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Changes in Well-Being Around Elections*

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Abstract

Elections constitute the essential element of democracy, yet surprisingly little is known about their immediate consequences for individual well-being. Cross-country empirical evidence is particularly absent for the campaign period leading up to elections. While elections as a process allow citizens to contribute to democratic quality, they are also intrinsically conflictual and require voters to exert effort to make informed decisions. To measure the aggregate changes in well-being along the entirety of the electoral process, I use survey data from before and after 148 national elections in 24 European countries between 1989 and 2019. Respondents interviewed in the months preceding election day report significantly lower levels of life satisfaction than their compatriots asked the same calendar week but in years without elections. Once voting has taken place, aggregate well-being immediately returns to its regular average. Exploratory analyses suggest that partisan conflict and social pressures regarding democratic participation may play a role in explaining the reduction in life satisfaction before elections.

JEL classifications: D72, D91, I31

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1 Introduction

Whether democracy positively affects individuals' lives has been of long-standing interest to numerous scholars.[1] In contrast, how democratic elections themselves affect individual well-being has received little attention until very recently.[2] A growing number of researchers have started investigating if and how long the outcomes of elections and popular votes impact winners and losers differently.[3] For example, studies for the 2016 EU membership referendum in the UK (Powdthavee et al., 2019) and the 2016 US Presidential election (Lench et al., 2019; Pinto et al., forthcoming) show that the eventually victorious option's supporters may experience relatively short-lasting gains in well-being, while the negative consequences for those among the losing side tend to be somewhat more enduring. In the aggregate, the effects of election outcomes appear to dissipate relatively quickly, though (Dolan et al., 2008 and Pierce et al., 2016 report similar results for different UK and US elections).

Even less systematic evidence exists for aggregate well-being during the period before election day. As public engagement and interest in politics arguably reach their apex during the campaign season, feelings towards democracy more generally are likely heavily influenced by individuals' experiences during this stretch of time. Given the historically low levels of satisfaction with democracy among the public in many advanced democracies (Foa et al., 2020), the lack of knowledge regarding the consequences of the electoral process on well-being seems particularly problematic. The only available evidence I am aware of in this regard concerns almost exclusively medical outcomes. In the most extensive study, Chang and Meyerhoefer (2020) show with an RDD around voting age for Taiwanese presidential elections that health care use and expenditures increase by almost a fifth during the campaign season. For the 2009 Israeli and 2016 US elections, Waismel-Manor et al. (2011) and Hoyt et al. (2018), respectively, find elevated cortisol levels shortly before and on election night.

However, these existing studies suffer from a lack of cross-country and within-country longitudinal data. The role of different institutions, such as a country's electoral system, cannot be evaluated. Results from the study of only a single election may arise only because of a particularly contentious campaign. They may thus not even be representative of the effects of elections in the respective country. They also highlight the importance of subjective well-being as a more holistic measure of individual welfare. People may end up in the hospital with a higher likelihood or stress levels might increase before election day. However, both of these observations are also true for many activities that are clearly beneficial to individual well-being. Thus, studying the consequences of election campaigns with measures that seemingly already presume elections to have negative consequences, or at the very least, can pick up positive changes only by the

[1] The majority of research indicates that more extensive democratic rights improve outcomes for various measures (see, e.g., Frey and Stutzer, 2000; Besley and Kudamatsu, 2006; Dorn et al., 2007; Touchton et al., 2017).

[2] See, e.g., Frey and Stutzer (2002b), Frey and Stutzer (2002a), and Stutzer (2020) for discussions of well-being as measure in general and in particular for outcomes relating to institutions.

[3] Another emerging strand in the literature, such Liberini et al. (2017) for the UK, Herrin et al. (2018) and Ward et al. (forthcoming) for the US, and Ward (2020) for 15 European Union member countries, focuses on how well-being explains voting decisions and show its high predictive power in the aggregate level for electoral outcomes.

absence of adverse outcomes, may risk drawing the wrong conclusions.

Stepping back from looking at the consequences of the electoral process for well-being only as an empirical question and also pondering the reasons why elections could even affect well-being in the first place more theoretically underscores that is not at all clear whether to expect the effects to be negative or positive. While mostly not directly related to well-being, the literature studying individual behavior around elections offers many insights and possible mechanisms to explain both increases and decreases in life satisfaction during election campaigns. Being involved in the process that sets the country's course for the future may provide individuals a positive sense of self. Contributing to democratic quality by making informed decisions in the voting booth could provide a warm-glow from the contribution to this public good. Working together with other like-minded fellow citizens during election campaigns could not only fulfill the human need for social interactions. It may also provide some utility just from the expression of one's views or even social identity. However, many of these potential mechanisms also have a downside to them. Obtaining political information and voting itself is costly. Combined with social pressures and norms to do so, individuals may suffer both from experiencing social stigmatization or internal guilt if they do not follow the aspirations of being a good democratic citizen. Social identities around politics, such as party affiliation, may work as sources of conflict with members of the political out-group in social networks and thus damage valued relationships. The heightened attention to politics potentially also produces information that contrasts with existing views and identities, thus producing cognitive dissonance or requiring costly measures to avoid information altogether.

To measure the net changes in well-being around elections, I use survey data from 86 Eurobarometer (EB) waves between 1989 and 2019 for 24 European Union (EU) member countries. The EB's long existence allows me to analyze 148 different national legislative elections during these thirty years. With the exact interview date known for approximately 1.3M respondents, I can calculate how many days are left to or have passed since the nearest election in the respective country. This enables me to follow the development of well-being over the entire year before and after the election. Due to a large number of observations from each country over many years, I then compare average life satisfaction in eight 90-day intervals within these two election years with that of respondents in the same country and same calendar week, but outside of this span of two years around a particular election. Thus, the only thing that should be systematically different in respondents' lives is whether they have been asked about their well-being near to an election.

The results reveal that individuals report strikingly lower life satisfaction levels in the three months leading up to elections. Interpreting the four-step life satisfaction scale used the EB as cardinal numbers, average life satisfaction is roughly 1% lower ($p<0.01$) than in the reference period.[4] None of the other seven 90-day intervals around the election come close to the observed changes just before elections neither in magnitude nor any usual level of statistical significance. This lack of results for any other interval implies that the negative effects of the most intense period of campaigning also dissipate very quickly. Even in the immediate aftermath of election

[4]In the final 30 days before the election, the relative decrease in life satisfaction amounts to 1.5% ($p<0.01$.)

day, individuals return to the usual satisfaction levels, at least in the aggregate.

Finally, in an explorative search for possible mechanisms behind the reduction in aggregate well-being before elections, I analyze the existence of heterogeneity therein. While this exercise can only produce at most suggestive evidence, some compelling early potential indications emerge. For one, the lower levels of life satisfaction do not appear to be a recent phenomenon, as I do not find decreases thereof to become significantly greater over time. The rise of social media and other developments regarding the political information landscape thus seem an unlikely source of the negative well-being consequences during the apex campaign seasons. With their origins likely being more tied to a fundamental aspect of democratic elections, I next study differences between electoral systems. More precisely, I repeat the analysis separately for countries with majoritarian and proportional systems and find that citizens in the former group of nations become relatively much less satisfied with their life prior to election day. Furthermore, highly educated individuals and residents of rural communities and small towns show noticeably stronger negative well-being reactions during election campaigns than their respective less educated and city-dwelling counterparts. Even though these initial results have to be taken and interpreted with caution, the existence of possibly large differences in the effects of elections across institutions and individuals offers a promising avenue for future research.

The rest of the chapter is structured as follows. In Section 2 I compile and categorize evidence from the literature on elections that may point to possible mechanisms for positive or negative consequences of the electoral process for well-being. Next, in Section 3, I discuss the data I use and the empirical design of my empirical analyses regarding the net changes in well-being around election day, the results of which I present in Section 4. Finally, I briefly go over some of the heterogeneity in the results and possible implications before offering concluding remarks in Section 6.

2 Elections and well-being

As Downs (1957) argues, fully rational individuals abstain from voting in large-scale elections because the expected utility of doing so would be negative. Yet, in democracies across the world, two thirds and more of eligible citizens defy this prediction on a regular basis. In order to explain this *paradox of voting*, scholars have proposed numerous extensions and alternative models (for an overview, see Schnellenbach and Schubert, 2015). On the one hand, expanding the possible motivations for casting a vote beyond the purely instrumental one also introduces the prospect of well-being gains from participation in the electoral process at large. On the other hand, the Downsian logic requires some actual costs to be associated with voting. Otherwise, even with an infinitesimally low likelihood of being the decisive voter, the expected utility would not be negative. Thus, the mere existence of any such costs raises the possibility of the electoral process more broadly having negative consequences for well-being. Moreover, "solving" the paradox of voting by relaxing assumptions of perfect rationality, also introduces additional possible mechanisms through which the electoral process may result in lower, or higher, aggregate well-being.

Research on the relationship between the electoral process and well-being, particularly for the

campaign period preceding election day, has been scarce thus far. Therefore, I will now discuss some possible mechanisms that may play a role for changes in well-being around elections. In Section 2.1 I examine feasible positive connections to well-being during the campaign period, while in Section 2.2 I consider potential links in the opposite direction. Section 2.3 concludes the theoretical considerations by briefly going over possible effects on well-being after voting has taken place.

2.1 Positive effects on well-being before elections

Besides normative considerations, advocates of participatory democracy have argued that extending the franchise would benefit citizens beyond instrumental utility gains from better policy outcomes. The right to vote in of itself is theorized to produce a more rational, knowledgeable, and autonomous public, and to provide individuals with a sense of purpose, efficacy, agency, and empowerment (see, e.g, Mill, 1861; Pateman, 1970; Mansbridge, 1983; Sen, 1999). Upcoming elections may further accentuate these psychic gains and translate into additional short-term increases in well-being.

2.1.1 Procedural utility

One possible avenue for the electoral process to boost well-being comes in the form of procedural utility (Frey et al., 2004). The authors propose that individuals care not only about whether the result of a process matches their preferences but also *how* the outcome comes about, i.e., they have preferences regarding the process itself. Frey et al. (2004) highlight the importance of how a process corresponds with the views that individuals have of themselves. Thus, simply being "asked" to co-determine the future direction of one's country may contribute to higher levels of satisfaction during the election campaign period.

