is attributed a certain level of social acceptance. Denote by $a^j_{i,r} \in [0,1]$ the social acceptance of party j as perceived by individual i with 0 denoting no social acceptance and 1 denoting the maximum level of acceptance. Let $a^j_{i,r}$ be a concave function of j's current average support level v^j_R over all regions. Consequently, the larger is observable support among the electorate, the higher is i's perception of j' acceptance (Kuran, 1987, 1989).

Overall utility of individual i from openly supporting j then reads

$$U^j_{i,r} = d^j_{i,r}\Big(p_{i,r}(x_{i,r}), p^j\Big) \cdot a^j_{i,r}(v^j_R, p^j). \tag{1}$$

Note that a lack of social acceptance results in an overall utility level of 0. The explanation is straightforward. Even if individual i considers party j to perfectly represent his or her own vector of political values, j's lack of social acceptability will prevent i from openly expressing that support in order to avoid reputational losses. Since $a^j_{i,r} \in [0,1]$, it can be considered the probability with which i reveals her ideological closeness to j

[4] This assumption implies that the individual impact on the election outcome is irrelevant so that we can neglect any instrumental utility from political participation.

based on concerns of social rejection. In that sense, $1 - a^j_{i,r}$ can be considered the size of the social desirability bias (Kuran, 1987, 1989).

An individual now compares the utilities to be gained from supporting any party $j = 1, ..., J$ to choose the option which promises the highest utility. Consider the simple case of the political system comprising two parties, J and $-J$. Platforms of both parties are identical with the exception of nationalism so that p^j collapses to a single value denoting the extent of nationalist policies favored by party j. The established party $-J$ promotes multiculturalism and openness, so that $p^{-J} = 0$ while the newly emerging party J promotes policies strongly characterized by nationalism and national isolation, $p^J = 1$. Individual i decides to support J iff utility from doing so is greater than utility from supporting $-J$[5], that is iff

$$U^J_{i,r} > U^{-J}_{i,r}. \tag{2}$$

Because $U^j_{i,r}$ is a function of the random variable $d^j_{i,r}$, the utility differential $\Delta_{i,r} = U^J_{i,r} - U^{-J}_{i,r}$ is itself a random variable. Let $F_{i,r}$ denote the twice continuously differentiable cumulative distribution function[6] of $\Delta_{i,r}$ (Lindbeck and Weibull, 1987). Then, the probability that i expresses a preference for party J is

$$Prob_{i,r}[U^J_{i,r} - U^{-J}_{i,r} > 0] = F_{i,r}\Big(\Delta_{i,r}\Big) \tag{3}$$

The impact of an election information shock can now be illustrated as follows. We start from an equilibrium case in which an individual harbors a certain ideological preference for nationalist policies, i. e. $p_{i,r} > 0$. Yet, the observable aggregate support v^J_R for party J is relatively low and renders the individual indifferent between the parties,

$$U^J_{i,r}(v^J_R) = U^{-J}_{i,r}(1 - v^J_R). \tag{4}$$

With v^J_R denoting the aggregate support share for party J, $1 - v^J_R$ denotes the respective support share for $-J$. If now an election is held in any region $-r$, i's information about v^J_R is updated. Suppose that $\sigma^J_{-r} > 0$ captures the shock in revealed political preferences for the populists, in terms of a positive deviation[7] of election results in $-r$ from the

[5] We assume that indifference between a new and an established party entails support for the established party as a security option. [6] With $f_{i,r}$ being the corresponding density function which is centered around 0. [7] All calculations can also be shown for $\sigma^J_{-r} < 0$.

earlier observed general support level v_R^J. Then, $v_R^{J'}(\sigma_{-r}^J > 0) > v_R^J$ captures the new aggregate support level for party J and Equation (4) changes to

$$U_{i,r}^J(v_R^{J'}) > U_{i,r}^{-J}(1 - v_R^{J'}) \Leftrightarrow F(\Delta_{i,r}^{'}) > F(\Delta_{i,r}) \tag{5}$$

so that i now openly supports the populist party J. Individuals who already harbored a certain preference for J but did not dare to state their opinion due to J's previously low social acceptance now side with J (Kuran, 1987, 1989).

Taking the first derivative of $F_{i,r}(\Delta_{i,r}^{'})$ with respect to the election information shock σ_{-r}^J yields

$$\frac{\partial F_{i,r}(\Delta_{i,r}^{'})}{\partial \sigma_{-r}^J} = f_{i,r}(\cdot)\Big[d_{i,r}^J \cdot \frac{\partial a_{i,r}^J}{\partial v_R^{J'}} \frac{\partial v_R^{J'}}{\partial \sigma_{-r}^J} - d_{i,r}^{-J} \cdot \frac{\partial a_{i,r}^{-J}}{\partial v_R^{J'}} \frac{\partial v_R^{J'}}{\partial \sigma_{-r}^J} \cdot (-1)\Big] > 0$$

Thus, an election information shock with respect to support for the populist party J is positively related to the individual likelihood to express a respective preference. We can use these theoretical considerations to derive the basic estimation problem for the following empirical analysis. Let $Y_{i,r}^J$ denote i's answer to the survey question whether he or she intends to cast a ballot for the populist party J. $Y_{i,r}^J$ equals 1 if i reports a populist vote intention and 0 otherwise. To empirically determine the probability of being a populist supporter given an election observation shock σ_{-r}^J, we estimate

$$Pr(Y_{i,r}^J = 1 | \sigma_{-r}^J, x_{i,r}) = F\Big[\Delta_{i,r}(\sigma_{-r}^J, x_{i,r})\Big]. \tag{6}$$

3 Empirical approach and data

3.1 Database

We study the period between the two most recent federal elections in Germany, held on September 22, 2013 and on September 24, 2017. At the state level, 14 out of 16 elections were held during this regular election cycle.[8] Data on individual vote intentions comes

[8] Two states, Bavaria and Lower Saxony, held elections before or after the federal election cycle. In Bavaria, elections were on September 15, 2013; in Lower Saxony, elections were on January 20, 2013 and October 15, 2017.

from the *Politbarometer* surveys. To obtain the data, the *Forschungsgruppe Wahlen* (Election Research Group) conducts telephone interviews in intervals of two to three weeks with approximately 1,250 respondents per survey round. The survey team applies a rigorous sample selection strategy based on randomly generated household phone numbers and their members' birthdays. Interviews are conducted between Tuesday and Thursday of the respective survey week. The survey dates are usually fixed at the beginning of the year but their frequency may be increased around important events. In the run-up to federal elections, interviews are conducted on a weekly basis. Applying sample weights, each survey contains a sample of individuals which is representative for the eligible voting population.

The questions cover different topics concerned with current political issues but a core set of questions is asked in every round. Foremost, respondents are asked about their readiness to participate in an upcoming, hypothetical federal election and about their voting intention (the so-called *Sunday Question*). The exact wording of the question is: "If there were federal elections next Sunday, which party would you vote for?" We use the answer to this question to construct our dependent variable capturing the self-reported individual vote intention. In addition, the surveys document a wide range of demographic and socio-economic characteristics as well as specific political attitudes, e. g. the self-reported position on a left-right scale.

What is important with regard to our research design is the fact that the Politbarometer surveys are not used to collect information on political sentiments with regard to state-level elections. For this purpose, separate surveys around the election in question are performed in the respective state. Due to this separation, we can assume that the interview date of a given individual in the *Politbarometer* is random with respect to state-level election dates.

Even though the sample drawn in each survey round is representative, aggregates of vote intentions reported in the interviews do not necessarily match actual election outcomes. Deviations relate to short-run events and political shocks, indecisiveness of voters or wittingly or unwittingly false statements. Regarding the latter, this links back to the social desirability problem in personal interactions. Therefore, eventually published polls are calculated from the raw interview data using a predictive model which includes additional assumptions about voting persistence and macro-economic fundamentals and trends. Importantly, in our analysis, we use the raw interview data to measure individual vote intentions. Throughout the paper, we use the term *survey* to refer to the raw

interview data collected. In contrast, we use the term federal-level *poll* when we refer to the estimate published based on the last national survey conducted before the state election in question. In addition, we refer to state-level *polls* as the youngest available forecast based on a state-specific survey which is usually published two to three days before the election in question.

3.2 Empirical specification and sample

To test the effect of state-level elections on self-reported vote intentions, we use an event-study design with quasi-randomized treatment and a repeated cross-section of surveyed individuals. Our empirical approach closely relates to strategies recently applied in Depetris-Chauvin et al. (2018), Giani and Méon (2018), and Mikulaschek et al. (2019). Treatment is defined as the individual exposure to an election information shock, which is the deviation of a state-level election result from previously reported polls. We aim to estimate the short-run effect of information shocks in one state on self-reported federal vote intentions of individuals residing in other states. Our estimation model reads as follows:

$$Y_{i,r,t,e} = \beta\sigma_{t,e} + \Gamma' X_i + \gamma_e + \theta_r + \varepsilon_{i,r,t,e}, \tag{7}$$

where i denotes the individual, t denotes the week of survey, r denotes the state of residence and e refers to the election. $\varepsilon_{i,r,t,e}$ is an individual-level error term. Our dependent variable is a binary indicator that equals 1 if an individual reports an AfD vote intention in the Sunday Question and is 0 if the individual reports a vote intention different from the AfD, no vote intention or not to know their vote intention.[9]

$\sigma_{t,e}$ captures our treatment, the exposure to an election information shock. If a respondent i is interviewed in the first survey after a state election e, then $\sigma_{t,e} = 1$. If a respondent i is interviewed in the last survey before the election day, then $\sigma_{t,e} = 0$. Our key parameter of interest is thus β which captures the average difference in the individual likelihood to report an AfD preference right after a state election compared to shortly before the election.

The survey data contains information about the calendar *week of survey* (WoS) for each individual, also referred to as the survey round. We match this information with the

[9] We deem the latter two manifestations to be of special importance to capture a mobilizing effect.

calendar *week of election* (WoE). Following Mitra et al. (2017), we define for each election e an *election window* (δ_e) which identifies the period in which the surveys closest to the WoE are conducted. δ_e is given by

$$\delta_e = [\underline{\delta_e}, \overline{\delta_e}] \; with \begin{cases} \overline{\delta_e} = min\{t\}_{t>t_e}, & WoE_e = t_e \\ \underline{\delta_e} = max\{t\}_{t<t_e}, & WoE_e = t_e. \end{cases}$$

The upper boundary $\overline{\delta_e}$ refers to the first survey after the election, constituting the treatment group. $\underline{\delta_e}$ refers to the closest survey before the election in which the respective control group is interviewed.

When defining the groups, a few points merit careful attention. First, we test the effect of a state election in r only for respondents in all other states $-r$. We hereby tackle what Manski (1993) calls the "reflection problem". It is *a priori* not clear whether the observed AfD vote share impinges upon individual preferences within the same area or whether the average vote share is simply the aggregation of all individual preferences in this area. Hence, our treatment will not be exogenous when including respondents of the same state. In contrast, we can assume that election outcomes in one state are not driven by vote intentions reported in surveys closely before and after an election in *other* states, after controlling for election-window fixed effects.

Second, each individual is assigned to only one election window, either to the control or treatment group. For our identification strategy to be valid, we need to ensure that treated individuals are affected by the election information shock in question whereas individuals in the control group are unaffected, at least in the short run. Two or more elections may take place on the same day or follow each other too quickly so that election windows overlap. In Schleswig-Holstein and North Rhine-Westphalia, for instance, elections took place on two subsequent Sundays (May 7 and May 14, 2017), corresponding to calender weeks 18 and 19. Survey data is available for calender weeks 17 and 20, narrowly enclosing these elections. Therefore, we treat these elections as one election window. Treated individuals here are exposed to both elections while non-treated individuals are interviewed before the first election.

Furthermore, election windows may overlap such that the treatment group for the first election would at the same time be the control group of the second. This is the case for Saxony (election held on August 31, 2014) and for Brandenburg and Thuringia (elections

held on September 14, 2014). We therefore drop the latter elections from our sample because the control group may be affected by the earlier election in the neighboring state of Saxony.[10]

In a final step, we identify other potentially confounding events within our election windows, ensuring that our treatment effect is only driven by state elections. To that end, we also control for elections held at other administrative levels (e. g. federal elections, European elections). These procedures leave us with seven election windows that cover a total of eleven single elections and a sample of 20,525 individual-level observations. Individuals interviewed in weeks which do not fall into any election window are dropped. An overview of the WoE and WoS for each election window is provided in table 1.

Table 1: Election windows.

State	**Election date**	**AfD vote share**	**Pre-election poll**	**Type of shock**	**WoE**	**WoS control group**	**WoS treatment group**
SN	Aug 31, 2014	9.7	4.0	+	35	34	36
HH	Feb 15, 2015	6.1	6.0	+	7	5	9
HB	May 10, 2015	5.5	6.0	-	19	16	21
BW	Mar 13, 2016	15.1	10.0	+	10	7	11
RP		12.6	10.0	+			
ST		24.3	10.0	+			
MV	Sep 4, 2016	20.8	11.0	+	35	32	38
BE	Sep 18, 2016	14.2	11.0	+	37		
SL	Mar 26, 2017	6.2	9.0	-	12	10	14
SH	May 7, 2017	5.9	8.0	-	18	17	20
NW	May 14, 2017	7.4	8.0	-	19		

Note: Pre-election poll refers to the last federal-level poll before the indicated election date. SN = Saxony, HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia.

To calculate the election information shocks, two different strategies are basically applicable. First, state-level election outcomes can be compared to the last pre-election polls for this specific election at the *state level*, as depicted in figure B.2a. Second, state-level election outcomes can likewise be related to the last *federal-level* AfD poll before the election in question, as reported in figure B.2b. We prefer the second strategy because

[10] To ensure that we do not overestimate the treatment effect for Saxony, we additionally drop all individuals residing in Brandenburg and Thuringia from the treatment group in this election window.

relating election outcomes to state-specific polls comes with some significant drawbacks. This approach presupposes that state polls are available and consumed by all individuals in the treatment group. While state-election *outcomes*, especially those that stand out relative to the national average, are usually intensively covered by major media outlets, public interest outside the state in question is usually low in the run-up to the election. One may therefore question whether individuals residing in other states actually consume these state-specific election polls. In fact, appendix figure B.3 supports these doubts. It displays relative frequencies of Google searches of the respective state name in the four weeks around the election, showing sharp peaks just on election day. Hence, state-level politics are not very salient before the election, at least in other states. Individuals from other states are supposedly more likely to compare the information from state elections to general AfD support rather than to state-level polls.[11] Also, German state elections have been observed as *barometer elections* which may reflect general changes in voter attitudes and preferences (Anderson and Ward, 1996; Jeffery and Hough, 2001, 2003), even at the federal level, supporting our approach.

In order to identify an unbiased estimate $\hat{\beta}$ of the treatment effect, three critical assumptions have to hold. First, respondents in the treatment and control group should not systematically differ with respect to individual-level characteristics. Therefore, we include a vector of individual-level controls, X_i, among which are demographics (gender, age, marital status and highest level of education) and socio-economics (employment status).[12] We also include a vector of political attitudes because party preferences are likely correlated with other political attitudes. These comprise one's party choice in the last federal election, self-placement on the left-right scale, satisfaction with the government and the importance of the immigration issue.

Since the survey rounds of the Politbarometer are designed to be representative for the voting-age population, the distribution of covariates in the treatment and control group is generally very similar as is confirmed in a number of covariate balance tests (see appendix table B.1). However, there may be slight but critical differences affecting the distribution of political attitudes. Therefore, we follow Hainmueller (2012) and apply the *entropy balancing* approach. This matching technique attributes weighs to the control

[11] Yet, in a robustness test, we show that our results do not change when applying different strategies to capture the election information shock.

[12] The respective status groups are included as a set of dummies with full-time employment representing the reference group. Apart from that, the respondent can indicate to be in school, part-time employed, marginally employed, unemployed, in vocational training or retired.

units such that the distribution of covariates matches the respective distribution in the treatment group with regard to the first three moments. In addition, we apply an even more demanding balancing strategy by constructing entropy weighing schemes at the election-window level. Weights are applied such that a treated individual in election window δ_e is compared to control individuals from the *same* δ_e, matching on the state of living as well as the demographic and socio-economic covariates. This strategy allows us to control for a large fraction of unobserved heterogeneity driven by socio-economic characteristics, state-specific voting patterns, and general trends in AfD support. Finally, we could also match on political attitudes although these variables are potentially affected by the treatment. More precisely, if unexpected information about the AfD's electoral success reduces the associated social desirability bias, the individual likelihood to reveal other political attitudes such as a previous ballot cast for the AfD may increase as well. We therefore employ this approach with caution and leave political attitudes out of our baseline matching strategy. After this pre-processing, covariate imbalance between control and treatment groups becomes negligible.[13]

A second important assumption for identification is that state-level elections actually disseminate novel and unexpected information about the aggregate level of AfD support, which was *not* anticipated by individuals in the control group. As motivated above, the specific case of the AfD is well-suited to identify such effects because of the young but controversial party history combined with pronounced uncertainty about the true aggregate support level. Yet, if respondents in the control group anticipated state-level election results, our estimate will be biased downwards. Referring to the above-mentioned lack of interest in state-specific political discussion before the election, we feel confident that respondents in other states were unable to anticipate election outcomes. In the unlikely case that such an anticipation existed, we interpret our results as conservative estimates of the true treatment effect.

Finally, a last crucial assumption for identification is that there are no other unobserved events or macro-level trends that coincide with state-level elections. We include election-window (γ_e) fixed effects to account for the possibility that respondents in different election windows were exposed to unobserved time-variant macro-level effects, such as general differences in political demand across the electoral cycle. We also include state-of-living (θ_r) fixed effects to control for unobserved heterogeneity in voting patterns across regions. To assess the reliability of our standard error estimates, we cluster $\varepsilon_{i,r,t,e}$

[13] In the appendix, we also present covariate imbalance statistics for each election window separately.

at different levels of aggregation and compare clustered and bootstrapped standard error estimates.

During our analysis, we split the sample, distinguishing between positive and negative election information shocks. As outlined in section 2.3, we suppose that a higher-than expected vote share increases the propensity to report a respective party preference while a lower-than expected vote share should reduce it. Sub-sample analyses allow us to test these effects separately. As reported in table 1, state-level vote shares positively deviated from federal polls in Saxony, Brandenburg, Thuringia, Hamburg, Baden-Wuerttemberg, Rhineland-Palatinate, Saxony-Anhalt, Mecklenburg-Hither Pomerania and Berlin. These elections comprise our sub-sample of *positive* election information shocks. In contrast, state-level vote shares negatively deviated from federal polls in Bremen, Schleswig-Holstein, North Rhine-Westphalia and Saarland, constituting the sub-sample of *negative* election information shocks.

4 Results

We present our baseline results in table 2. In panel A, we include all election windows that were exposed to an election information shock. In panel B, we only include election windows with positive election information shocks, while in panel C we only examine election windows with negative shocks. We perform step-by-step regressions, reporting OLS coefficients in models (1) through (5). In model (1), we only include state-of-living and election-window fixed effects. In model (2), we add demographic covariates, while in model (3) we also include the individual employment status. Since party preferences are likely to be correlated with other political attitudes and may differ across our treatment and control group, we include respective indicator variables in model (4). In model (5), we apply the entropy balancing method, matching on the demographic and socio-economic covariates as well as the state of living. Since our dependent variable is binary, we provide average marginal effects of a logit estimation in column (6).

Since our model errors are likely correlated across individuals, we use three different types of cluster-robust standard errors to assess statistical inference. In particular, we suspect within-cluster correlation at the level of the survey round because treatment is assigned at this level, and at the regional level because existing evidence suggests profound differences in voting patterns across German states. Since the appropriate way

of clustering in our case is not a priori clear, we follow Cameron and Miller (2015) and cluster at progressively higher levels. For our baseline estimates, we cluster the standard errors at the intersection of an individual's state of living and the survey round. Cluster-robust standard errors at this level are reported in square brackets in table 2, with the respective p-values reported below. Inspecting our results based on these estimates in Panel A, we find a significantly higher likelihood for the treatment group to report an AfD vote intention after a state-level election.

Yet, our standard error estimates may neglect substantial correlation of observations within a survey round (across regions) and within a region (across time) that could lead to over-rejection of the true null hypotheses (Cameron and Miller, 2015). Therefore, we also separately cluster by state of living and survey round. However, since the number of clusters shrinks with an increasing level of aggregation (with 16 regions and 14 survey rounds), reliable inference becomes more challenging. This is particularly the case if the inference parameter is the coefficient of a treatment dummy variable, the treatment is assigned at the cluster level and there are only few treated clusters (MacKinnon, 2019). In such cases, using clustered standard errors could still lead to over- or under-rejection of the true null hypothesis (Cameron and Miller, 2015). To approach this issue, we apply the so-called *wild cluster restricted bootstrap* (WCRB) procedure to estimate our standard errors when clustering at higher aggregation levels, using the *boottest*-command in STATA (Roodman et al., 2019).

An accumulating body of literature suggests that the wild bootstrap procedures provide valid inferences under many circumstances typical to applied research, even when the number of clusters is small (Cameron, Gelbach, et al., 2008; Cameron and Miller, 2015). MacKinnon and Webb (2017, 2018) demonstrate that the wild cluster bootstrap with the null hypothesis imposed performs reasonably well when the number of treated clusters is not too small relative to the total number of clusters, and works "extremely well" for seven or more clusters. These conditions are fulfilled in our case. Out of 14 survey rounds, seven rounds or 50 percent are treated. Additionally, Cameron and Miller (2015) show that with few clusters, inference can be improved by applying the 6-point distribution proposed by Webb (2013) instead of the common 2-point Rademacher distribution. MacKinnon and Webb (2017) also show that the restricted bootstrap procedure leads to more conservative p-values than the unrestricted approach and that it tends to moderately under-reject. Thus, the wild cluster restricted bootstrap should be the most

reliable and conservative approach in our case.[14] In table 2, we report the p-values for the null hypothesis that $\beta = 0$ when the WCRB standard errors are clustered by state of living and by survey round, respectively. Statistical inference in Panel A becomes less clear when we account for potential within-cluster correlation at higher levels of aggregation. Clustering at the level of treatment, i.e. the survey round, should be the most conservative approach to cluster our model errors. When accounting for within-survey correlation, however, we do not find a robust treatment effect of state elections on reported AfD vote intentions.[15]

Yet, in Panel A, the estimated effects may hide treatment heterogeneity with respect to the information revealed by different state elections. In fact, when looking at the sub-sample of state elections with positive deviations of the AfD vote share from the federal support level, we find a larger and positive treatment effect that is highly significant across specifications and levels of clustering. The estimates in Panel B of table 2 indicate that the likelihood to report an AfD vote intention when interviewed shortly after a positive election information shock increases by 2 to 2.7 percentage points compared to respondents interviewed shortly before the election. In our preferred specification in column (5), which includes the full set of fixed effects and individual-level controls and applies entropy balancing weights, we find that being exposed to a higher-than expected AfD election outcome increases the propensity to report an AfD vote intention by about 2.1 percentage points. This is a sizable effect, corresponding to an increase in publicly expressed AfD support of about 36 percent after a state election compared to the sample average probability to report an AfD preference, which is 5.8 percent.

[14] See MacKinnon (2019) for a critical discussion of the wild bootstrap methods and alternative recent developments to approach the problem of few clusters. In the online appendix, we compare the WCRB estimates with alternative approaches that are recommended in the case of few clusters, i.e. unrestricted wild cluster bootstrap or ordinary wild bootstrap. The results show that the WCRB estimates always lie within the range of different estimates.

[15] In the online appendix, we show that the lack of significance is not driven by the mentioned tendency of the WCRB procedure to under-reject the true null hypothesis (MacKinnon and Webb, 2017, 2018). To this end, we use the fact that the restricted version of the wild cluster bootstrap tends to under-reject the null while the unrestricted version can lead to over-rejection. We report the p-values of both estimates as an upper and lower bound of the WCB estimate and show that there is only little disagreement among the two methods.