Frey and Stutzer (2005) and Stutzer and Frey (2006) indeed find that in Switzerland, individuals report higher levels of life satisfaction if they reside in a canton with more extensive direct democratic rights. Both studies also show that the cantonal differential is lower for foreign nationals, who profit instrumentally from direct democracy but have no say in the decisions. Swiss citizens, in contrast, thus appear to derive additional utility from involvement in the procedure. It remains open to which degree the findings for initiatives and referendums also translate to the representative elections I study in this chapter. Not only do ballot votes in Swiss cantons occur with greater frequency than national elections, but direct democratic decisions also transfer greater authority to citizens and are thus especially likely to produce procedural utility.

2.1.2 Democratic participation as a public good

The image citizens cultivate of themselves could play an additional role if participating in elections is regarded as a civic duty resembling a public good.[5] Andreoni (1990) proposes that

[5] Already Downs (1957) discusses and Riker and Ordeshook (1968) expand on the concept of civic duty as a possible explanation for why the predictions of the rational voter do not appear to manifest themselves in real-world elections. More recently, François and Gergaud (2019) present empirical evidence from French elections for

individuals not only contribute to a public good because of the subsequently expected private consumption thereof but also because they derive utility — a "warm glow" — from the act of contributing itself.

Few would deny that functioning democracies require citizens to participate beyond simply casting a vote on election day. An informed public is seen as vital for democratic systems to prosper. This requires individuals to obtain at least a minimal level of political information and knowledge about the available options prior to an election. However, as no citizen can be excluded from the societal benefits of such efforts, the quality of a democracy is akin to a public good. Consequently, the incentives to free-ride by not bearing the costs of staying informed about politics are high.

Similar to the paradox of voting, reality seems to again contradict the rational choice predictions regarding the gathering of political information. Even though the average voter may not be as informed as in the democratic ideal (see, e.g, Caplan, 2008; Achen and Bartels, 2017), the existence of a market for political information indicates that for a sufficiently large share of the citizenry, contributing to the functioning of democracy appears to yield some utility in of itself. In the lead-up to election day, being politically informed becomes particularly crucial. At the same time, the focus of media coverage on the upcoming election reduces the costs of doing so. Thus, impure altruism and corresponding warm glow utility from being a good democratic citizen may result in higher aggregate well-being during election campaigns.

2.1.3 Collective action and deliberation

As a public good, the private payoffs to individual citizens from an informed electorate greatly depend on the contributions, i.e., being politically knowledgeable, from others. Apart from the long-term benefits from improved democratic quality when a greater share of the public engages in politics, well-being may thereby be elevated more immediately too.[6] As Minozzi et al. (2020) show, most political discourse occurs as a byproduct of regular social interactions and not from individuals purposefully seeking politically like-minded and similarly knowledgeable peers. If elections induce voters to seek political information, these incidental political conversations are likely not only more frequent but also of greater informative content compared to other times. Thus, the level of individual political knowledge likely increases (Mutz and Mondak, 2006), which is again linked to greater participation (see, e.g., McClurg, 2006).

Deliberation about politics may thereby itself already increase well-being by further giving individuals a positive sense of self from engaging in an important aspect of the democratic process. Increased knowledge from political discussions in social networks moreover contributes to voters being better able to align political views with their underlying preferences (Sokhey and McClurg, 2012). Individuals tend to be averse to uncertainty and ambiguity (for an overview of the topic, see Etner et al., 2012). More extensive political deliberation before elections may thus additionally increase well-being by reducing citizens' uncertainty regarding their views on particular policies and political actors.

greater civic duty (as measured by blood donations) converting into higher turnout.

[6] In general, individual political participation appears to have large multiplier effects on participation by other members of a social group (see, e.g., Nickerson, 2008; Fujiwara et al., 2016).

For people who always follow politics closely, greater political deliberation within their social group during election campaigns presents an opportunity to obtain warm glow utility by informing and assisting their peers. More individualistic motivations for political enthusiasts, such as convincing others to adopt their views or gain social status by signaling political sophistication, may also contribute to increased well-being prior to election day. Furthermore, elections provide opportunities to engage with other citizens who share the same interests and strive as a collective towards a common goal, which has been shown to be an important determinant of happiness and life satisfaction (see, e.g, Helliwell and Putnam, 2004).

2.1.4 Identity and expressive utility

Civic engagement with others for a shared purpose during election campaigns, can form the basis for or activate existing social identities. The importance of such identities has been long-established in social psychology (Tajfel, 1982) and more recently adopted in economics by Akerlof and Kranton (2000).[7] In the political realm, researchers have mostly focused on the role of more permanent identities, such as gender or ethnicity. With recent rises in elite and mass polarization, attention has shifted to politics itself becoming a source for powerful identities. Particularly in the U.S. context, partisan identity has been shown to better explain political activity of individuals than their actual views on issues or ideological self-placement (see, e.g., Huddy et al., 2015).

Michelitch and Utych (2018) and Singh and Thornton (2019) provide empirical evidence using survey data from many countries for the proximity to elections increasing partisanship. Thus, if individuals derive utility from the activation and expression of their partisan identities, we would expect to see higher levels of well-being in the lead-up to elections. Even for citizens without strong attachments to political parties, elections nonetheless provide an opening to signal their values to themselves and others. In one of the earliest attempts to explain the rational voter paradox, Fiorina (1976) proposes that individual derive utility by expressing their views with the act of voting (see Hillman, 2010, for a general overview of expressive behavior in politics).[8] If voters derive expressive utility from casting their ballot, despite doing so in secret, then this channel becomes likely even more pronounced during the campaign period with many opportunities to publicly express and signal their preferences.

2.2 Negative effects on well-being before elections

The scarce existing research on effects of election campaigns (Chang and Meyerhoefer, 2020; Waismel-Manor et al., 2011; Hoyt et al., 2018) appears to presume the consequences thereof to be negative, mental health organizations offer advice on how to withstand political events[9], and election stress seems a popular topic in mass media in the lead-up to election day. Nonetheless, systematic reflections about exactly why and how the electoral process may be detrimental to

[7] For a recent overview, see Charness and Chen (2020).

[8] There is also strong empirical evidence for expressive voting behavior. For example, Pons and Tricaud (2018) exploit the two-stage system in French legislative elections and find that expressive motivations can be sufficiently strong that some voters consciously harm their instrumental political interests.

[9] `https://www.mentalhealth.org.uk/publications/tips-mental-health-political-change`

well-being are mostly absent from these discussions. Thus, in the following, I examine how some of the insights from previous research on elections and politics may relate to a potential negative connection between election campaigns and well-being. Many of these possible channels can be seen to function almost as the opposite side of the same coin of some of the potential positive mechanisms highlighted in the previous section.

2.2.1 Pressure to vote and voting costs

While a sense of performing a civic duty might form an intrinsic reward for citizens who participate in elections, the same social norm of voting may result in substantial extrinsic pressure for other individuals. From studies on the abolition of compulsory voting, it appears that significant portions of the electorate are mainly motivated by external forces. For example, Bechtel et al. (2017) find that the introduction of compulsory voting in a Swiss canton substantially increased contemporary turnout. Yet when voting became voluntary again, participation almost immediately dropped to the pre-compulsion level.

Even when electoral abstention is not punishable by law, a large social stigma nonetheless seems to surround the refusal of casting a vote and may thereby play an important role in explaining observed turnout. Reviewing the literature on social image concerns, Bursztyn and Jensen (2017) list voting as the first example of where such considerations apply to. In a field experiment, DellaVigna et al. (2017) find, when informing individuals that an in-person survey on the following day would contain questions about past voting behavior, those who abstained in a recent election were 20% less likely to partake in the survey. If social norms surrounding democratic participation already influence interactions with strangers, then the role of social pressure is likely even more pronounced, when individuals' peers can observe voting behavior. Funk (2010) studies how the introduction of postal voting affects participation in Switzerland. In small communities, where each resident's physical presence at the voting booth was previously easily observable, the option to vote-by-mail actually decreased turnout, despite lowering the costs of voting. Similarly, Gerber et al. (2008) mailed different letters to households in advance of the 2006 primary elections in Michigan. Their results show that turnout increased most among households whose letter contained a warning that neighbors will be notified about their participation in the upcoming election. Letters only reminding recipients that voting is a civic duty, caused only a slight improvement in turnout relative to control households.

Put together, individuals faced with a constant pressure to incur the costs of voting, despite having no intrinsic preferences for doing so, may very well suffer in terms of well-being in the run-up to election day. Even individuals who do not have to fear others finding out about their abstention may exhibit lower levels of well-being because they feel a sense of shame for not intending to vote. The potential negative consequences of the social norms around voting are likely particularly high for those who already face the highest direct voting costs.[10] Even though these basic costs already lead to the rational choice prediction of total abstention, the threat of sufficiently large social sanctions may nonetheless cause citizens to accept significant costs

[10]People seem to be quite sensitive to the costs that arise from casting their ballots. Cantoni (2020), for example, finds for elections in the U.S. that an increase of only roughly 400 meters to the polling location voters are assigned to decreases turnout by up to 3 percentage points.

just to avoid them. At least for such individuals, well-being is likely lower than if there were no upcoming election.

2.2.2 Information costs and decision complexity

The choices voters face in elections can be complex with potentially high policy stakes. Voters may end up being overwhelmed by particularly difficult decisions or high numbers of available options. Nagler (2015) studies how the number of candidates in Australian elections, where voting is compulsory, affects the number of intentionally invalid votes cast. He finds a U-shaped relationship, where voters appreciate greater choice until the number of available options becomes too large and they appear unwilling to participate seriously. The results indicate that too complex decision environments may cause some voters to feel alienated by politics and thus well-being may be reduced prior to election day.[11]

Similar to how social pressure may lead voters to cast a ballot despite their "private" preferences, analogous concerns may cause overinvestment in political information gathering from an individual standpoint. Particularly among citizens with low inherent interest in politics, norms regarding political knowledge may decrease well-being before elections. With strong social norms, they either pursue acquiring political knowledge or they risk becoming a target of derision within their social group and society at large. However, social dynamics could lead to an "arms race" in which even politically engaged individuals acquiring an amount of information that is (potentially) optimal from a social, but not from an individual perspective. In an experiment with students in Mexico in the lead up to elections, Marshall (2018) shows that politically unsophisticated individuals strategically obtain political information only if they are informed that their peers will observe their level of knowledge and the social network they belong to collectively values political knowledge. Moreover, he finds the threat of making political knowledge known to their social group also induces politically interested subjects to further invest in political information gathering to signal greater sophistication. In a survey of the American public, 54% of individuals indicate that they would like to follow the news to stay informed, but doing so causes them stress (American Psychological Association, 2019).

For a considerable portion of the public, following political coverage in the media seems to be not so much a voluntary act but rather a byproduct of limited alternatives. Gentzkow (2006) measures the effects of the introduction of TV in the U.S. and detects sharp subsequent decreases in political knowledge and turnout, due to a lower focus on politics on TV than in newspapers and on the radio. In an RCT, Allcott et al. (2020) pay users to deactivate their Facebook profiles in the run-up to the U.S. 2018 midterm elections. They discover that while this reduces factual knowledge about politics and polarization, subjective well-being increases relative to the control group. However, it remains open if social media decreases welfare in general, or if the relative improvement in well-being is related to lower attention on politics during an election campaign. The authors further find that despite deactivating Facebook resulted in lower political

[11] On the other hand, Stutzer et al. (2019) find no evidence for a larger number of issues that simultaneously appear on the ballot for direct democratic votes in Swiss cantons lowering the quality of decision making. On the contrary, the authors even provide evidence for more propositions actually leading to greater satisfaction with democracy in the medium and longer term.

knowledge, the likelihood to vote in the subsequent election was nonetheless higher.

While the increased likelihood of "involuntary" exposure to political news before elections could reduce satisfaction among those who wish to avoid politics entirely, the individuals most invested in politics may also experience disutility.[12] Partisans would probably rather avoid receiving negative information about their preferred candidate or party as it otherwise results in dissonance (see Golman et al., 2017, for an overview of information avoidance in general). Piolatto and Schuett (2015), for example, build a theoretical framework where an increase in the number of media outlets available in a market results in higher turnout among independents by enabling them to make more informed decisions. Partisans, on the other hand, become better informed about the "true" ability of their preferred candidate, which can result in them staying at home on voting day. Similarly, Cowen (2005) constructs a theoretical model about the role self-deception plays in politics. Since voters do not want to feel bad about their current political affiliations and world-views, they discard even free information that would otherwise decrease their utility by damaging their self-image. Consequently, with greater (media) attention on politics before elections, it may actually be citizens nested in social groups with high levels of political sophistication and/or who have stronger attachments to their political views and parties who experience the strongest well-being reductions during election campaigns.

2.2.3 Conflict and affective polarization

Conflict is fundamental to elections as they function to resolve the competition over ideas, values, and resource allocation between groups in democratic societies. Election campaigns, as the pinnacle of this struggle, may thus cause disruptions within existing social connections and cause cleavages between different groups to flare up. Disagreement with peers about politics appears to be already sufficient to deter some individuals from political participation or from following their "true" preferences. Perez-Truglia and Cruces (2017) conduct a field experiment during the 2012 U.S. presidential election and find that informing individuals about their political donations being visible to their neighbors decreases subsequent contributions by those who have previously donated to the local minority party while having the opposite effect for individuals that support the local majority party. Also for the U.S., Rogowski (2014) presents empirical evidence that greater ideological distance between candidates running for office depresses turnout, Hersh and Ghitza (2018) show that citizens married to supporters of the opposite party are less likely to vote, and Klofstad et al. (2013) find that even non-partisan political disagreement within social networks reduces interest in politics.

As discussed before, powerful social identities can form around party attachments. While I argue above that interaction with members of the same party could potentially elevate well-being during election campaigns, discrimination against the out-group often forms a fundamental aspect of social group identities (Tajfel and Turner, 1979). Just as the proximity of elections has been shown to increase partisan attachment, election campaigns also seem to intensify affective polarization, i.e., animosity between supporters of different parties (see, e.g., Iyengar et al., 2019,

[12] Smith et al. (2019) find in a survey that 32% of individuals in the U.S. report that "exposure to media outlets promoting views contrary to [theirs] can drive [them] crazy".

for a recent overview on the topic). Hansen and Kosiara-Pedersen (2017) find that the distance in voters' preferences between their least and most liked parties steadily increases throughout the 2011 Danish election campaign. Using data from post-electoral surveys in 42 countries, Hernández et al. (forthcoming) show that affective polarization decreases as more time has passed since election day.

Research on the consequences of affective polarization shows its potential to significantly impact even completely apolitical aspects of everyday life.[13] Conducting field experiments before, during, and after the 2008 election in Ghana, Michelitch (2015) finds that only during the election stage, taxi drivers charge noncopartisan customers higher prices, while accepting lower fares from copartisans (the effects arise independently from ethnicity). Sheffer (2020) repeats dictator games just before and after the 2015 Canadian election and reports that differences in bias levels towards copartisans and noncopartisans decrease by a third within two days after the election. He shows that this mainly comes from reduced discrimination against the out-group. In an experiment in the U.S. with different prospects for a scholarship, Iyengar and Westwood (2015) find that partisan cues in the applications lead to greater discrimination by opposing partisans than discrimination based on similar racial cues. Even if an applicant is objectively more qualified than his rival, partisans select the lesser qualified individual about 75% of the time if they belong to the same party. The authors further hypothesize that discrimination based on partisan identity is so prevalent because, in contrast to other social identities, no social stigma surrounds such practices. In conclusion, the electoral process' potential to increase conflict between groups with potential discrimination in non-political activities and disagreement within social networks may reduce aggregate well-being before election day.[14]

2.3 Effects on well-being after elections

In contrast to the almost complete lack of empirical evidence regarding well-being during election campaigns, a small set of empirical research exists that studies well-being in the time period after elections. Given that this research generally finds only very short-lasting effects that diverge, as one would expect, based on support for either the losing or winning side of elections, I will discuss potential mechanisms for life satisfaction changes after election day only very briefly.

It seems quite straightforward that losing a competition, such as an election, likely leads to a decrease in well-being, with the opposite applying to those on the winning side. Particularly among supporters that are either heavily invested instrumentally, due to anticipated policy outcomes, or strong partisans with investment being more emotional, akin to a sports team's supporters, the changes might be relatively large. However, given the findings presented in the previous section of out-group discrimination rapidly declining once voting has taken place, for the vast majority of citizens, the impact of their partisan identity likely loses importance to their well-being rather quickly.

A non-trivial share of voters seems to regret the choices they made on election day (Bol et

[13] Mason (2015) provides evidence that high levels of affective polarization do not require similar levels of issue polarization to exist, highlighting the strong group identity aspect to the former type of political polarization.

[14] In a 2017 survey, more than a fifth of Americans indicated that a friendship they valued has been damaged by differences in political views (Smith et al., 2019).

al., 2018), which may contribute to lower life-satisfaction. On the other hand, the number of voters who feel they made the wrong decision but care enough about politics for it to impact their well-being substantially is likely quite small. A more probable cause for reductions in the post-election period could be found in a sense of shame that non-voters feel of having shirked their perceived civic duty. The same applies to less intrinsically motivated citizens but fear their peers eventually find out about their abstention or already experience some social stigma. Positive well-being changes may arise from a similar underlying mechanism — the civic duty that many attach to voting could also cause higher well-being among those for whom participation translates into a sense of civic pride in the period after election day.

3 Data and empirical strategy

I start with discussing the sources of the data used to measure well-being and the elections around which I study changes thereof in Section 3.1. Next, in Section 3.2 I explain my empirical design and the strategy for making any potential connections between well-being and the electoral process as robust as possible. Finally, I detail the selection of the final sample in Section 3.3 and provide some descriptive statistics in Section 3.4.

3.1 Data sources and initial preparation

Well-being — The Eurobarometer (EB) is a regularly occurring public opinion survey conducted in all 28 present and past European Union (EU) member states since 1973.[15] Each wave, which usually lasts for roughly two to three weeks and occurs simultaneously across all nations, asks about 1,000 residents of every EU country the same questions regarding their attitudes towards the EU and various other current and recurring topics. In 127 waves up to 2019, respondents answered the question "*on the whole, are you a) very satisfied, b) fairly satisfied, c) not very satisfied, or d) not at all satisfied with the life you lead?*". With such a measure of evaluative well-being, I am able to study how elections affect individuals more holistically than previous research which relies mostly on specific medical outcomes.[16] Moreover, the cross-country longitudinal design of the EB also enables a more systematic approach than looking only at individual elections or countries. This reduces the risk of any changes in well-being around elections being caused by unrelated events or simply measuring the unique circumstances of a particular election. Another advantage of using the EB lies in the surveys being conducted completely independent of any elections in a particular country. In contrast to other studies of public opinion that ask respondents about how specific political events make them feel (e.g., American Psychological Association, 2019; Smith et al., 2019), the EB surveys themselves should therefore not prime subjects to think about politics (and not produce a demand effect) when asked about their current life satisfaction.

Elections and dates — Before continuing with the EB data, I briefly detail the actual elections

[15] Some waves also include European countries that are not members of the EU. However, given the irregularity with which individuals in these other countries are surveyed, I remove all observations from non-EU countries.

[16] For example, attending a sporting match may cause temporarily elevated stress levels, yet concluding on this basis that such activities are bad for well-being would be quite a leap.

used in the analysis. Since political systems differ in many aspects across the 28 countries, I attempt to use as comparable elections as possible. While some countries hold direct elections for the head of state (i.e., president) and the upper chamber of bicameral systems, voters across all countries directly elect the members of the lower house (or the entirety of parliament in unicameral systems) of their respective national legislatures. Therefore, I analyze well-being only around national legislative elections and discard presidential (and Czech senate) elections. This enables the comparison of elections across countries that are all of roughly equal political importance. For the election dates, I use the NELDA data set (Hyde and Marinov, 2012) and update missing information for elections after 2015 by hand. In countries that employ two rounds in national legislative elections, I assign the date of the second round as the day of the election. In total, the original data encompasses 198 elections in 28 EU member states between 1987 and 2021.[17]

Assigning respondents to elections — The EB provides the exact date of each interview only since 1989 (except for a small number of waves in the early 2010s). Instead of using all 127 surveys going back to 1973, this limitation reduces the number of suitable waves to 86. Given the relatively large number of respondents from each country in every wave, the data still consists of about 2 million respondents. Afterward, I assign to each respondent the national election with the smallest absolute distance between the corresponding election day and the date of the interview.[18] Since the respective election day may therefore lie ahead or behind the interview date, I am able to study changes in life satisfaction along the entirety of the electoral process.