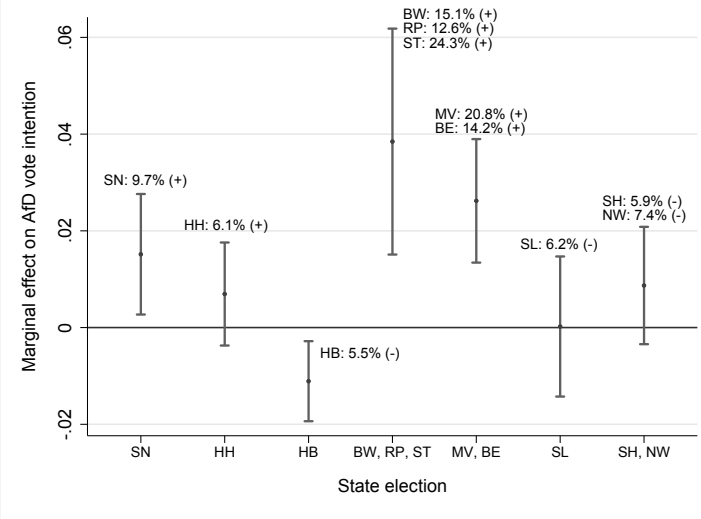
Table 2: Election information shocks and self-reported voting intentions: Baseline results.

DV: AfD vote intention	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	Logit (6)
Panel A: Pooled sample						
Post-election	0.0125	0.0134	0.0134	0.0129	0.00962	0.0153
	[0.00408]	[0.00410]	[0.00409]	[0.00411]	[0.00353]	[0.00382]
Cluster-robust SE, *p-value*	0.003***	0.001***	0.001***	0.002***	0.007***	0.000***
WCRB by state of living, *p-value*	0.030**	0.023**	0.025**	0.022**	0.068*	0.015**
WCRB by survey round, *p-value*	0.113	0.084*	0.083*	0.113	0.194	0.008***
R^2	0.01	0.03	0.03	0.22	0.23	0.31
Observations	20,861	20,728	20,728	17,821	17,821	17,821
Panel B: Positive shocks						
Post-election	0.0260	0.0265	0.0265	0.0244	0.0207	0.0268
	[0.00583]	[0.00597]	[0.00597]	[0.00582]	[0.00489]	[0.00560]
Cluster-robust SE, *p-value*	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
WCRB by state of living, *p-value*	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***
WCRB by survey round, *p-value*	0.004***	0.004***	0.004***	0.005***	0.012**	0.002***
R^2	0.02	0.03	0.03	0.26	0.25	0.34
Observations	11,290	11,211	11,211	9,909	9,909	9,868
Panel C: Negative shocks						
Post-election	-0.00473	-0.00342	-0.00336	-0.00291	-0.00339	-0.00200
	[0.00447]	[0.00438]	[0.00437]	[0.00470]	[0.00420]	[0.00445]
Cluster-robust SE, *p-value*	0.293	0.437	0.443	0.537	0.422	0.654
WCRB by state of living, *p-value*	0.485	0.600	0.611	0.700	0.594	0.756
WCRB by survey round, *p-value*	0.516	0.759	0.761	0.834	0.741	0.703
R^2	0.01	0.02	0.02	0.15	0.20	0.27
Observations	9,571	9,517	9,517	7,912	7,912	8,930
State-of-living FE	Y	Y	Y	Y	Y	Y
Election-window FE	Y	Y	Y	Y	Y	Y
Demographics		Y	Y	Y	Y	Y
Socio-economics			Y	Y	Y	Y
Political attitudes				Y	Y	Y
Entropy balancing					Y	

Note: Estimates for OLS regressions in columns (1) to (5) and average marginal effects for logit regressions in column (6). Standard errors (SE) clustered by state of living × survey round are reported in square brackets. The respective *p-value* is based on a standard Wald test under the null hypothesis that $\beta = 0$. *WCRB p-values* test the same hypothesis using the wild-cluster restricted bootstrap (WCRB) with standard errors clustered by state of living and by survey round, respectively. We apply the *boottest* command in STATA, using the 6-point distribution from Webb (2013). Election windows included in panel B are: SN, HH, (BW, RP, ST), (MV, BE). Election windows included in panel C are: HB, (SH, NW), SL. Demographics: age (18-70+, 10 cat.), age squared, gender (0-1), marital status (0-1), full set of dummies on education attainment (low, medium, high, in school). Socio-economics: full set of dummies on employment status (full time, part time, marginal, unemployed, in training, retired, other). Political attitudes: last vote AfD (0-1), self-positioning on left-right-scale (0-10), scaling of government performance (0-10), immigration perceived as most important issue (0-1). In models (1) to (4) and model (6), sample weights are used which are provided with the poll data. In model (5), matching weights from entropy balancing are applied based on the demographic and socio-economic covariates as well as the state of living. R^2 reports the adjusted R-squared for models (1) to (5) and pseudo R-squared for model (6). In panel C, model (6), we do not include immigration attitude as a control because including this variable reduces the sample such that the WCRB methods are not feasible. The sign and magnitude of the coefficient estimate does not change upon inclusion of this covariate. *** p<0.01, ** p <0.05, * p<0.1.

In contrast, we find no significant – if anything, a negative – effect of the exposure to lower-than expected state-level AfD vote shares on individually reported vote intentions, as displayed in Panel C of table 2. One potential explanation for this result could be that negative deviations from federal-level polls are not only less frequent but also smaller in absolute magnitude than the positive shocks. They may therefore be too small to provoke observable reactions. Statistical inference based on the different standard error estimates is highly consistent across specifications in Panels B and C of table 2. This increases our confidence that neither the number of clusters nor the cluster-level treatment is problematic for correct statistical inference in our application. The mixed overall treatment effect for the pooled sample in Panel A is thus likely driven by heterogeneity in election information shocks with respect to sign.

Not only does the sample separation into positive and negative shocks imply a separation according to shock magnitude but also, to some extent, a geographical and temporal separation. The largest positive shocks occurred in states in former East Germany where the AfD has been particularly successful to mobilize voters. Furthermore, elections entailing negative information shocks were rather held at the end of our investigation period. Consequently, shocks in specific states could have had a greater impact while, at the same time, later elections could have been subject to a familiarization effect regarding AfD success. With these suppositions in mind, we investigate the single elections more closely. Figure 2 shows the results of separate regressions for each election window in chronological order. We find significant effects for three election windows: Saxony, the combined elections in Baden-Wuerttemberg, Rhineland-Palatinate and Saxony-Anhalt as well as the election window comprising Mecklenburg-Hither Pomerania and Berlin. In fact, we observe larger and significant effects for East German states, suggesting that these larger shocks induce stronger reactions. In line with this, election windows enclosing multiple election events tend to have a more pronounced effect on individual vote intentions.

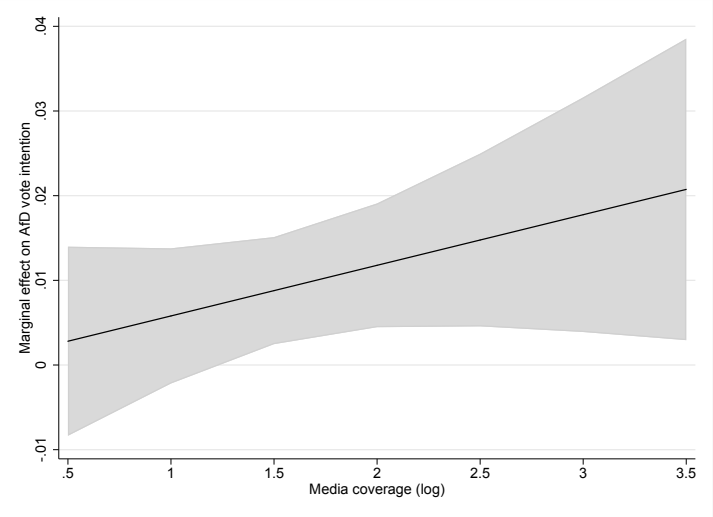


Note: Separate regressions by election window. Coefficient estimates from linear regressions estimating model (5) from table 2 for each election window separately, with coefficient intervals based on SEs clustered by state of living × survey round. Full estimation results displayed in appendix table C.3. The percentage numbers indicate the AfD vote share in each election, while the positive (negative) sign in parentheses indicates that the election outcome positively (negatively) deviated from the most recent federal-level poll on the AfD. Results for state elections in chronological order from 2013 to 2017. SN = Saxony, HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia.

Figure 2: Results by election window.

A critical assumption of our empirical set-up is that individuals actually receive the information from election outcomes in states other than their state of living. Therefore, one important transmission channel of voting contagion is media coverage. As a proxy for the extent of media coverage, we use the relative frequencies of Google searches for the party name *AfD* (or *Alternative fuer Deutschland*) in the survey weeks in question. Figure 3 shows the marginal treatment effect from our baseline estimation when interacted with media coverage. As can be seen, the effect of election information shocks on AfD vote intentions is indeed increasing in the extent of media coverage. Individuals react more strongly to the information shock if the AfD was a frequent subject of media discussion.[16] This is a crucial result for the causal interpretation of the identified relationship as it suggests that the differences in AfD support between treatment and control groups are in fact driven by the informational spillovers on individuals in other parts of the country.

[16] Unfortunately, with our measure, we cannot account for the tonality of the respective media content. Yet, since we focus on media consumption around the time of state elections, we can assume that the AfD election outcomes provide an objective criteria for the general support level in society which is independent of the specific tonality of the news consumed through Google.



Note: Marginal effects from linear regressions estimating model (5) from table 2 with *media coverage, log* and its interaction with *post-election*. *media coverage, log* measures relative frequencies of weekly Google searches of *AfD* (or *Alternative fuer Deutschland*). For the control group, we take the value in the control WoS. For the treatment group, we use the value in the WoE to account for a direct link between elections and media coverage.

Figure 3: Election information shocks and media coverage.

In the following section, we provide a number of sensitivity checks on the validity of our results. Additional results on alternative specifications can be found in the appendix.

5 Robustness checks

5.1 Placebo treatment

A major threat to identification in our empirical model is the presence of unobserved time-varying factors at the macro level which are not captured by the included election-window fixed effects and affect the individual disposition to report an AfD vote intention. To check for general trends in AfD support or other confounding events, we conduct placebo tests and estimate our empirical model for *counterfactual* election windows. To this end, we identify windows of two subsequent surveys that are not affected by a state election but may be driven by the same cyclical or macro-level factors as our examined elections to which they are close. We apply two different placebo tests. First, we choose the two most recent surveys *prior* to an election window. This method implies that now the control group from our baseline regression becomes the treatment group in the

placebo regression. Second, we run the same regressions using the first two surveys *after* the election window, i. e. the treatment group from our baseline regression becomes the control group in the placebo regression. Table 3 displays no significant effects of these counterfactual treatment variables, regardless which method we use. These results provide strong support for our claim that it is in fact the election information shock which shapes individually reported vote intentions.

Table 3: Results for placebo election information shocks.

	Placebo treatment I			Placebo treatment II		
DV: AfD vote intention	Pooled	Positive shocks	Negative shocks	Pooled	Positive shocks	Negative shocks
	(1)	(2)	(3)	(4)	(5)	(6)
Counterfactual post-election	-0.00463 [0.00306]	-0.00305 [0.00371]	-0.00681 [0.00452]	-0.00186 [0.00325]	-0.00389 [0.00472]	0.000550 [0.00366]
Cluster-robust SE, *p-value*	0.132	0.412	0.135	0.568	0.412	0.881
WCRB by state of living, *p-value*	0.211	0.374	0.267	0.651	0.489	0.899
WCRB by survey round, *p-value*	0.116	0.456	0.233	0.757	0.689	0.585
R^2	0.24	0.26	0.21	0.23	0.25	0.21
Observations	18,380	10,467	7,913	18,178	10,263	7,915
State-of-living FE	Y	Y	Y	Y	Y	Y
Election window FE	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y	Y	Y
Entropy balancing	Y	Y	Y	Y	Y	Y

Note: Table displays estimates for OLS regressions. Standard errors (SE) clustered by state of living × survey round are reported in squared brackets. For *Placebo treatment I*, we use the two most recent survey waves *before* the actual election. For *Placebo treatment II*, we use the two most recent survey waves *after* the actual election. *Counterfactual* election windows for sample with positive shocks: SN, HH, (BW, RP, ST), (MV, BE). *Counterfactual* election windows for sample with negative shocks: HB, (SH, NW), SL. Control variables as in table 2. Matching weights from entropy balancing based on the demographic and socio-economic covariates as well as the state of living. R^2 reports the adjusted R-squared for all models. *** p<0.01, ** p <0.05, * p<0.1.