Restrictions on respondents — Since my interest lies in the link between elections and well-being, I restrict the sample to individuals who are actually eligible to vote in the closest election. Even though the share of respondents who do not possess citizenship of the country they reside in is low in the EB, non-citizens are nonetheless removed from the analysis.[19] The Eurobarometer surveys a country's population above the age of 14. I thus exclude all individuals that either had not been or would not be 18 in the year of the nearest election. These two restrictions regarding voting eligibility discard approximately 100,000 individuals. Next, as regular legislative terms vary between four and five years across countries, I keep only respondents surveyed at most 720 days before or after the closest election to their interview. This ensures that observations from countries with five-year terms that lie significantly further away from elections than would be possible in other states do not cause any disturbances. This step reduces the number of observations by roughly another 100,000.

[17] However, as I will explain later, restrictions arising from the empirical design result in the number of countries and elections used in the main analysis to be lower.

[18] I, therefore, follow a conceptually similar approach as Michelitch and Utych (2018) who study how the proximity to elections increases partisanship, Singh and Thornton (2019), Hernández et al. (forthcoming) who also look at how elections activate party attachment and polarization and Loveless (forthcoming) who investigates how the satisfaction with democracy changes for electoral winners and losers, but the latter three works focus solely on the post-election period.

[19] The nationality of the respondent is not part of the available cumulative EB data set (neither is the interview date). For this reason, all individual waves had to be combined manually. Until recently, variable names and value codes also differed between waves. Special diligence was thus paid during data consolidation to prevent findings arising from incorrectly merged data.

3.2 Empirical strategy

Discussing the next steps in preparing the data requires first explaining the empirical design used in the upcoming analysis. Two major challenges arise in attempting to establish the relationship between the electoral process and well-being. First, against which stretch of time should the level of life satisfaction in the "treated" period around elections be compared to? Hence, a suitable reference (or "control") period, during which any past or future elections should not affect current well-being (at least not directly through the electoral process itself), needs to be determined. Second, even though I have longitudinal data from up to 30 years on the country level, I only have repeated cross-sections on the individual level, as individual respondents are not observed at more than one point in time. Addressing the latter issue thus requires making observations around elections as comparable as possible to those asked during the reference period. Ideally, the only systematic difference remaining between interviewees in these two time periods is the presence of a temporally near election or the lack thereof.

3.2.1 Treatment and reference period

Treated and control periods — As described earlier, I only use observations within 720 days before or after the nearest election. In the ideal case without snap elections, this results in a four-year window around each election (two years before and two years after). I leverage this constant temporal structure around each election by dividing these 1440 days into four periods of equal lengths. This way, the period before an election and the period after an election are simply the 360 days before and the 360 days after the corresponding election takes place (see Fig. 1). I argue that this represents the maximum stretch of time during which the electoral process itself may directly influence well-being.[20] Thus, I designate respondents in this time frame as treated. The remaining two years constitute the reference period to compare against. My assumption that the electoral process itself does not affect well-being beyond these one year periods before and after an election is an empirical question. I will later check if there are not any significant differences in life satisfaction at the beginning and end, respectively, of the treated periods. Fig. 1 displays the temporal structure of this design graphically.

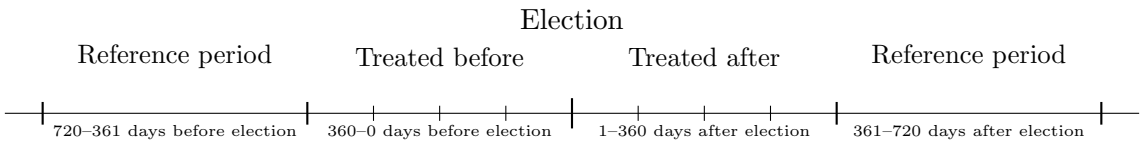


Figure 1: Overview of treated (with eight 90-day intervals) and reference periods

Notes: The entire time period can be understood as being comprised of many subsequent time blocks like the one shown in the graph. Thus, after the election above, the same figure follows again. Put differently, once the reference period of 361–720 days after the election has ended, the reference period of 720–361 days before the next election begins immediately after.

Intervals within treated period — The direct effects of the electoral process likely vary with the distance to election day. Hence, I further divide the year before and the one after the

[20] Policy outcomes and other political activity may of course still have effects (see, e.g., Di Tella and MacCulloch, 2005), but these are probably more constant over the entire political cycle, and thus should be captured with an adequate control strategy.

election into equally sized intervals. This allows me to measure how life satisfaction differs in each of these intervals around elections compared to the reference period. A trade-off presents itself regarding the choice of the length of these intervals. With the majority of voters likely to start paying close attention to upcoming elections only a few weeks before voting takes place[21], I expect the effects of elections to be most pronounced in a relatively short time span before and after elections. Hence, a length of 30 days or even less would seem appropriate for the intervals. On the other hand, choosing a too narrow band risks introducing a high degree of noise to the results, as very few or even no respondents would fall into some of these short intervals.[22] Intervals with a longer duration alleviate this issue and likely produce more stable estimates. Yet, intervals that include times when public attention to the election is still or again low, risk underestimating the effects of the electoral process. Nonetheless, I opt for the more conservative approach for the main analyses and employ 90-day intervals.[23] Consequently, I will measure the changes in life satisfaction around elections by comparing four intervals in the year before (360–271, 270–181, 180–91, and 90–0 days) and four intervals after (1–90, 91–180, 181–270, 271–360 days) election day to the reference period. The small ticks in Fig. 1 symbolize these eight intervals.

Early elections — Underlying my design so far has been the assumption that legislatures always fulfill their full terms, i.e., at least four years lie between two consecutive elections. In reality, given that the vast majority of EU member states have parliamentary systems, this ideal setting is often violated by legislatures calling for early elections. Two or more elections happening in quick succession disturb the temporal structure I outlined above. Particularly problematic are snap elections that occur so close to the previous election that they lie within the treated period after the election. Once this occurs, the complete year after cannot be observed, because, after the halfway point between the current and the upcoming early election, respondents would be assigned to the treated period before the next election. Drawing any sound conclusions regarding changes in life satisfaction during different stages of the electoral process would be difficult. Therefore, I require that at least either a full pre- or post-election year accompanies every election. In practice, this means completely removing all observations from elections that occur within 720 days of each other (including the corresponding reference periods). 25 elections[24] and a further 175,000 responses that have been assigned to these elections are lost. With this approach, I may not end up having two complete years as reference periods for each election, but it ensures that no complications arise from overlapping treated pre- and post-election years. As I observe multiple elections in every country, observations in the intervals around elections are not required to stem from the same particular election as the corresponding

[21] Furthermore, public debates between candidates, arguably the pinnacle of public and media attention during the campaign season, usually happen the final 30 days preceding the election.

[22] The shorter the intervals, the more pronounced the differences in the number of observations between intervals. Wider intervals smooth out this distribution over intervals, additionally reducing the risk that any findings arise from a mostly random variation in life satisfaction because of a small sample size in a particular interval.

[23] In sensitivity analyses I also evaluate the changes around elections using 60 and 30-day intervals. To preview, I indeed find that the results within the shorter intervals become less stable. This primarily affects intervals not in the vicinity of election day. The changes in the interval closest to the day of the vote also become more pronounced, though.

[24] The number is odd because my criteria remove all three elections in Greece between 1989 and 1990.

reference period. To provide an illustrative example, the level of life satisfaction of an individual interviewed in Belgium on November 10, 1991, i.e., 14 days before the general election held on November 24, 1991, may thus be compared with the response of another Belgian citizen questioned on November 6, 2001, which was 558 days before the corresponding nearest election, which will have taken place on May 18, 2003.

3.2.2 Attributing changes in life satisfaction to the electoral process

That treated and control respondents need not come from the same election highlights the importance of making observations between the treated and control periods as comparable as possible otherwise. I mainly rely on incorporating several fixed effects to achieve this goal.

Country-specific calendar weeks — Given the considerable differences in the level of life satisfaction across the nations in our sample, we clearly want to avoid discerning the impact of the electoral process on well-being by comparing respondents from different countries. Using country fixed effects would offer a straightforward solution to this problem. However, even within the same country, average life satisfaction among the population changes substantially, but somewhat systematically, over the course of the year. If elections in a country were always to occur during the dark and cold days of winter, well-being might be lower around elections, but in this case largely driven by seasonal effects rather than the electoral process. While the timing of elections within calendar years shifts around in most countries, the number of elections observed in each country does not reach sufficiently high levels to alleviate this concern by itself. Thus, I employ a very restrictive approach and incorporate country-specific calendar week fixed effects.[25] I therefore only consider respondents in the treated period, if observations from the same calendar week in the same country also exist in the reference period, and vice versa. Table A.1 in the Appendix provides an example for Denmark to illustrate how I compare subjects interviewed during one of the 90-day intervals around elections to respondents surveyed in the control period.

Additional controls — To further reduce the risk of detecting a merely spurious relationship between the electoral process and well-being I also include fixed effects for the year in and day of the week on which the subject was interviewed.[26] I additionally include basic individual demographic characteristics, namely age, age squared, gender, age finished education, occupation (including unemployed), and marital status as independent variables to prevent potential differences in the selection of individuals within a country from one wave to another leading to wrong conclusions. As upcoming elections may induce incumbent parties to attempt boosting the economy using the means of government in efforts to win re-election, I control for these political business cycle dynamics by including the GDP per capita growth rate and the unemployment rate in the country during the year an individual was interviewed.

[25] As a country's calendar week fixed effects sum up to the overall fixed effect for the country, there is no need to also include country fixed effects in the estimations anymore.

[26] An alternative would be to use fixed effects for EB waves, instead of years. However, this then runs into over-identification problems using EB waves that result in the fixed effects not being balanced across the dimensions. To maintain the interpretability of the fixed effects (though not necessarily needed for the current purposes), I choose to account for general shifts over time on the year level. As I will show in robustness checks later, the results do not substantially change with EB wave fixed effects.

Estimation model — The empirical strategy culminates in the linear regression model

$$Y_{iec_jjtd} = \beta interval_e + \gamma cal.\ week_{c_j} + \delta_t + \omega_d + \eta Xind_i + \rho Xecon_{jt} + \epsilon_{iec_jjtd} \quad (1)$$

where I regress the level of life satisfaction Y_{iec_jtd} of individual i in calendar week c of country j in year t on the day of the week d in interval e on a series of binary indicators regarding classification of individual i into interval e based on i's distance in days to the nearest election, calendar week c in country j, year t, and day of the week d fixed effects corresponding to the date on which i was interviewed, as well as a series of demographic characteristics $Xind$ of i and economic indicators $Xecon$ for country j in year t.[27] As I study national elections, all individuals residing in a particular country experience the same treatment, i.e., the proximity to the nearest national legislative election, at a specific point in time. Since EB waves are in the field for only about two to three weeks, I argue that each wave within a country comes closest to the level that the treatment occurs on. Thus, I cluster standard errors on the country-wave level. In all regressions, coefficients are estimated using ordinary least squares (OLS).[28]

Interval-specific country weights — The number of observations per country is roughly equal between the reference and the entirety of the treatment period. Yet within the individual intervals that make up the latter, the share of each country can be quite different from the control period. Maintaining comparability and avoiding an abnormally high number of observations from a particular country (relative to the reference period) dominating the findings for any particular interval is of importance. Therefore, I decide to employ interval-specific country weights in the estimations so that each country's weight in each interval matches the one in the reference period. Thereby, I do not change how much weight each interval as a whole receives. The adjustment concerns only the composition within the intervals, so that the relative frequencies of countries is constant across the reference period and every interval. The main consequence of this approach lies in easier and more accurate comparisons of the interval estimates to the average in the reference period when translated into relative changes. Ideally, the use of weights also reduces the noisiness of the estimates between intervals. In later robustness checks, I show that my main findings regarding the changes in life satisfaction around elections do not fundamentally change when these weights remain unused.

All in all, while my chosen approach still yields results of a correlational nature, it is difficult to imagine reasons for why life satisfaction should systematically differ between two otherwise similar respondents from the same country and calendar week. The sole systematic difference that remains is that one observation stems from an election year and the other being removed at least one year from the closest election. Thus, unless the electoral process were indeed to have any positive or negative net consequences for well-being, I expect to find no differences in the intervals closest to an election. The large number of interviewees from many countries and

[27] With the use of intervals and multiple sets of fixed effects, and given the later analysis of heterogeneity between groups of individuals and countries, I do not necessarily expect the treatment effects of the electoral process to be homogeneous. Thus, problems with negative coefficient weights as de Chaisemartin and D'Haultfœuille (2020b) describe, may potentially arise. de Chaisemartin and D'Haultfœuille (2020a) further advance this to designs with staggered treatments and is thus more applicable to the empirical strategy I pursue in this chapter.

[28] Accordingly, in the few specifications where the four-step life satisfaction scale is not interpreted cardinally, but instead binary satisfaction outcomes are used, a linear probability model is estimated.

years should also reduce concerns that any findings are driven by reasons unrelated to elections. Positive and negative exogenous shocks to life satisfaction, but orthogonal to elections, should off-set themselves over the entire sample. If the empirical strategy works and if the electoral process itself has only relatively short-lasting effects on life satisfaction, as argued above, then we should see no systematic differences in intervals further away from election day.[29]

3.3 Selection of final sample

Returning to the data, my requirement for country-specific calendar weeks to have observations in both the treated election years and the reference period eliminates an additional 160,000 individuals from the sample.[30] This step also results in not all countries being observed in each of the eight 90-day intervals around elections that I employ in my main specification. Specifically, this concerns four — Croatia, Greece, Romania, and Sweden — out of all 28 countries. As this means that the selection of countries would differ across intervals, I decide no to use these countries in the analysis.[31] Therefore, I do not use the roughly 170,000 observations from these four nations that have met the other inclusion criteria and end up with my final sample that consists of 1,283,035 respondents surveyed in the remaining 24 countries between 1989 and 2019.

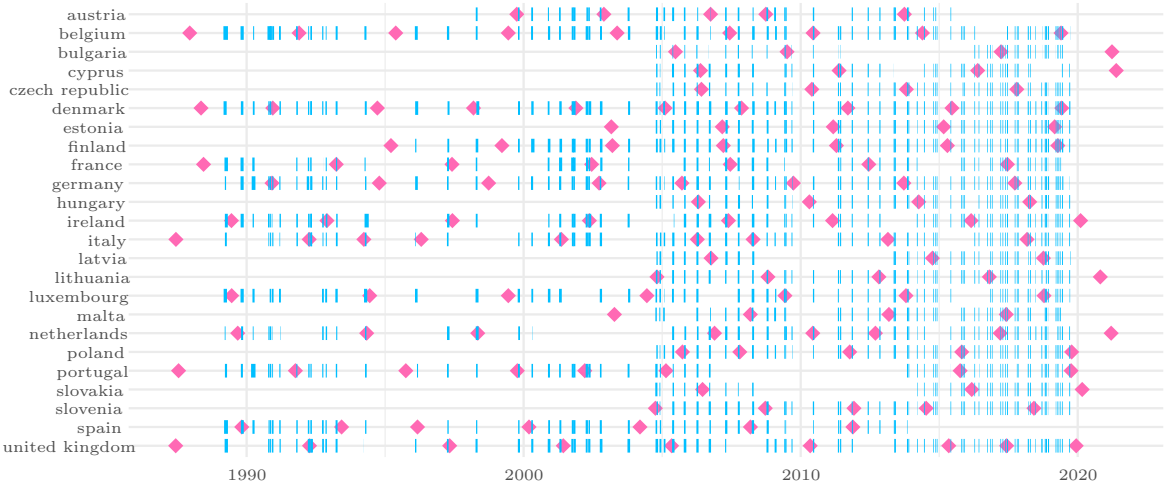


Figure 2: Overview of Eurobarometer waves and national elections

Notes: Blue rectangles represent the 86 EB waves conducted in the 24 countries in our sample (for a total of 1,460 distinct country surveys). The horizontal length of each blue rectangle indicates how long a particular wave was in the field for in the respective country. Red diamonds represent the 148 national legislative elections for which observations exist in the 1,440 days surrounding the corresponding election day.

Fig. 2 provides an overview of the 86 EB waves (blue rectangles) and the 148 national legislative elections (red diamonds) in our final sample. For 137 of these elections, I observe

[29] Particularly in the intervals at the start and end, respectively, of the two treated years, the divergence to the reference period needs at least not be statistically significantly different from zero. Otherwise, observations in the reference period are likely still affected (treated) by the electoral process, which would invalidate my empirical design.

[30] Of the initial 1,316 country-specific calendar weeks, that contain at least one subject, 317 calendar weeks do not fulfill this condition.

[31] As a sensitivity check, I later repeat the main estimations with all 28 countries and find that the implications for the changes in well-being around elections do not substantially differ.

respondents in at least one of the pre- and post-election intervals. For the remaining eleven elections, respondents have only been observed during the reference period (which mostly concerns elections that will be held between one and two years after the final wave of November 2019 in my sample). Also evident from Fig. 2 is the increase in the number of waves after 2004, which coincides with the EU's first eastern enlargement. The stretches of time without any waves in some countries highlight the complete removal of observations assigned to elections occurring within 720 days of each other.

3.4 Descriptive statistics

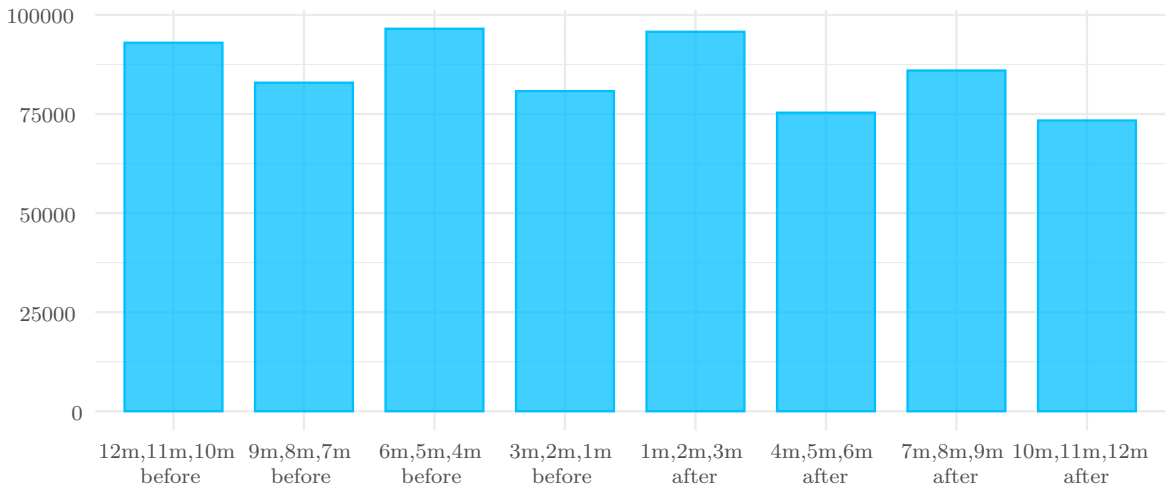


Figure 3: Number of observations across 90-day intervals around closest election

Notes: The number of observations in the reference period (0.60M) is not shown. Across all intervals, 0.68M respondents are observed in the treatment period of the electoral process.

The distribution of observations over the eight 90-day intervals is displayed in Fig. 3.[32] On average, each of the eight 90-day intervals consists of about 85,500 respondents. Slight differences in the number of observations between intervals exist even adopting a wider width of intervals, as the largest interval (180 to 91 days before the election) exceeds the smallest (271 to 360 days after the election) by roughly a quarter in terms of the number of observations. However, I do not see a straightforward reason why this should introduce some form of bias to the results (except for coefficients with fewer observations measured less accurately). For a more detailed overview, Table A.2 in the Appendix presents how many respondents are observed, and from how many different elections, in each 90-day interval per country. Figs. A.1 and A.2 in the Appendix show the classification of subjects over 60-day and 30-day intervals. Clearly, using narrower intervals leads to a much greater variation in size.[33] The histograms for different interval lengths validate

[32]The reference period, with roughly 0.60M respondents (46.7% of the total sample), is not shown so that differences between the intervals remain visible. With approximately 0.68M subjects interviewed across all intervals (53.3% of all observations), the balance between treated and control periods does not seem to be a point of concern.

[33]As I describe in Section 3.3, I exclude countries without observations in each of the 90-day intervals. If I were to do the same for 60-day and 30-day intervals, it would drastically reduce the number of countries (for 30-day intervals no country meets this requirement). Hence, I conduct the analyses for the narrower intervals with the

the choice to focus mainly on 90-day intervals for the upcoming analyses, even though findings regarding the electoral process' may underestimate its impact on well-being, compared to the shorter intervals.