5.2 Heterogeneous effects

As clearly discernible in figure 2, the AfD holds a stronger voter basis in East German states. The reasons for a potentially structural difference in populist dispositions was also intensively discussed in the media. Generally, the difference in voting patterns

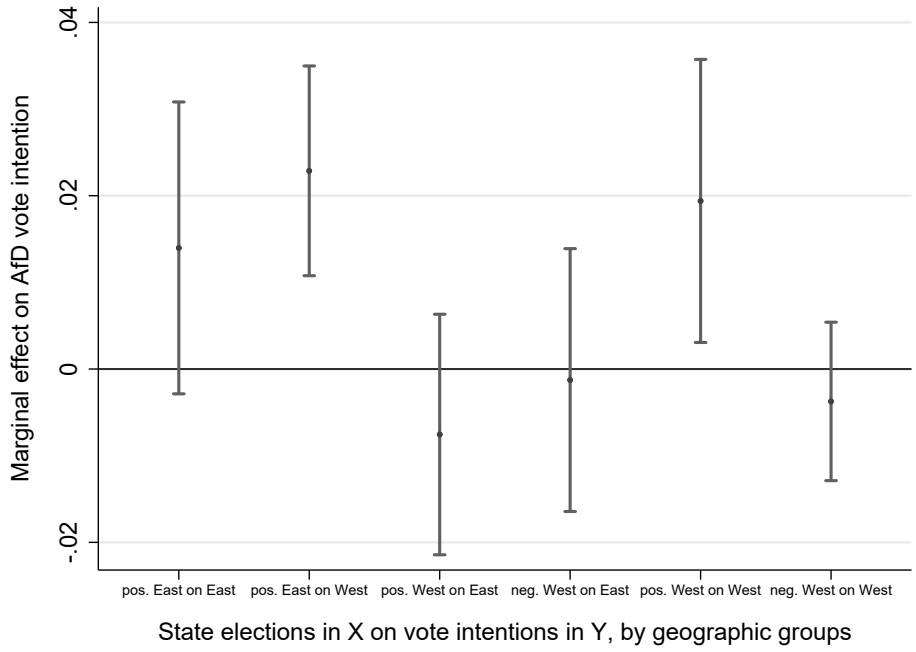
between former Eastern and Western states may correspond to a division along socio-economic and cultural characteristics. Thus, when reflecting on dominant public views, individuals may be more strongly affected by changes in the region they consider their socio-cultural or spatially close peer group. This gives rise to the question of whether, on the one hand, election information shocks in the East had a stronger effect on survey respondents in general and, on the other hand, whether respondents react differently to a shock in their own region than to a shock in the other part of the country. Figure 4 shows OLS estimates[17] for model (5) in table 2 when splitting both election windows and survey respondents according to their affiliation with East or West Germany.[18] Strikingly, no election information shock, neither in the East nor West, seems to have a significant impact on East German survey respondents. However, positive shocks in either part of the country significantly increase the self-reported AfD vote intention of West German respondents. This pattern suggests that larger-than expected AfD support specifically encouraged respondents in states with previously low AfD advocacy to reveal their preference. Consequently, the evidence points to a non-linear, base-dependent effect of election information shocks on vote intentions, which matches our theoretical considerations. Unfortunately, we cannot test this functional form due to our limited number of election windows. Nevertheless, the evidence does not support the idea that cultural proximity disproportionately affects responsiveness to election information shocks.

In order to further investigate this base-dependent effect, we conduct a second test, interacting our treatment variable with an indicator taking up the value 1 if the AfD was already represented in the parliament of a respondent's own state of living at the time of the election in question. OLS estimates are reported in columns (1) and (2) of table 4. While the simple effects of a larger-than expected AfD vote share are still comparable to the baseline estimation, they are not significantly different for respondents in a state with an AfD fraction present in the state parliament. A possible explanation could lie in the nature of the here-conducted test. While, for instance, the AfD vote share in a previous election in the respondent's state of living provides precise information on the party's popularity, a simple indicator of AfD representation may imply to little variance in support.[19]

[17] Estimates are reported in appendix table C.4.

[18] We leave out the election window for BW, RP and ST which comprises both Eastern and Western states.

[19] However, we refrain from including support information from the respondent's state of living as a control variable to avoid the endogeneity problem discussed in section 3.2.



Note: Marginal effects from linear regressions estimating model (5) from table 2 for elections held in X and respondents in Y. Elections in *West* refer to the elections in HH, HB, SL and (SH, NW). Elections in *East* refer to the elections in SN and (MV, BE). Respondents in *West* and *East* comprise inhabitants in the respective states.

Figure 4: Election information shocks and vote intentions by geographic groups.

As a final test of heterogeneity in responsiveness to election information shocks, we examine a potentially time-varying effect. Specifically, we explore whether elections held after the AfD's programmatic shift and the *refugee crisis* in the late summer of 2015 had a stronger effect on vote intentions than in the early years of the AfD. Estimates in columns (3) and (4) of table 4 suggest that this is actually the case. *Post-election* captures the treatment effect during the pre-shift period where positive (negative) information shocks lead to a higher (lower) likelihood to self-report a vote intention, which is exactly in line with our theoretical considerations. Yet in the post-shift period, positive election information shocks seem to even more strongly increase AfD vote intentions. The overall marginal effect for negative shocks post-shift is however insignificant, reflecting the pattern from our baseline results.

Table 4: Results for heterogeneous election information shocks.

	X: AfD in own state parliament?		X: Post-shift	
DV: AfD vote intention	Positive shocks (1)	Negative shocks (2)	Positive shocks (3)	Negative shocks (4)
Post-election	0.0170	-0.00765	0.00959	-0.0134
	[0.00526]	[0.00496]	[0.00521]	[0.0062]
Cluster-robust SE, *p-value*	0.00164***	0.126	0.0682*	0.0336**
WCRB by state of living, *p-value*	0.0081***	0.4304	0.0895*	0.0466**
WCRB by survey round, *p-value*	0.0226**	0.2680	0.0638*	0.3088
X	0.0114	-0.00424	-0.00654	0.0204
	[0.0127]	[0.00989]	[0.00867]	[0.00653]
Cluster-robust SE, *p-value*	0.373	0.669	0.452	0.002***
WCRB by state of living, *p-value*	0.5664	0.7292	0.5105	0.0063***
WCRB by survey round, *p-value*	0.5161	0.5765	0.3800	0.0804*
Post-election × X	0.0120	0.00864	0.0215	0.0157
	[0.0110]	[0.00838]	[0.00889]	[0.00817]
Cluster-robust SE, *p-value*	0.279	0.305	0.017**	0.058*
WCRB by state of living, *p-value*	0.4193	0.4982	0.1778	0.0198**
WCRB by survey round, *p-value*	0.5100	0.5295	0.0051***	0.1123
R^2	0.253	0.197	0.253	0.197
Observations	9,909	7,912	9,909	7,912
State-of-living FE	Y	Y	Y	Y
Election-window FE	Y	Y	Y	Y
Demographics	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y
Entropy balancing	Y	Y	Y	Y

Note: Table displays estimates for OLS regressions. Standard errors (SE) clustered by state of living × survey round are reported in squared brackets. X refers to the explanatory variable as indicated in the column header. *AfD in own state parliament?* is 1 if the AfD was already represented in the respondent's state parliament at the time of the election in question and 0 otherwise. *Post-shift* is 1 for all elections held after the refugee crisis and the AfD program shift in summer 2015 and 0 for elections held prior to these events. Matching weights from entropy balancing based on the demographic and socio-economic covariates as well as the state of living. R^2 reports the adjusted R-squared for all models. *** p<0.01, ** p <0.05, * p<0.1.

While the post-shift period put professed AfD supporters at a higher risk of social rejection, the party strongly benefited from the migration policy debate, as reflected in relatively large vote shares in the election windows of Baden-Wuerrtemberg, Rhineland-Palatinate and Saxony-Anhalt as well as Mecklenburg Hither-Pomerania and Berlin. Not only did polls underestimate AfD support in all of these state elections, vote shares also took up two-digit figures for the first time. The refugee inflow as an exogenous event in combination with five highly successful elections may have lead to a ratchet effect. Exceeding a support threshold of 10 percent may additionally have shifted AfD perception among voters away from a minor, special-issue party towards a serious political contestant. This further underlines our interpretation that the rather small negative shocks in the later elections in Saarland, North Rhine-Westphalia and Schleswig-Holstein were too weak to affect vote intentions.

5.3 Alternative measures of party support

We also test whether our results are consistent for other individual-level measures of AfD support. To this end, we replace the dependent variable by two other items from the Politbarometer that capture an AfD-supportive attitude. First, we use *party ranking* as a dependent variable. Here, the survey respondent is asked to rank a number of parties in descending order of assessed competence and appeal. We use this ranking to create a binary variable, taking up the value 1 if the respondent ranks the AfD first and 0 if he or she ranks any other party first. Second, we use a respondent's general *party affinity* as a dependent variable. In addition to short-term vote intentions, survey respondents are asked for their long-term identification with a party, which can be assumed to depend on ideological proximity rather than short-notice strategic concerns or protest voting behavior. Again for *party affinity*, we construct a binary variable which equals 1 if the individual states an AfD affinity and 0 if an affinity for any other party or no party affinity is stated.

The results for both alternative support measures confirm our previous findings (see table 5). When exposed to a positive election information shock, survey respondents are more likely to report a high ranking of as well as a general identification with the AfD compared to respondents interviewed before the shock. Nevertheless, the estimates are smaller in magnitude than the effects found for vote intentions. Interestingly, we find limited support for the notion that negative information shocks are associated with a lower

party ranking, although this effect is less precisely estimated and not significant when accounting for within-survey correlation. Following the argumentation of informational cascades outlined in section 2.2, individuals may understand their fellow voters' decisions as a sign of party quality assessment. In that sense, lower-than expected votes shares signal a lower-than necessary political competence.

Table 5: Alternative measures of self-reported AfD support.

DV:	Party ranking			Party affinity		
	Pooled	Positive shocks	Negative shocks	Pooled	Positive shocks	Negative shocks
	(1)	(2)	(3)	(4)	(5)	(6)
Post-election	0.00303	0.0115	-0.00721	0.00239	0.00497	-0.000996
	[0.00288]	[0.00391]	[0.00385]	[0.00151]	[0.00156]	[0.00251]
Cluster-robust SE, *p-value*	0.294	0.004***	0.065*	0.115	0.002***	0.692
WCRB by state of living, *p-value*	0.275	0.001***	0.084*	0.110	0.002***	0.637
WCRB by survey round, *p-value*	0.540	0.005***	0.168	0.432	0.226	0.654
R^2	0.23	0.23	0.23	0.06	0.05	0.07
Observations	16,297	9,019	7,278	17,458	9,704	7,754
State-of-living FE	Y	Y	Y	Y	Y	Y
Election window FE	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y	Y	Y
Entropy balancing	Y	Y	Y	Y	Y	Y

Note: Table displays estimation coefficients for OLS regressions. Cluster-robust standard errors (SE) clustered by state of living × survey round reported in squared brackets. *Party ranking*= 1 if the respondents ranks the AfD first among all parties; 0 if the respondents ranks a different party first; missing if the respondents does not state any party. *Party affinity*= 1 if the respondents states that, among all parties, she feels in general most closely connected to the AfD; 0 if the respondent states a different party affinity; missing if the respondent states no party affinity. Election windows for the sample of positive shocks: SN, HH, (BW, RP, ST), (MV, BE). Election windows for the sample of negative shocks: HB, (SH, NW), SL. Control variables as in table 2. Matching weights from entropy balancing based on the demographic and socio-economic covariates as well as the state of living. R^2 reports the adjusted R-squared for all models. *** p<0.01, ** p <0.05, * p<0.1.

From the results for *party affinity*, we can derive the tentative conclusion that sudden shifts in observed aggregate-level AfD support also affect the disposition to report a more general and persisting party preference, although statistical inference on this effect is somewhat mixed (see column (5) in table 5). To the extent that this effect is not counterbalanced by negative shocks in aggregate party support, these results could point

to a hysteresis effect on social acceptability of nationalist and right-wing populist parties. Yet, given the limits of our data structure, we are cautious in drawing final conclusions about the persistence of contagion effects in populist voting and leave this question for future research.

6 Conclusion

Is populist voting contagious? In order to explain the rist of populist and nationalist movements around the globe, understanding the role of social compliance in electoral behavior appears important. In this paper, we examine whether unexpected shifts in observed social support for a right-wing populist party encourage individuals to report a respective political attitude in survey interviews.