Table 1: Descriptive statistics for outcome variables

	All				Ref. period			
Variable	Mean	SD	Min	Max	Mean	SD	Min	Max
Life Satisfaction (1-4)	3.028	0.736	1	4	3.025	0.735	1	4
Satisfied (3 or 4)	81.776	38.604	0	100	81.689	38.676	0	100
Not at all satisfied (1)	3.760	19.023	0	100	3.760	19.023	0	100
Very satisfied (4)	24.777	43.172	0	100	24.557	43.042	0	100
Observations	1,283,035				599,422			

As mentioned in Section 3.2.2, I mainly focus on a cardinal interpretation of the four answer possibilities in the EB life satisfaction question, assigning the values one ("not at all satisfied") to four ("very satisfied"), as my dependent variable. In addition, I later also run linear probability models for the likelihood a respondent indicates to be satisfied, i.e., answering either with "fairly satisfied" or "very satisfied", and the probabilities to be "not at all satisfied" and "very satisfied", respectively. Table 1 provides descriptive statistics on all four variables that I use to measure how the electoral process may potentially affect well-being.[34] I will use the averages of the reference period sub-sample to translate the absolute differences, that the regression coefficients represent, into relative changes in life satisfaction around elections.

4 Net changes in life satisfaction around elections

Before presenting the results of the regression analyses in Section 4.2, I start by looking at the development of average life satisfaction over the daily distance from the closest election in Section 4.1.

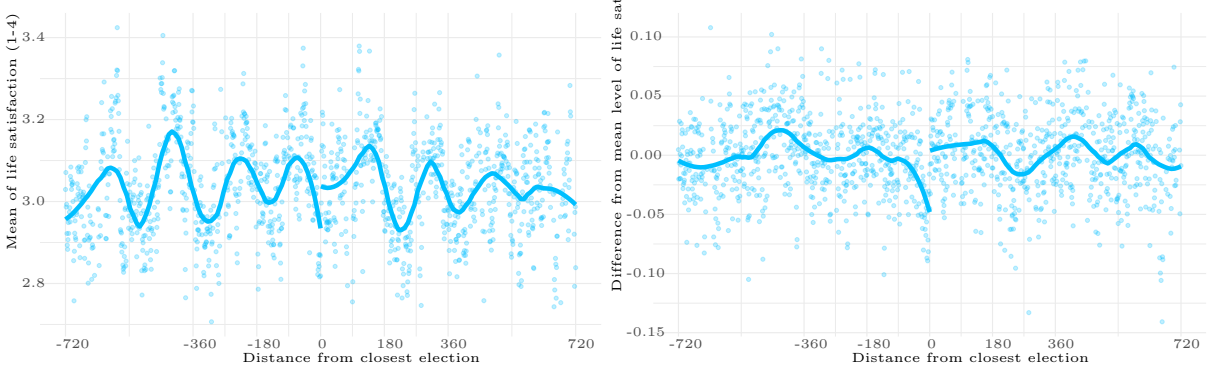
4.1 Average life satisfaction over distance to nearest election

In Fig. 4, I present the average life satisfaction for each daily distance from the respondents' nearest election. Hence, there are no designated treated or control periods and no intervals within the years around election day. Looking at panel (a) in Fig. 4, there appears to be a brief dip in average life satisfaction in the days (or even weeks) just preceding an election. In the days and weeks immediately following an election, no similar changes to well-being are noticeable. However, as the locally estimated scatterplot smoothing (LOESS) results (the continuous lines) show, this hardly represents any indication for the electoral process exhibiting any systematic impact on individuals' satisfaction with their lives. The significant oscillations over the entire

same sample of countries as used with the 90-day intervals. As a consequence, the share of each country across intervals can differ substantially.

[34] As surprising as it may seem, the share of respondents indicating that they are "not at all satisfied" is indeed exactly equal across treated and control periods.

period may primarily even be the product of systematic appearances of countries (plus seasonal patterns therein) and years during particular distances from elections.



(a) Raw mean of life satisfaction

(b) Mean of life satisfaction centered around FEs

Figure 4: Raw and demeaned life satisfaction over daily distance from nearest election

Notes: Each point represents the respective average of respondents observed on days with a particular distance from the closest election. The lines result from locally estimated scatterplot smoothing (LOESS), always using 30% of all points in the estimate for each daily distance. The LOESS are run separately for the pre- and post-election periods. In panel (b) the same fixed effects (country-specific calendar week, year of interview, day of the week) as in the later regression analyses are used to center life satisfaction. Of the 1440 days in total, I do not show the 130 days with less than 400 observations.

Panel (a) highlights the potential to wrongly attribute any changes around elections to the electoral process, rather than some completely unrelated variation. As a first step to avoid coming to wrong conclusions, I center life-satisfaction around the year of the interview, country-specific calendar weeks, and day of the week (which are the same fixed effects I later use in the regression analyses).[35] The resulting average difference from the demeaned life satisfaction over the daily distance from the closest election are displayed in panel (b) of Fig. 4. The intuitions discussed above regarding the origins of the pattern in panel (a) may thus indeed have some basis in reality. Centering life satisfaction around the set of fixed effects practically eliminates the relatively regularly occurring oscillations in the raw daily distance mean. This provides some confidence that any remaining systematic deviations in life satisfaction are quite likely indicative of some regularly occurring underlying event.

Clearly, the interest lies in whether any such systematic deviations from the mean happen around election day. One prolonged shift is indeed observable, namely in the stretch of time immediately preceding election day. The downturn of the pre-election LOESS curve delivers some early suggestive evidence that average well-being may be lower during the apex of election campaigns than it would usually be during the same time of year in the same country.[36] In

[35]If I were to average the centered life satisfaction over all intervals and the reference period, instead of just the daily distance averages, the differences between them would be equivalent to the coefficients that result from a regression using unprocessed life satisfaction only with the fixed effects and interval dummies, but no other controls. I report these results as a robustness check in column 5 of Table A.4 in the Appendix.

[36]I point out that by producing the LOESS curve separately for the periods before and after an election, the negative development of average life satisfaction prior to election day does look particularly pronounced. The apparent reversion to the mean (or even increase) of well-being immediately afterward does not factor into the estimated curve, which only considers observations until voting takes place. Since I currently do not compare the centered averages of life satisfaction against any reference period, only the difference from the respective mean of life satisfaction in the same calendar week in the same country is shown. If life satisfaction were indeed comparatively lower shortly before elections take place, then it has to be higher in the same calendar weeks without elections, once expressed as the differences to the respective average. Since countries tend to hold elections around

the period following the day of the election, no substantial discrepancy to the overall level of average life satisfaction is noticeable. At least, in the aggregate, the actual election does not appear to elevate levels of life satisfaction. Even though after an election there have to be more winners than losers (at least in most electoral systems), any potential losses in well-being for the defeated also do not appear to be so large that they outbalance the gains by the victorious side in aggregate life satisfaction. Even if having had the ability to make use of one's right to vote exerted some well-being benefits that apply to all participants, irrespective of the eventual outcome, these would seem to be nowhere near the magnitude of what the graphical evidence so far suggests occurs during the campaign period prior to election day.

4.2 Regression results

The graphical evidence in Section 4.1 provides some preliminary evidence of possible changes in well-being around elections, particularly before voting occurs. I now turn to regression analyses to try to quantify the influence of the electoral process with more precision. The results from my main specification using 90-day intervals commence the regression analyses, before I turn to the estimations using 60- and 30-day intervals, followed by robustness and sensitivity checks, with results for different binary life satisfaction outcome measures rounding out the analyses regarding the net changes of life satisfaction around elections in the aggregate.

4.2.1 Main results

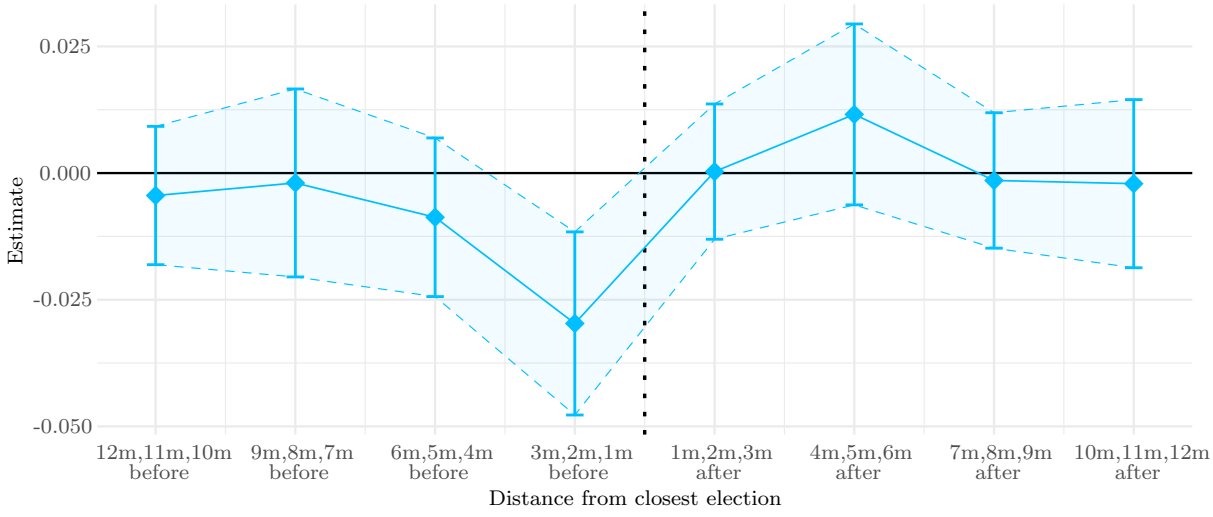


Figure 5: Net changes in life satisfaction around elections

Notes: The full regression results are shown in column 1 of Table A.3 in the Appendix. Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regression with interval-specific country weights based on 1.28M observations with an adjusted R^2 of 0.22. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

the same time of year, the two pronounced positive upticks in the difference to average life satisfaction roughly one year before and one year after the election may be taken as an indication of exactly this happening. In this case, the relative impact of the pre-election process would also likely be more negative than panel (b) of Fig. 4 suggests.

I present the coefficient estimates for each of the eight 90-day intervals (with the period of 720–361 days before and 361–720 days after an election serving as the reference category) in Fig. 5. The vertical bar represents the 95% level confidence interval of each coefficient. The results from the more refined regression analysis bolster the earlier impressions from Fig. 4. Respondents interviewed in the 90-day interval prior to a national legislative election report a level of life satisfaction that is on average 0.030 lower on the four-point scale than in the reference period. The coefficient is highly statistically significant ($p<0.01$) and represents about a 1% decrease in relative terms.[37] In none of the other seven 90-day intervals do we observe a difference in life satisfaction to the control period that comes close to being statistically different from zero or to the magnitude of the change in the immediate three months before an election. In fact, all coefficients, except for the coefficients of the third and sixth 90-day interval (the former potentially already capturing the start of the campaign season), are nearly zero.[38] This finding is especially relevant for the intervals at the beginning and end of the treated period. It seems that the requirement of none of the effects of the electoral process lasting long enough to contaminate the control period is fulfilled.[39]

My results also imply that the lower levels in well-being before elections vanish very quickly once voting has taken place. Average life satisfaction in the subsequent 90 days is almost precisely identical to the reference period level. Hence, my findings align with the set of previous empirical research that also largely sees no changes in aggregate well-being after elections.[40] In contrast to these earlier works, I am able to show that these results appear to generalize over a large number of elections in many countries and three decades.

Before moving on from my main finding, I note again that the results do not establish a definitive causal connection between the run-up to an election and lower life-satisfaction. Nonetheless, given the restrictive design and large number of elections from 24 different countries, it does appear unlikely that this drop in life satisfaction arises completely independent of the most intense stretch of election campaigning. In Section 4.3, I will further discuss these findings in the context of the theoretical mechanisms previously outlined in Section 2. As this study is to my knowledge the first attempt to systematically capture changes in well-being before election day and is indeed the only period of time where significant changes occur, I will mainly focus on this period for the remainder of the chapter. Thus, remarks regarding the results or implications thereof in the following refer to the time immediately preceding election day, unless explicitly noted otherwise.

[37] As a comparison, the decrease in life satisfaction is equivalent to roughly one-twelfth of the effect of becoming unemployed or one-eighth of being divorced, two of the largest individual-level predictors of well-being.

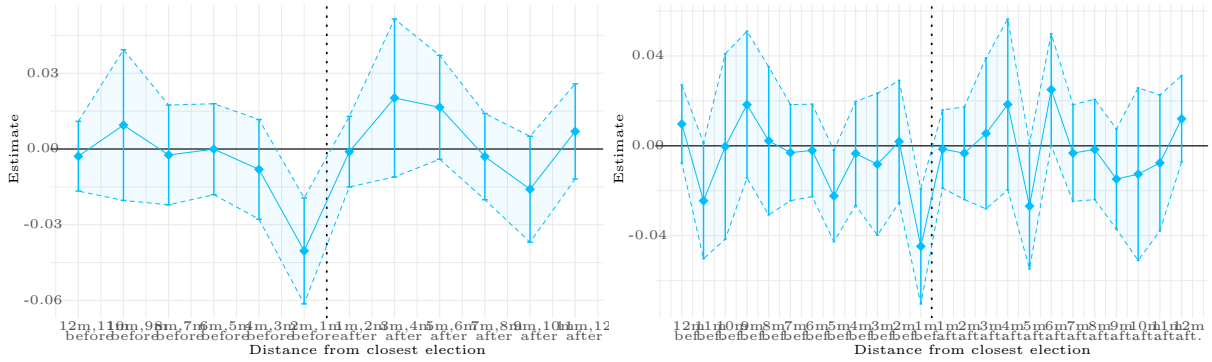
[38] The positive bump in life satisfaction in the interval consisting of respondents observed four, five, and six months after an election, seems to be almost entirely driven by Austria (as evident from Fig. A.10 in the Appendix). I do not have an adequate explanation for why the coefficient in this interval also becomes almost zero when excluding Austria. This appears to be the only case where one country has such a large impact on the results for one of the intervals.

[39] Not only are the two coefficients far away from achieving statistical significance at any normal level, but their effect sizes represent only a relative change of about one per mill to average life satisfaction in the reference period.

[40] However, the majority of these works do find some differences between the electoral winners and losers in the post-election period. At this point, the data does not allow me to similarly differentiate between the two groups, unfortunately.

4.2.2 Results for shorter intervals

As I discuss in Section 3.2.1, the choice of interval length involves a trade-off. Narrower intervals potentially capture more noise due to fewer observations within each, while wider ones potentially underestimate the impact of the electoral process. Moreover, if the upcoming election were indeed responsible for the drop in life satisfaction we observe with the 90-day intervals, then the largest changes should always occur in the respective interval closest to the election. Were we to find that the decrease in well-being actually happens 90–61 days before an election, and as such is responsible for the large reduction seen in Fig. 5, then the argument for a connection to the electoral process would clearly become more arduous. However, the coefficient estimates employing intervals of 60 and 30 days (the length of the treated period itself remains unchanged) shown in Fig. 6 clearly show this not to be the case.



(a) 60-day intervals (b) 30-day intervals

Figure 6: Net changes in life satisfaction around elections with 60-day and 30-day intervals

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 1.28M observations with adjusted R^2s of 0.22 and 0.23. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

60-day intervals — Looking at the results in Fig. 6a, I find that the only statistically significant change in life satisfaction occurs once again in the final interval before election day. The 60-day coefficient being larger than the one for 90 days, strengthens the case for the existence of a negative relationship between aggregate well-being and upcoming elections. Expressed in relative terms, whereas during the final 90 days before voting takes place individuals reported on average a 1% lower level of life satisfaction, within only the closing 60 days the relative reduction amounts to 1.3%.[41] What we do not see, even with shorter intervals, are any aggregate changes after the election. I again find a difference of almost exactly zero compared to the control period, despite looking only at a 60-day stretch. Fluctuations between the coefficients generally increase somewhat, which seem to be tied to some extent to the lower number of observations in particular intervals (shown in Fig. A.1 in the Appendix). Nonetheless, the decrease just before

[41] I also note that in the penultimate 60-day interval before the election, half of which makes up the first third of the 90-day interval of interest, no statistically or economically relevant difference to the reference period exists. While also more stable, the 90-day results seem therefore to underestimate the effects of the electoral process. This is likely due to them also encompassing a period of time during which the upcoming election does not yet play a meaningful role in most citizens' lives.

an election remains almost twice the size of the next-largest coefficient. The coefficients for the first and last intervals do not deviate substantially from zero, thus further alleviating concerns about effect decay into the control period.

30-day intervals — Next, I reduce the number of days per interval from twelve 60-day to twenty-four 30-day intervals in the treatment period. Fig. 6b presents the corresponding estimates, which lend additional support to the existence of a link between satisfaction with life and the electoral process. As anticipated, we see rather large swings in life satisfaction differentials between the intervals containing only 30 days. Yet only a single other interval (five months before) beyond the 30 days just before the election is actually statistically different from zero at the 95% confidence level (though with a substantially smaller effect size). Across all intervals, both in terms of statistical significance (the final interval before election day is significant at the 99.9% level), as well as the magnitude of the estimate, the reduction in average life satisfaction during the last 30 days stands out. Despite other estimates quite clearly picking up more noise than the comparatively stable 90-day and 60-day intervals, the downturn immediately prior to election day is more than 1.5 times the size of the next largest coefficient. The 30-day results further emphasize that the potential adverse effects of elections are short-lived and arise only during the very end of the campaign season. Compared to the longer intervals, the magnitude of the decline in well-being again becomes larger. During the ultimate month before voting occurs, individuals report an average level of satisfaction with their lives that is 1.5% lower than in the reference period. [42]

4.2.3 Robustness and sensitivity checks

The consistency with which life satisfaction differs most from the control period during the respective final interval before election day strongly indicates a negative connection between well-being and election campaigns. That these results actually represent the effects of some events that are wholly unrelated to elections and just happen to always occur in the month prior to 137 different elections from 24 countries across 30 years seems quite unlikely. However, with the negative changes being quite substantial and the novelty of the subject matter preventing comparisons between my results and (non-existing) previous findings, I now perform a variety of robustness and sensitivity checks.

Stepwise addition of independent variables — I start by gradually adding different sets of fixed effects and individual and country-level controls to the most basic specification using only the eight 90-day intervals.[43] Columns 1 to 6 in Table A.4 in the Appendix display the corresponding results, with column 7 being the regression that produces the estimates in Fig. 5. Column 1 shows that even without any additional independent variables, the reduction in life satisfaction before elections is still present, but the coefficient is much smaller and very imprecisely estimated.

[42] Expressing this expected reduction in the level of aggregate well-being during the Ft30-day run-up to election day somewhat pointedly, the decrease in average life satisfaction is roughly equal to one-eighth of the electorate losing their job, or every sixth married couple divorcing. The latter calculation follows from it taking two individuals for separation to be possible and the effect of elections in the final 30 days being about one-sixth of changing from married to divorced.

[43] Just as in the main regression, I always incorporate interval-specific country weights. I discuss the impact of these weights below.

Adding year dummies in column 2 and then subsequently also "overall" country fixed effects in column 3 delivers similar results. The estimate for the final pre-election interval only becomes substantially larger (and statistically significant) once adjusting not only for differences in the level of life satisfaction *between* countries but also *within* the same country over the time of year with country-specific calendar week fixed effects in column 4. The change in size due to this step (in absolute terms) is substantially larger than in any other interval, which may be taken as a further indication of a connection between the electoral process and well-being.[44] No similar changes emerge from this step for the period right after election day, which can be taken as further evidence for the aftermath of elections having no impact on aggregate well-being. Adding day of the week fixed effects, individual and economic controls only negligibly alters the results.[45] Finally, column 8 presents the results that arise when replacing year with wave fixed effects. While doing so reduces the size of the effect, the changes in well-being that I attribute to the electoral process remain nonetheless highly statistically significant.

Use of weights — Next, I repeat the main regressions without using interval-specific country weights. Figs. A.7 and A.8 in the Appendix show these results. Without weights, the coefficients before elections become slightly smaller in the estimations using 90-day and 60-day intervals but actually increase in size with 30-day intervals.[46] The main finding regarding the reduction in life satisfaction before election day remains unchanged across all interval lengths irrespective of the use of weights.

Random assignment of treatment — As a further check of whether the observed decrease in well-being is indeed related to the upcoming election, I randomly reshuffle the actual distances in days to the nearest election in the sample across individuals.[47] Based on these randomly assigned distances, I then reclassify respondents into either the reference period or one of the eight 90-day intervals. Essentially, this approach equals a randomly assigned treatment across the otherwise completely unaltered observations. Afterward, I run the identical regression as the one that produces the results shown in Fig. 5. I repeat this entire process, i.e., reshuffling, reclassifying into intervals, and the regression, 1,000 times. Fig. A.9 in the Appendix shows the estimates from each of the 1,000 repetitions for each interval as black dots. Blue dots portray

[44] No event that I am aware of consistently occurs at the same relative point in time before or after elections and would thus always be contained in the same interval. Therefore, fluctuations in life satisfaction within the remaining intervals are likely already quite random. The addition of the country-specific calendar week fixed effects then does not fundamentally reduce this randomness in the absence of a systematic treatment like the election day.