We apply a quasi-experimental event-study design for Germany, where the right-wing populist AfD has registered considerable support among the German electorate since its foundation in 2013. This upward trend was accompanied by a heated debate about the acceptability of the AfD platform, which features nationalist, xenophobic and far-right elements. We presuppose that voters are hesitant to openly support socially unaccepted movements in order to avoid reputational losses and social rejection. However, the observation of higher aggregate advocacy may serve as a signal of the party's improved social standing. The German federal system in combination with the recent emergence of a far right-wing party thus provides a well-suited setup for analysis. Voters were able to use AfD vote shares in the staggered state elections as a source of new information about general AfD preferences. We empirically test whether the exposure to such election information shocks impacts on the individual disposition to openly report an AfD vote intention.

To that end, we employ repeated cross-sectional data from the German Politbarometer survey. *Election information shocks* are defined as the deviation of AfD state election outcomes from previously known opinion polls at the federal level. We compare the average likelihood to report an AfD preference for individuals interviewed right after a state election to those interviewed right before the election and thus unaffected by the information shock. Our empirical results provide systematic evidence that information shocks associated with larger-than expected AfD vote shares in state elections raise subsequently reported AfD vote intentions in *other* states by up to 3 percentage

points. These findings are consistent for other measures of individual-level AfD support and robust to accounting for different levels of within-cluster correlation of the model errors. Applying entropy balancing and conducting placebo tests supports our argument that election information shocks have a causal effect on self-reported individual vote intentions.

While these findings cannot necessarily be extrapolated to other right-wing populist parties, they provide clear and quantitatively relevant evidence that peer influence and social compliance play an important role in shaping populist attitudes. Given reputational benefits from electoral behavior, the interaction between aggregate and individual-level support can lead to a contagious cascade of electoral success for nationalist ideas. In fact, our results provide some tentative evidence for a hysteresis effect of right-wing populist party support.

In this regard, our findings bear important implications for the impact of (social) media and key political figures on shaping public images and ideas. While we remain agnostic about the normative assessment of how these cascades potentially change policies as such, we acknowledge that the authoritarian elements inherent in right-wing populist platforms pose a risk to democratic functionality which can be magnified by a populist cascade. Policymakers are thus challenged to address the concerns of voters turning towards populist movements. Furthermore, political education is called to foster the process of individual and independent opinion-forming. As our analysis shows that extensive media coverage of the AfD election outcomes increases the stimulating effect on self-reported vote intentions, media outlets should reflect upon their role in shaping public opinion .

Ultimately, the empirical results presented here are related to the social desirability bias in survey research. However, our data does not allow us to draw conclusions concerning changes in actual voting behavior in response to observable shifts in aggregate party advocacy. Similarly, our quasi-experimental set-up limits conclusions about the dynamic effects of social contagion in populist voting as well as their persistence. Future research could exploit suitable panel data to study the long-run effects of aggregate information shocks. Nevertheless, to our knowledge, the results provide novel evidence on the presence of a contagion effect in populist preferences that helps to a priori assess the potential dynamics of populist movements in other political systems where the lack of a suited empirical set-up does not allow to quantify them.

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Declaration of interests

None

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Appendix

A Variables and summary statistics

Table A.1: Summary statistics.

Variable	Obs	Mean	Std. Dev.	Min	Max
Dependent variable:					
AfD vote intention	20,861	0.058	0.234	0	1
Independent variables:					
Election information shock	20,861	0.498	0.5	0	1
Gender	20,728	0.473	0.499	0	1
Age	20,728	7.618	2.209	1	10
Age squared	20,728	62.916	28.865	1	100
Married	20,728	0.597	0.490	0	1
Low education	20,728	0.183	0.387	0	1
Medium-level education	20,728	0.361	0.480	0	1
In school	20,728	0.005	0.068	0	1
Part-time employment	20,728	0.122	0.327	0	1
Marginally employed	20,728	0.001	0.039	0	1
Unemployed	20,728	0.02	0.142	0	1
In vocational training	20,728	0.027	0.163	0	1
Retired	20,728	0.345	0.475	0	1
Other employment status	20,728	0.032	0.177	0	1
Voted for AfD in the last federal election?	17,821	0.022	0.148	0	1
Self-positioning on left-right scale	17,821	5.445	1.894	1	11
Satisfaction with current government	17,821	7.051	2.489	1	11
Immigration as most important issue?	17,821	0.536	0.499	0	1

Table A.2: Variables description.

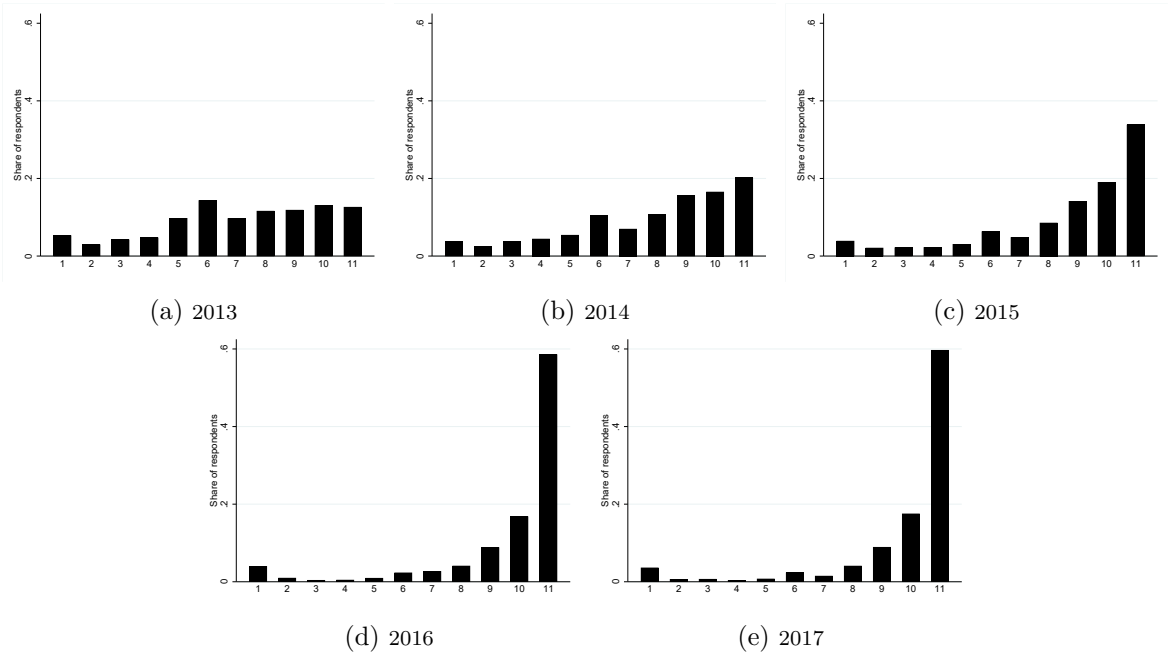
Variable	Description	Scale	Manifestations
Dependent variables:			
AfD vote intention	Respondent intends to vote for the AfD in a hypothetical federal election next Sunday.	nominal	0=No, 1=Yes
AfD party ranking	Respondent ranks AfD first in a group of German political parties.	nominal	0=No, 1=Yes
AfD party affinity	Respondent reports a general affinity to the AfD as a party.	nominal	0=No, 1=Yes
Explanatory variables:			
Election information shock	Treatment; exposure to newly acquired information about aggregate AfD support as revealed by the most recent state election	nominal	0=No, 1=Yes
Election information shock (positive)	Treatment; exposure to newly acquired information about aggregate AfD support as revealed by the most recent state election, AfD vote share is larger than the last federal-level poll before the election	nominal	0=No, 1=Yes
Election information shock (negative)	Treatment; exposure to newly acquired information about aggregate AfD support as revealed by the most recent state election, AfD vote share is lower than the last federal-level poll before the election	nominal	0=No, 1=Yes
Media coverage, log	Log of relative frequencies of weekly Google searches for terms *AfD* and *Alternative for Germany* in the respondent's survey week	metric	
Covariates:			
Gender	Respondent's gender	nominal	0=male, 1=female
Age	Respondent's age category	nominal	1=18 to 20 years, 2=21 to 24 years, 3=25 to 29 years, ..., 8 = 50 to 59 years, 9=60 to 69 years, 10= 70 years and older
Age squared	Respondent's age^2	nominal	squared values of *Age*
Married	Respondent is married.	nominal	0=No, 1=Yes
Low education	Respondent's highest education level is low (=high school degree) education.	nominal	0=No, 1=Yes
Medium-level education	Respondent's highest education level is medium-level (=vocational training) education.	nominal	0=No, 1=Yes
High education	Respondent's highest education level is high (=academic) education.	nominal	0=No, 1=Yes
In school	Respondent still goes to school.	nominal	0=No, 1=Yes
Part-time employment	Respondent is part-time employed.	nominal	0=No, 1=Yes
Marginally employed	Respondent is marginally employed.	nominal	0=No, 1=Yes
Unemployed	Respondent is unemployed.	nominal	0=No, 1=Yes
In vocational training	Respondent currently undergoes vocational training.	nominal	0=No, 1=Yes
Retired	Respondent has retired.	nominal	0=No, 1=Yes
Other employment status	Any other employment status applies to the respondent.	nominal	0=No, 1=Yes
Voted for AfD in the last federal election?	Respondent gave his/her second vote to the AfD in the last federal election in 2013.	nominal	0=No, 1=Yes
Self-positioning on left-right scale	Respondent's self-assessed political position on a scale from 1=left to 11=right	ordinal	1 to 11
Satisfaction with current government	Respondent's satisfaction with the incumbent federal government on a scale from −5=very dissatisfied to 5=very satisfied	ordinal	−5 to 5
Immigration as most important issue?	Respondent reports immigration to be the most important political issue.	nominal	0=No, 1=Yes
State of living	Federal state that respondent is currently living in	nominal.	1 to 16

B Case selection and sample

In this section, we provide further information on perceptions of the AfD in Germany that supports our claim that German state elections can be understood as election information shocks, providing unexpected information about a controversial political platform. We also show that these shocks split the sample of survey respondents evenly in a treatment and a control group and that there are no structural differences in the distribution of personal characteristics of the respondents across these groups.

Figure B.1 shows how respondents in the German Longitudinal Election Study rated the AfD on a left-right-scale from 1 to 11. Figure B.2 displays for each state election deviations of the realized AfD vote shares from earlier polls. Figure B.2a uses as poll data the most recent forecast for the specific election in question which is based on survey interviews conducted in the state of the election (these surveys are not part of our sample). Normally, the last poll based on these interviews is published three days before the election. In contrast, figure B.2b uses as poll data the most recent estimate of the current AfD vote share at the federal level. This estimate is based on the regular Politbarometer surveys which we use in our analysis. Table C.5 shows that the relative deviation of AfD vote shares from *Politbarometer* polls is comparable to other data sources. Figure B.3 shows relative frequencies of Google searches of the respective state name in the four weeks around an election. Together, these figures show that (i) during our sample period public perceptions of the AfD as a nationalist far-right party clearly increased; (ii) the party has realized vote shares in German state elections that substantially deviated from pre-election polls both at the state and at the federal level; and (iii) these deviations were unanticipated due to the low interest in state elections before the election day.

Table B.1 displays covariate imbalance tests for the full sample. For each covariate, the reported coefficient reports the estimated average difference of this variable in the treatment and control group. Imbalance statistics for each election window separately and after entropy balancing can be found in the online supplementary material.

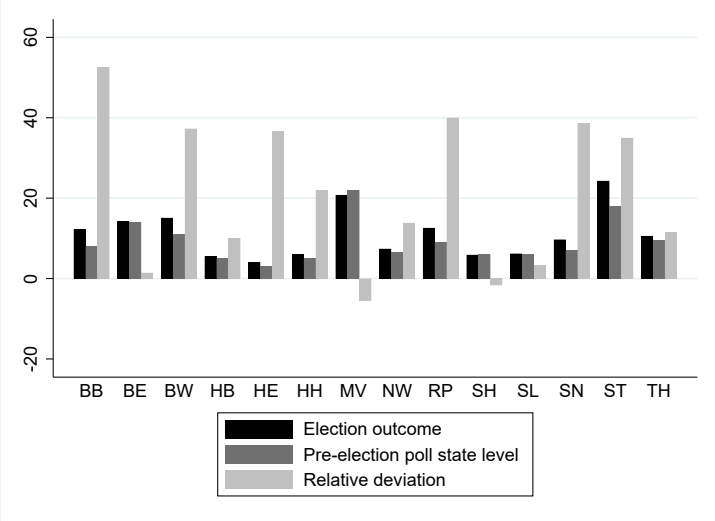


(a) 2013 (b) 2014 (c) 2015

(d) 2016 (e) 2017

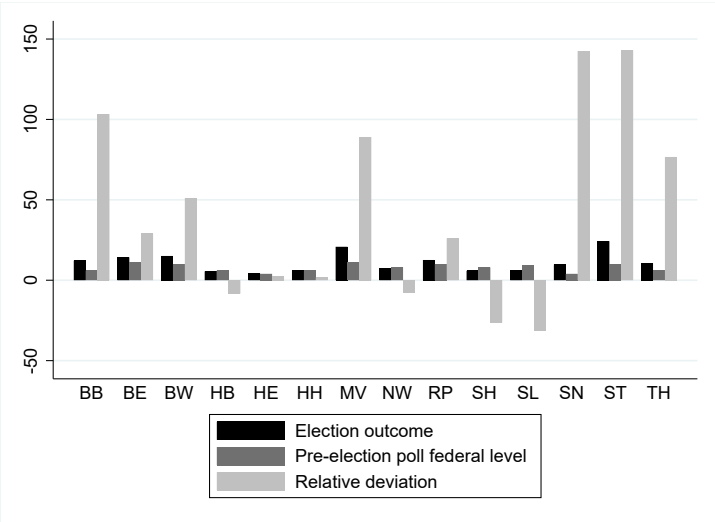
Note: Figure displays the share of respondents assigning the respective rating to the AfD. Scale runs from 1=left to 11=right.

Figure B.1: AfD ratings on a 1-to-11 left-right-scale in the German Longitudinal Election Study.



Note: Results for state elections in chronological order from 2013 to 2017. *Pre-election poll state level* reports the most recent pre-election poll for the respective state election (published ca. three days before the election). *Relative deviation* measures the percentage deviation of the poll from the realized vote share. HE = Hesse, SN = Saxony, BB = Brandenburg , TH = Thuringia , HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia.

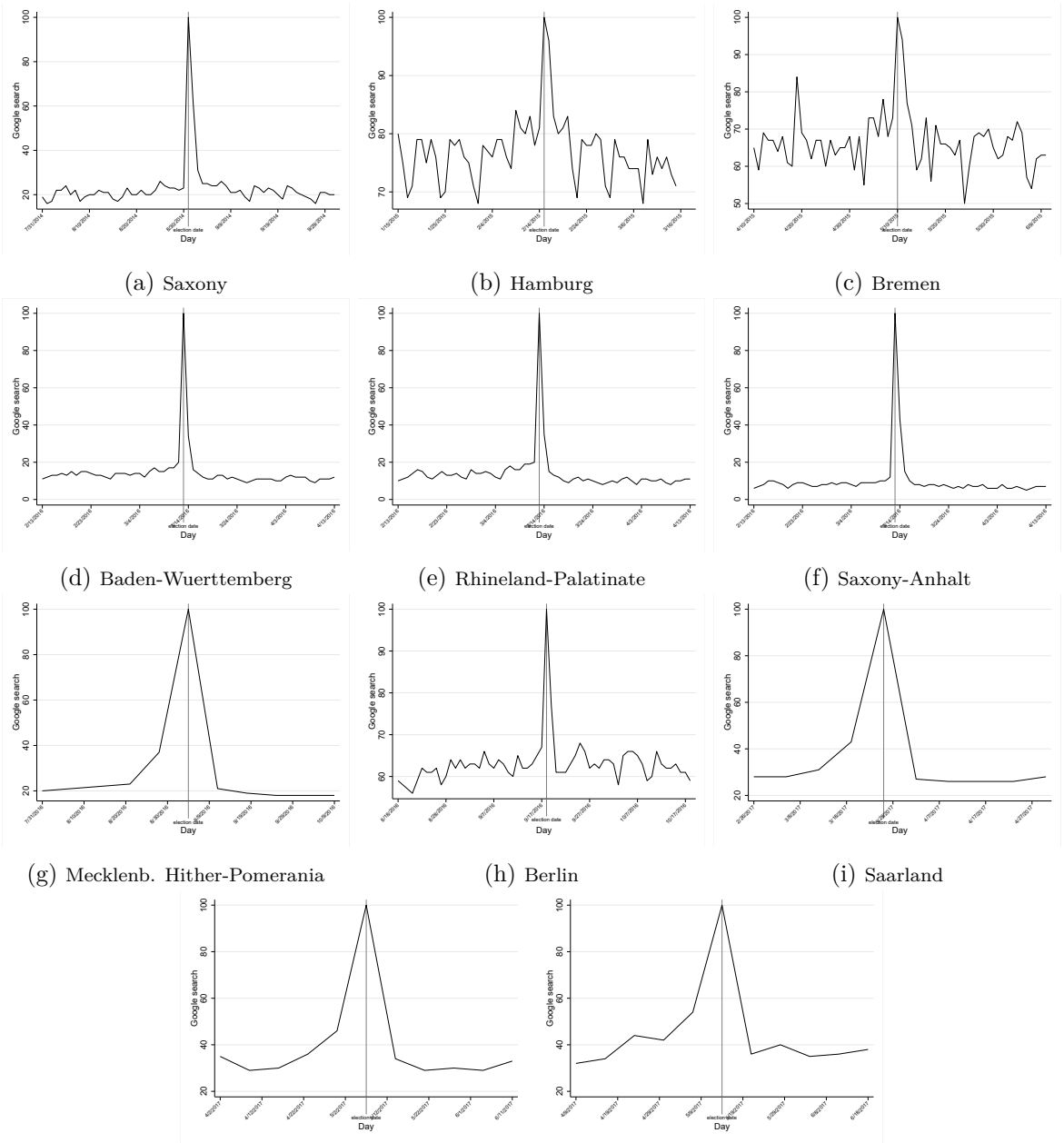
(a) State-level election outcomes vs. pre-election polls.



Note: Results for state elections in chronological order from 2013 to 2017. *Pre-election poll federal level* measures the current estimated AfD share at the federal level (as published by the most recent general poll *before* the state election). *Relative deviation* measures the percentage deviation of the poll from the realized vote share. HE = Hesse, SN = Saxony, BB = Brandenburg , TH = Thuringia , HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia.

(b) State-level election outcomes vs. federal-level polls.

Figure B.2: Vote shares for the AfD in German state elections and pre-elections polls.



(a) Saxony (b) Hamburg (c) Bremen

(d) Baden-Wuerttemberg (e) Rhineland-Palatinate (f) Saxony-Anhalt

(g) Mecklenb. Hither-Pomerania (h) Berlin (i) Saarland

(j) Schleswig-Holstein (k) North Rhine-Westphalia

Figure B.3: Relative frequencies of Google searches for a state name around the respective state election.

Table B.1: Balance in covariates between treatment and control groups.

Covariate	Estimates	Observations	R-squared
State of living	-0.00137	20,728	0.000
	(0.0640)		
Gender	0.00492	20,728	0.000
	(0.00694)		
Age	-0.00357	20,728	0.000
	(0.0307)		
Age squared	-0.00776	20,728	0.000
	(0.401)		
Married	-0.00401	20,728	0.000
	(0.00681)		
Low education	0.00646	20,728	0.000
	(0.00537)		
Medium-level education	-0.00401	20,728	0.000
	(0.00667)		
In school	-0.00103	20,728	0.000
	(0.000948)		
Part-time employed	-0.00104	20,728	0.000
	(0.00454)		
Marginally employed	0.000686	20,728	0.000
	(0.000537)		
Unemployed	0.000150	20,728	0.000
	(0.00197)		
In vocational training	0.00213	20,728	0.000
	(0.00227)		
Retired	0.00330	20,728	0.000
	(0.00660)		
Other employment status	0.00236	20,728	0.000
	(0.00246)		
Voted for AfD in the last federal election?	0.00215	17,821	0.000
	(0.00221)		
Self-positioning on left-right-scale	0.00699	17,821	0.000
	(0.0284)		
Satisfaction with current government	0.0356	17,821	0.000
	(0.0373)		
Immigration as most important issue?	-0.00899	17,821	0.000
	(0.00747)		

Notes: Coefficients for 18 OLS regressions of a covariate on *post-election*. *post-election* takes the value 1 if the respondent was interviewed in the first survey round after an election (treated), and takes the value 0 if the respondent was interviewed in the last survey round before the election (control). *** p<0.01, ** p<0.05, * p<0.1

Table B.2: Balance in covariates between included and excluded observations.

Covariate	Estimates	Observations	R-squared
State of living	-0.0759**	167,717	0.000
	(0.0640)		
Gender	-0.00108	167,717	0.000
	(0.000514)		
Age	-0.00892	167,717	0.000
	(0.0311)		
Age squared	-0.0220	167,717	0.000
	(0.375)		
Married	-0.0121**	167,237	0.000
	(0.00525)		
Low education	-0.00186	167,034	0.000
	(0.00552)		
Medium-level education	-0.00190	167,034	0.000
	(0.00552)		
In school	0.00232*	167,034	0.000
	(0.00471)		
Part-time employed	0.000725	167,717	0.000
	(0.00363)		
Marginally employed	0.000514	167,717	0.000
	(0.000573)		
Unemployed	-0.00206	167,717	0.000
	(0.00148)		
In vocational training	-0.00290	167,717	0.000
	(0.00282)		
Retired	-0.00358	167,717	0.000
	(0.00487)		
Other employment status	-0.00446*	167,717	0.000
	(0.00243)		
Voted for AfD in the last federal election?	0.000525	167,717	0.000
	(0.00150)		
Self-positioning on left-right-scale	-0.0477**	158,554	0.000
	(0.0229)		
Satisfaction with current government	0.0591**	147,459	0.000
	(0.0299)		
Immigration as most important issue?	0.113***	153,819	0.005
	(0.00567)		

Notes: Coefficients for 18 OLS regressions of a covariate on indicator of being in the main sample. *** p<0.01, ** p<0.05, * p<0.1

C Additional results

Alternative bootstrap methods

Here, we compare our WCRB estimates to alternative bootstrap procedures. The basic intuition of the wild bootstrap method for statistical inference can be summarized as "generating many bootstrap samples that resemble the actual one, computing the test statistic for each of them, and then deciding how extreme the original test statistic is by comparing it with the distribution of the bootstrap test statistics" (Roodman et al., 2019). In the wild cluster bootstrap, the resamples are separately drawn for each cluster g. The new set of the dependent bootstrap variables and the bootstrap error terms are generated by assigning an auxiliary random weight d_g to all residual estimates in cluster g. The weight is commonly drawn from the two-point Rademacher distribution, meaning that it take values -1 and $+1$ with equal probability. A drawback of this approach in samples with few clusters is that there are only 2^G possible combinations of the data. Therefore, we follow Cameron and Miller (2015) and apply instead the 6-point distribution by Webb (2013) which assigns weights d_g of $\{-\sqrt{1.5}, -\sqrt{1}, -\sqrt{0.5}, \sqrt{0.5}, \sqrt{1}, \sqrt{1.5}\}$ with a probability of $1/6$. Simulation evidence provided by Webb (2013) shows that this methods outperforms the two-point wild bootstrap for $G < 10$ and is thus the preferred methods in our case.

For the wild cluster bootstrap, two versions are available, the restricted and unrestricted one. Intuitively, the wild cluster unrestricted bootstrap (WCUB) uses the parameter estimates from the full (i.e. unrestricted) model, whereas the restricted version imposes whatever restriction(s) are to be tested, e.g. $b_j = 0$ (Roodman et al., 2019). Djogbenou et al. (2019) and MacKinnon (2019) show that the restricted version of the wild cluster bootstrap is preferred in most applications. MacKinnon and Webb (2017) also show that the restricted bootstrap leads to more conservative p-values compared to the unrestricted version and that it tends to moderately under-reject. The WCRB is thus the most conservative approach in our empirical application. For these reasons, we apply the WCRB in our main analysis. We use the STATA command *boottest* introduced by Roodman et al. (2019) with the 6-point distribution by Webb and 99.999 repetitions.

However, MacKinnon (2019) and MacKinnon and Webb (2018) demonstrate that the WRCB can be unreliable in certain cases, in particular if the number of treated clusters is very small, the clusters vary greatly in size and the number of treated observations

within a cluster is small. Even at the highest level of clustering in our analysis, i.e. the survey round, we are confident that these concerns should not apply to our specification as the number of treated clusters is not too small relative to the overall number of clusters (50 percent of the survey rounds are treated) and clusters are relatively homogeneous in size (around $1,000$ respondents per survey round). Yet, since the overall number of clusters becomes relatively small for the samples of positive and negative shocks (6 and 7 respectively), we aim to assess the validity of our WCRB estimates by reporting results from the unrestricted wild cluster bootstrap and the ordinary wild bootstrap as well. The ordinary wild bootstrap is a special case of the wild cluster bootstrap because here clusters are drawn at the level of the individual observations and hence, the sample is not clustered at all. MacKinnon and Webb (2018) show that the ordinary wild bootstrap can work well in cases with few clusters.

Table C.1 reports the results from these different procedures. We present p-values from the wild cluster bootstrap with and without the null hypothesis imposed for model errors clustered at the state of living and the survey round, respectively. In addition, we report p-values from the ordinary wild bootstrap procedure, again with and without the null imposed. The estimates are remarkably similar across different procedures and levels of clustering, especially when distinguishing between positive and negative election information shocks in model (2) and (3). We find a significant positive treatment effect of positive election information shocks but no effect of negative election information shocks. Given these heterogeneous effects, it is thus not surprising that for the pooled sample statistical inference based on the different procedures is inconclusive, especially when clustering at the highest level of aggregation, i.e. the survey round.

Table C.1: Alternative bootstrap methods.

DV: AfD vote intention	Pooled	Positive shocks	Negative shocks
	(1)	(2)	(3)
Post-election	0.00962	0.0207	-0.00339
	[0.00353]	[0.00489]	[0.00420]
Cluster-robust SE, *p-value*	0.007***	0.000***	0.422
WCRB by state of living, *p-value*	0.068*	0.002***	0.756
WCUB by state of living, *p-value*	0.056*	0.002***	0.590
WCRB by survey round, *p-value*	0.194	0.012**	0.703
WCUB by survey round, *p-value*	0.215	0.013**	0.740
Ordinary WRB, *p-value*	0.001***	0.000***	0.439
Ordinary WUB, *p-value*	0.003***	0.000***	0.459
Adjusted R^2	0.230	0.253	0.197
Observations	17,821	9,909	7,912
State-of-living FE	Y	Y	Y
Election window FE	Y	Y	Y
Demographics	Y	Y	Y
Socio-economics	Y	Y	Y
Political attitudes	Y	Y	Y
Entropy balancing	Y	Y	Y

Note: Table displays estimates for OLS regressions as in model (5) in table 2. Standard errors (SE) clustered by state of living × survey round are reported in square brackets. The respective *p-value* is based on a standard Wald test under the null hypothesis that $\beta = 0$. The same hypothesis is tested applying the wild-cluster restricted bootstrap (WCRB), the wild-cluster unrestricted bootstrap (WCUB) and the ordinary wild bootstrap, restricted (WRB) and unrestricted (WUB). We apply the *boottest* command in STATA, using the 6-point distribution from Webb (2013) and 99, 999 iterations for the WCRB and the WCUB and 999 iterations for the WRB and WUB. Election windows included in model (2) are: SN, (BW, RP ST), (MV, BE). Election windows included in model (3) are: HH, HB, SL, (SH, NW). *** p<0.01, ** p <0.05, * p<0.1.

Alternative matching strategies

Table C.2 shows estimation results from alternative matching approaches. In columns (1) and (2) of table C.2, we apply weights from an entropy balancing procedure where weights are computed such that they match the distribution of treated and control observations regarding the full set of demographic and socio-economic characteristics, state-of-living, election window as well as political attitudes. The covariates on political attitudes include last vote AfD (0-1), self-positioning on left-right-scale (0-10), scaling of government performance (1-10), immigration perceived as most important issue (0-1). However, we are somewhat cautious when interpreting these estimates as the reported political attitudes may be affected by the treatment and therefore not suited as matching variables. In addition, we also employ coarsened exact matching techniques that generate a matched subsample of treatment and control units based on the observed realizations of the covariates in the sample rather than applying a weighting scheme. Again, we show results for two vectors of covariates which we use for matching, including and excluding political attitudes.

Table C.2: Alternative matching results.

DV: AfD vote intention	Entropy balancing		Coarsened exact matching			
	Positive shocks	Negative shocks	Positive shocks	Negative shocks	Positive shocks	Negative shocks
	(1)	(2)	(3)	(4)	(5)	(6)
Post-election	0.0208***	-0.00339	0.0218***	-0.00430	0.0173**	0.00873
	[0.00468]	[0.00414]	[0.00516]	[0.00625]	[0.00662]	[0.00785]
Cluster-robust SE, *p-value*	0.000***	0.414	0.000***	0.493	0.010**	0.269
WCRB by state of living, *p-value*	0.001***	0.582	0.011**	0.640	0.037**	0.391
WCRB by survey round, *p-value*	0.012**	0.635	0.009***	0.775	0.071**	0.297
Adj. R^2	0.254	0.204	0.263	0.205	0.151	0.0938
Observations	9,909	7,912	7,682	5,722	3,822	2,329
State-of-living FE	Y	Y	Y	Y	Y	Y
Election-window FE	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y	Y	Y
Baseline matching	Y	Y	Y	Y	Y	Y
Baseline matching plus political attitudes	Y	Y	N	N	Y	Y

Note: Table displays estimates for OLS regressions. Standard errors (SE) clustered by state of living × survey round are reported in square brackets. In all models, matching is based on the demographic and socio-economic control variables, the state of living and the election window. In models (1), (2), (5) and (6), we additionally include the covariates on political attitudes in the matching process. Positive shocks comprise the elections in SN, (BW, RP ST), (MV, BE). Negative shocks comprise the elections in HH, HB, SL, (SH, NW). *** p<0.01, ** p <0.05, * p<0.1.

Regressions by election window

Table C.3: Regressions by election window.

Election:	SN	HH	HB	BW, RP, ST	MV, BE	SL	NW, SH
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DV: AfD vote intention	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Post-election	0.0152	0.00694	-0.0111	0.0385	0.0262	0.000219	0.00870
	(0.00607)	(0.00522)	(0.00406)	(0.0114)	(0.00622)	(0.00710)	(0.00593)
p-value	0.019**	0.194	0.010**	0.002***	0.000	0.976	0.153
WCRB by state of living, *p-value*	0.139	0.369	0.070*	0.044**	0.031**	0.971	0.335
Adj. R^2	0.362	0.320	0.291	0.225	0.228	0.170	0.205
Observations	2,040	2,783	2,896	2,469	2,617	2,751	2,265
State-of-living FE	Y	Y	Y	Y	Y	Y	Y
Time trend	Y	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y	Y	Y	Y
Entropy balancing (eb)	Y	Y	Y	Y	Y	Y	Y

Note: Table displays estimates for the OLS regressions. As the baseline, we report robust standard errors clustered by state of living × survey round in round brackets. *p-value* is based on a standard Wald test on the null hypothesis that $\beta = 0$ using the baseline cluster-robust standard errors. *WCRB p-values* refer to the same hypothesis using the wild-cluster restricted bootstrap (WRCB) with standard errors clustered by state of living. We do not report p-values for WCRB with standard errors clustered at the survey round as the number of clusters (two per regression) is too small to obtain valid inference for the state-specific regressions. The control variables included are those from table 2. In all models, matching weights from entropy balancing based on the demographic and socio-economic covariates as well as the state of living. *** p<0.01, ** p<0.05, * p<0.1.

Heterogeneity by geographic groups

Table C.4: Election information shocks and vote intentions by geographic groups.

DV: AfD vote intention	pos. East on East (1)	pos. East on West (2)	pos. West on East (3)	neg. West on East (4)	pos. West on West (5)	neg. West on West (6)
Post-election	0.0140	0.0229	-0.00756	-0.00127	0.0194	-0.00374
	[0.00790]	[0.00598]	[0.00642]	[0.00751]	[0.00774]	[0.00455]
Cluster-robust SE, *p-value*	0.0972*	0.000463***	0.261	0.866	0.0227**	0.416
WCRB by state of living, *p-value*	0.3243	0.0544*	0.4287	0.9069	0.0883*	0.6601
WCRB by survey round, *p-value*	0.0615*	0.1121	0.2230	0.9062	0.1659	0.5373
Adj. R^2	0.239	0.273	0.375	0.262	0.257	0.117
Observations	1,369	3,288	1,196	3,475	1,587	4,437
State-of-living FE	Y	Y	Y	Y	Y	Y
Election-window FE	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y
Socio-economics	Y	Y	Y	Y	Y	Y
Political attitudes	Y	Y	Y	Y	Y	Y
Entropy balancing	Y	Y	Y	Y	Y	Y

Note: Table displays estimates for OLS regressions. Standard errors (SE) clustered by state of living × survey round are reported in square brackets. Estimates capture the effect of an election information shock of an election held in region X on respondents in region Y, separately for positive and negative shocks. Elections in *West* refer to the elections in HH, HB, SL and (SH, NW). Elections in *East* refer to the elections in SN and (MV, BE). Respondents in *West* and *East* comprise inhabitants in the respective states. *** p<0.01, ** p <0.05, * p<0.1.

Alternative poll data

Although the Politbarometer provides us with rigorously calculated poll data, we want to make sure that our results are not driven by the specific figures as taken from this source. Luckily, there is a number of other opinion research institutes which provide similar poll data at the federal level, using the *Sunday Question* or a comparable question. Table C.5 lists the latest federal-level AfD poll before the indicated state election as published by the respective institute. Interestingly, all institutes find fairly similar support for the AfD, compared to the hitherto used Politbarometer poll. Hence, this data is not out of line. Yet, we repeat our estimation using the federal-level polls as calculated by INSA (*Institut für neue soziale Antworten*, Institute for New Social Answers). As

can be seen in table C.5, INSA predicted clearly higher AfD support than all other institutes. Located in Erfurt in the Eastern part of Germany, INSA has been subject to media controversies, suggesting a certain ideological proximity to the AfD. Thus, the treatment for voters taking these polls as their point of reference is much smaller than when relating the actual election outcome to the Politbarometer poll. We therefore check whether our treatment effect persists even when using the deviation of AfD vote shares from INSA polls.

Table C.5: Pre-election polls, all institutes.

State	**AfD vote share**	**Pre-election poll**						
		Politbarometer	**Allensbach**	**Emnid**	**Forsa**	**GMS**	**Infratest dimap**	**INSA**
SN	9.7	4.0	6.5	5.0	5.0	4.0	5.0	5.0
HH	6.1	6.0	6.0	6.0	6.0	6.0	7.0	7.5
HB	5.5	6.0	6.0	6.0	4.0	5.0	6.0	6.0
BW	15.1	10.0	10.5	11.0	9.0	11.0	11.0	11.5
RP	12.6	10.0	10.5	11.0	9.0	11.0	11.0	11.5
ST	24.3	10.0	10.5	11.0	9.0	11.0	11.0	11.5
MV	20.8	11.0	10.0	12.0	12.0	9.0	14.0	14.5
BE	14.2	11.0	10.0	14.0	13.0	13.0	14.0	15.0
SL	6.2	9.0	8.5	9.0	9.0	9.0	11.0	11.5
SH	5.9	8.0	7.0	9.0	8.0	9.0	10.0	9.0
NW	7.4	8.0	7.0	8.0	7.0	9.0	10.0	9.0

Note: Pre-election poll refers to the last federal-level poll before the respective election date as published by the indicated institute. SN = Saxony, HH = Hamburg, HB = Bremen, BW = Baden-Wuerttemberg, RP = Rhineland-Palatinate, ST = Saxony-Anhalt, MV = Mecklenburg-Hither Pomerania, BE = Berlin, SL = Saarland, SH = Schleswig-Holstein, NW = North Rhine-Westphalia.

Models (1) and (2) in table C.6 show that this is in fact the case. Splitting the sample into positive and negative shocks according to the alternative polls, we again find a significant positive treatment effect of positive shocks but no effect for negative shocks. Note that the sets of shocks are not the same as the Politbarometer poll underestimated the AfD result in Hamburg while the INSA poll overestimated it. Likewise, the Politbarometer underestimated AfD vote shares in the combined elections in Mecklenburg-Hither Pomerania and Berlin whereas the deviation from the INSA poll is ambiguous. The

significantly positive treatment effect of larger-than expected AfD vote shares can thus be considered robust to the inclusion of different poll data.

Table C.6: Alternative poll data.

	Deviation from federal INSA poll	
DV: AfD vote intention	Positive shocks (1)	Negative shocks (2)
Post-election	0.0263	-0.000785
	[0.00565]	[0.00354]
Cluster-robust SE, *p-value*	0.000***	0.825
WCRB by state of living, *p-value*	0.002***	0.862
WCRB by survey round, *p-value*	0.021**	0.931
Adjusted R^2	0.24	0.23
Observations	7,126	10,695
State-of-living FE	Y	Y
Election window FE	Y	Y
Demographics	Y	Y
Socio-economics	Y	Y
Political attitudes	Y	Y
Entropy balancing	Y	Y

Note: Table displays estimates for OLS regressions. Cluster-robust standard errors (SE) clustered by state of living × survey round reported in squared brackets. Election information shocks are defined here according to the deviation of the AfD election outcome from the most recent estimated AfD vote share at the federal level as published by the *Institute for New Social Answers*, INSA). Election windows included in model (1) are: SN, (BW, RP ST), (MV, BE). Election windows included in model (2) are: HH, HB, SL, (SH, NW). *** p<0.01, ** p <0.05, * p<0.1.

D Supplementary material

Time line of events

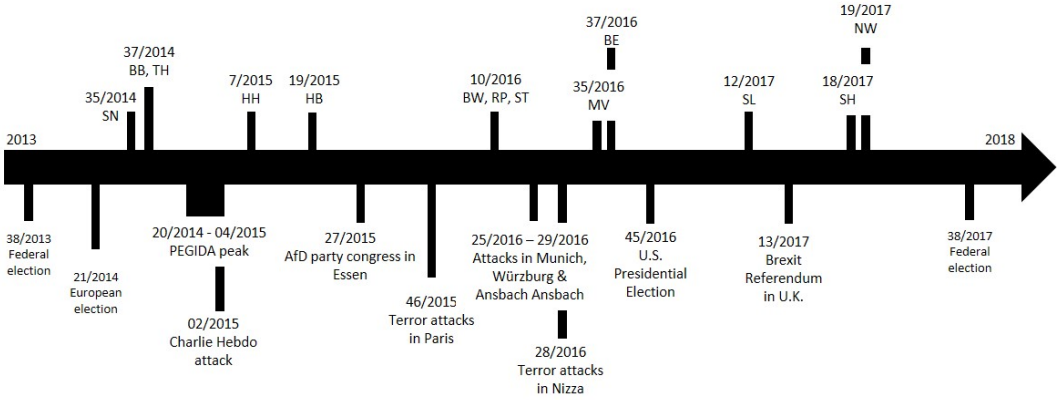


Figure D.1: Time line of events.

Covariate imbalance statistics for election windows

Table D.1: Imbalance statistics for election window 1: Saxony.

Effective treated units: 1615 Effective control units: 1699		Treatment Unconditional		Control Unconditional		 After balancing		Imbalance Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.512	0.250	0.506	0.250	0.512	0.250	0.06
Age	1-10	7.729	4.625	7.556	5.047	7.729	4.625	0.173
Married	0-1	0.589	0.242	0.604	0.239	0.589	0.242	-0.015
Low education	0-1	0.198	0.159	0.194	0.156	0.198	0.159	0.000
Medium-level education	0-1	0.360	0.231	0.376	0.235	0.360	0.231	-0.016
In school	0-1	0.002	0.002	0.004	0.004	0.002	0.002	-0.002
Part-time employed	0-1	0.126	.110	0.140	0.121	0.126	0.110	-0.014
Marginally employed	0-1	0.002	0.002	0.002	0.002	0.002	0.002	0.000
Unemployed	0-1	0.025	0.024	0.022	0.021	0. 025	0.024	-0.003
In vocational training	0-1	0.029	0.028	0.031	0.030	0.029	0.028	-0.002
Retired	0-1	0.362	0.231	0.334	0.223	0.362	0.231	0.028
Other employment status	0-1	0.032	0.031	0.039	0.037	0.032	0.031	-0.007
State of living	1-17	9.704	21.83	9.644	21.72	9.704	21.83	0.060

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.2: Imbalance statistics for election window 2: Hamburg.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.478	0.250	0.467	0.249	0.478	0.250	0.011
Age	1-10	7.747	4.549	7.746	4.293	7.747	4.549	0.001
Married	0-1	0.618	0.236	0.630	0.233	0.618	0.236	-0.012
Low education	0-1	0.207	.164	.213	0.168	0.207	0.164	-0.006
Medium-level education	0-1	0.366	0.232	0.355	0.229	0.366	0.232	0.011
In school	0-1	0.004	0.004	0.006	0.006	0.004	0.004	-0.002
Part-time employed	0-1	0.112	0.100	0.129	0.112	0.112	0.100	-0.017
Marginally employed	0-1	0.001	0.001	0.001	0.001	0.001	0.001	0.000
Unemployed	0-1	0.029	0.028	0.022	0.022	0.029	0.028	0.007
In vocational training	0-1	0.024	0.024	0.014	0.014	0.024	0.024	0.010
Retired	0-1	0.371	0.233	0.351	0.228	0.371	0.233	0.005
Other employment status	0-1	0.035	0.034	0.030	0.029	0.035	0.034	0.005
State of living	1-17	9.847	21.16	9.601	21.8	9.847	21.16	0.246

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.3: Imbalance statistics for election window 3: Bremen.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.484	0.250	0.481	0.250	0.484	0.250	0.003
Age	1-10	7.668	4.729	7.741	4.664	7.668	4.729	-0.073
Married	0-1	0.587	0.243	0.627	0.234	0.587	0.243	-0.040
Low education	0-1	0.185	0.151	0.180	0.148	0.185	0.151	0.005
Medium-level education	0-1	0.353	0.228	0.374	0.234	0.353	0.228	-0.021
In school	0-1	0.005	0.005	0.007	0.007	0.005	0.005	-0.002
Part-time employed	0-1	0.122	0.108	0.133	0.115	0.123	0.108	-0.011
Marginally employed	0-1	0.003	0.002	0.002	0.002	0.003	0.003	0
Unemployed	0-1	0.020	0.020	0.026	0.025	0.020	0.020	-0.006
In vocational training	0-1	0.031	0.030	0.023	0.023	0.031	0.030	0.008
Retired	0-1	0.353	0.228	0.362	0.231	0.353	0.228	-0.009
Other employment status	0-1	0.032	0.031	0.027	0.027	0.032	0.031	0.005
State of living	1-17	9.703	21.86	9.823	21.36	9.704	21.86	-0.120

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.4: Imbalance statistics for election window 4: Baden-Wuerttemberg, Rhineland-Palatinate, Saxony-Anhalt.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.454	0.248	0.470	0.249	0.454	0.248	-0.016
Age	1-10	7.493	5.202	7.606	4.791	7.493	5.202	-0.113
Married	0-1	0.611	0.238	0.593	0.241	0.611	0.238	0.018
Low education	0-1	0.15	0.128	0.171	0.142	0.15	0.128	-0.021
Medium-level education	0-1	0.372	0.234	0.363	0.231	0.327	0.234	0.009
In school	0-1	0.005	0.005	0.005	0.005	0.005	0.005	0
Part-time employed	0-1	0.117	0.103	0.12	0.106	0.117	0.103	-0.003
Marginally employed	0-1	0.001	0.001	0.002	0.002	0.001	0.001	-0.001
Unemployed	0-1	0.015	0.014	0.021	0.02	0.015	0.014	-0.006
In vocational training	0-1	0.027	0.027	0.022	0.022	0.027	0.023	0.005
Retired	0-1	0.335	0.223	0.351	0.228	0.335	0.223	-0.016
Other employment status	0-1	0.032	0.031	0.034	0.033	0.032	0.031	-0.002
State of living	1-17	9.762	21.24	9.762	21.51	9.762	21.24	0

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.5: Imbalance statistics for election window 5: Mecklenburg Hither-Pomerania.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.492	0.25	0.465	0.249	0.492	0.25	0.027
Age	1-10	7.634	4.841	7.612	5.13	7.634	4.842	0.022
Married	0-1	0.582	0.243	0.551	0.248	0.582	0.243	0.031
Low education	0-1	0.169	0.141	0.174	0.146	0.169	0.141	-0.005
Medium-level education	0-1	0.372	0.234	0.364	0.232	0.372	0.234	0.008
In school	0-1	0.004	0.004	0.003	0.003	0.004	0.004	0.001
Part-time employed	0-1	0.116	0.103	0.11	0.098	0.116	0.103	0.006
Marginally employed	0-1	0.001	0.001	0.001	0.001	0.001	0.001	0
Unemployed	0-1	0.02	0.02	0.019	0.018	0.02	0.02	0.002
In vocational training	0-1	0.023	0.023	0.031	0.03	0.023	0.023	-0.008
Retired	0-1	0.349	0.228	0.344	0.226	0.394	0.228	0.05
Other employment status	0-1	0.036	0.034	0.032	0.031	0.035	0.034	0.002
State of living	1-17	9.643	21.48	9.673	22.07	9.665	21.51	-0.03

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.6: Imbalance statistics for election window 6: Saarland.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.454	0.248	0.474	0.25	0.454	0.248	-0.02
Age	1-10	7.478	5.334	7.638	4.736	7.478	5.334	-0.16
Married	0-1	0.58	0.244	0.587	0.243	0.58	0.244	-0.007
Low education	0-1	0.181	0.148	0.164	0.137	0.181	0.148	0.017
Medium-level education	0-1	0.347	0.227	0.354	0.23	0.347	0.227	-0.007
In school	0-1	0.004	0.004	0.007	0.007	0.004	0.004	-0.003
Part-time employed	0-1	0.134	0.116	0.123	0.108	0.134	0.116	0.011
Marginally employed	0-1	0.003	0.003	0.001	0.001	0.003	0.003	0.002
Unemployed	0-1	0.018	0.017	0.017	0.016	0.018	0.017	0.001
In vocational training	0-1	0.036	0.035	0.025	0.024	0.036	0.035	0.011
Retired	0-1	0.329	0.22	0.338	0.224	0.329	0.22	-0.009
Other employment status	0-1	0.033	0.032	0.031	0.03	0.033	0.032	0.002
State of living	1-17	9.477	21.32	9.571	21.8	9.477	21.32	-0.094

Note: Descriptive statistics and imbalance before and after entropy balancing.

Table D.7: Imbalance statistics for election window 7: Schleswig-Holstein, North Rhine-Westphalia.

		Treatment		Control				Imbalance
		Unconditional		Unconditional		After balancing		Unconditional
	Range	Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean
Gender	0-1	0.46	0.249	0.45	0.248	0.46	0.249	0.01
Age	1-10	7.617	4.825	7.508	4.985	7.616	4.827	0.109
Married	0-1	0.617	0.237	0.603	0.24	0.617	0.237	0.014
Low education	0-1	0.188	0.152	0.155	0.131	0.187	0.152	0.033
Medium-level education	0-1	0.348	0.227	0.358	0.23	0.348	0.227	-0.01
In school	0-1	0.005	0.005	0.003	0.003	0.005	0.005	0.002
Part-time employed	0-1	0.116	0.103	0.107	0.096	0.116	0.103	0.009
Marginally employed	0-1	0.003	0.003	0.001	0.001	0.003	0.003	0.002
Unemployed	0-1	0.024	0.023	0.016	0.016	0.024	0.023	0.008
In vocational training	0-1	0.025	0.024	0.04	0.04	0.025	0.024	-0.015
Retired	0-1	0.324	0.219	0.331	0.221	0.324	0.219	-0.007
Other employment status	0-1	0.033	0.032	0.029	0.028	0.033	0.032	0.004
State of living	1-17	9.264	21.49	9.423	22.09	9.264	21.5	-0.159

Note: Descriptive statistics and imbalance before and after entropy balancing.

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University of Münster
CIW – Center for Interdisciplinary Economics
Scharnhorststrasse 100
D-48151 Münster

phone: +49-251/83-25329
fax: +49-251/83-28429

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