[45] That the coefficient becomes smaller, albeit only very slightly, once economic conditions are held constant, is rather surprising at first. Given the likely better economic conditions due to political business cycles in the year of the election, one would expect individuals, ceteris paribus, to be more satisfied during election years. Possibly, governments are more likely to collapse in economic downturns, causing early elections. If this potential channel dominates the effects of political business cycles, election years would be correlated with comparatively *worse* economic conditions rather than better ones. However, both possibilities highlight the need to account for economic factors in the given year.

[46] Besides better comparability of the effects relative to the reference period, one of the reasons for using these weights in the first place was to reduce big swings in some intervals that arise because of very different country shares therein. The comparison with the unweighted regression shows that this goal is generally achieved.

[47] Reassigning *election dates* across individuals and then calculating the new distance, could lead to "nearest" elections being assigned to observations that were interviewed up to 30 years before or after this election date. Since I limit the analysis to at most 720 days before and after an election, this would drastically reduce the number of observations available for the estimations.

the average of all coefficients for a particular interval, with the 95% significance level confidence intervals represented by dashed blue lines. As expected, when respondents are classified into the interval just before an election takes place completely by chance, there is, on average, almost exactly a difference of zero in life satisfaction before these "phantom elections" in comparison to the "reference period". For the other seven intervals, the same applies. Even with 1,000 repetitions, the coefficient from the run that produced the largest difference from zero in the relevant interval was only about -0.0085. This coefficient is less than a third of the estimate for the final 90 days in the main regression with the true distances to the nearest election. The random assignment of treatments thus provides additional strong evidence for the suggestion that the reduction in well-being is linked to the electoral process.

Influence of individual countries — In the last category of checks, I test the sensitivity of the results to the inclusion and exclusion of different countries. First, I repeat the main regression analysis, but this time leave each country out once and report these results in Fig. A.10 in the Appendix.[48] Across all these 24 regressions, life satisfaction remains statistically different from zero at the 95% significance level in the interval prior to election day. No other interval ever attains this level of statistical significance. My findings are therefore not solely driven by a single country. As some differences between the countries nonetheless appear to exist, the possible role of electoral institutions comes to light. I will briefly analyze and discuss some of these differences in Section 5 along with other potential sources of heterogeneity in the changes in well-being around elections. Finally, including the four countries thus far excluded, as explained in Section 3.3, does not fundamentally change the findings, as the corresponding results in Figs. A.11 and A.12 in the Appendix show.

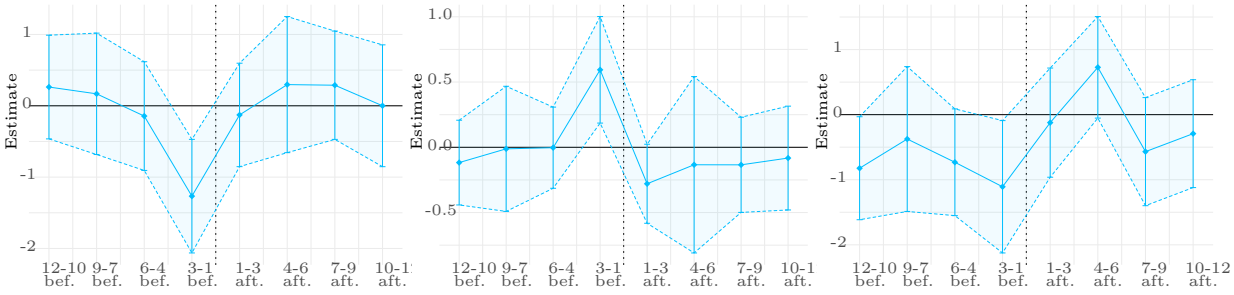
4.2.4 Results for binary life satisfaction outcomes

So far, we have seen evidence for election campaign periods to reduce aggregate well-being. Using the four steps in the life satisfaction scale as cardinal numbers leads to the interpretation of the results becoming somewhat abstract, though. Therefore, I also perform the same analyses as before using three different binary outcomes for life satisfaction. Employing a linear probability model, I estimate how the likelihood for respondents of being "satisfied" (either "fairly" or "very" satisfied), "not at all satisfied", and "very satisfied" changes around elections. Fig. 7 presents the results for these three outcomes with our main interval length of 90 days.

Satisfied — Beginning with the likelihood to report being either "fairly" or "very satisfied", Fig. 7a shows that over the 90 days before an election, the probability to do so is 1.27 percentage points lower than in the reference period. In the latter stretch of time, on average 82% of respondents indicate they are at least mostly satisfied with their life, so this corresponds to a 1.6% decrease.[49]

[48] Diamonds in green indicate that leaving the particular country out changes the sign of the coefficient compared to the original regression (the estimates of which are always marked by black crosses), red symbolizes a coefficient with the same sign but moving closer to zero, blue means the coefficient moves further away from zero if the country is excluded. The opacity of the coefficient represents how much the coefficient from the specific regression changes in absolute terms relative to the largest absolute shift (which has zero transparency) in each interval over all 24 regressions.

[49] As with the cardinal interpretation of the life satisfaction measure, I repeat the analyses with binary outcome



(a) Fairly or very satisfied (b) Not at all satisfied (c) Very satisfied

Figure 7: Net changes around elections with binary life satisfaction measures

Notes: The full regression results are shown in columns 2–4 of Table A.3 in the Appendix. Average likelihoods in the reference period (720–361 days before and 361–720 days after elections) are 0.82, 0.04, and 0.25. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 1.28M observations with adjusted R^2s of 0.15, 0.06, and 0.17. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

Not at all satisfied — Fig. 7b displays the estimates for the likelihood to respond with the lowest level of life satisfaction, i.e., "not at all satisfied". Among all results for the net changes before election day, I find the largest effects for this outcome. Relative to the 4% average probability to indicate being not at all satisfied in the control period, the coefficient of 0.59 corresponds to an increase thereof during the final 90 days before an election of almost 16%.[50] Interestingly, the lowest level of life satisfaction is the only measure for which aggregate well-being moderately increases directly after elections. Because the share of individuals who actually tell interviewers that they are not at all satisfied is very low in general, I would advise caution against putting too much importance on this outcome measure, though.[51]

Very satisfied — Whether subjects indicate feeling very satisfied with their life rounds out our analyses for the binary dependent variables. Fig. 7c shows that compared to the reference period, in the final stretch before elections, individuals are also roughly 4.5% (-1.11 percentage points) less likely to answer with the highest option on the life satisfaction scale. In contrast to the other outcomes, the discrepancy in probability to the reference period is only barely statistically different from zero at the 95% level. Almost all other estimates for the rest of the 90-day intervals are quite far away from zero (though not in the sense of statistical significance), which we have not seen with any of the other outcome measures. Strangely, this seems to only apply to the estimates using 90-day intervals. With 60 days and 30 days (Figs. A.3c and A.4c

measure with both 60-day and 30-day intervals. These results are shown in Figs. A.3 and A.4 in the Appendix. Similar to the earlier findings, the effect sizes become larger in the narrower intervals. With 60-day intervals the relative reduction in the likelihood to be satisfied amounts to 2.2% and with 30 days to 2.6%. Expressed once again relative to the largest predictor of being satisfied, unemployment, the reduction during the respective final interval before election day is roughly equal to between 6% (90 days) and 11% (30 days) of the decrease one would expect for individuals who lose their job.

[50] For the shorter intervals, their results shown in Figs. A.3b and A.4b in the Appendix, I find that within the last 60 and 30 days of election campaigns, individuals are respectively 18.7% and 20% more likely to feel not at all satisfied with their lives.

[51] Due to the potential issues associated with linear probability models for estimations when the average likelihood of an outcome lies close to 0 or 1, which is the case here, I also ran the otherwise same regression with a logit model. The resulting odds ratios do reveal somewhat smaller effects than the OLS model. Nonetheless, the implications for well-being before elections remain fundamentally the same.

in the Appendix), these deviations appear to manifest themselves to a much lesser extent.[52]

4.3 Discussion of net changes

My analysis of 148 different elections over three decades in 24 countries provides robust evidence that aggregate well-being is significantly lower during a relatively short period before election day.[53] Neither does this appear to be only a recent phenomenon. Interacting a linear time trend with the intervals, as shown in Fig. A.5 in the Appendix, reveals that elections today do not lead to statistically significantly greater reductions in well-being than in the past. The reasons for these negative consequences thus seem more fundamentally tied to the electoral process and not the product of recent changes in media and informational environment or increased political polarization. Nor do individuals seem to be able to adapt to whichever forces causing the decrease in aggregate life satisfaction. As the results of interaction of the intervals with the age of the respondents indicate (Fig. A.6 in the Appendix), the well-being of citizens who have experienced a larger number of elections over their lives is not significantly higher prior to election date than for individuals who only more recently reached voting age.

Once voting has taken place, aggregate life-satisfaction appears to revert to almost exactly its regular average. While earlier studies suggest that well-being may diverge between winners and losers, based on my findings from a substantially larger set of elections and countries, we can be quite certain that on aggregate these two effects cancel each other out. Alternatively, it may also be the case that once campaigns have ceased and votes have been cast, elections (and politics in general) simply do not continue to play an important role in the lives of most citizens. The immediate rebound to baseline satisfaction levels after election day also speaks against potential utility gains from participation in elections. Even if some citizens may derive some enjoyment of having performed a civic duty, the effects thereof appear to be either very small relative to all other determinants of aggregate well-being, or so short-lived that they are not even noticeable when looking at the 30-day window immediately following an election. Democratic systems may be associated with higher levels of well-being in general, but elections themselves do not appear to be a direct contributing factor.

Returning to the period prior to election day, the negative mechanisms of the electoral process, potentially some of those outlined in Section 2.2, seem to dominate any positive mechanisms. Of course, the results do not rule out that some citizens indeed derive some utility from democratic elections due to, for example, the process itself, performing a civic duty, collective action, and expression of identities. For the majority of the electorate though, the possible downsides to these positive mechanisms and other negative channels play a greater role for well-being during election campaigns. Albeit speculative, this would imply that rather than receiving a warm-glow from being good democratic citizens by voting and staying informed, more individuals feel forced to do so because of social pressure. Out-group discrimination based

[52] Using a logit instead of a linear probability model for the estimation with the widest intervals also produces estimates that are closer to zero for all intervals, except for the last one before the election.

[53] This finding may also provide important insights for future research attempting to measure the effects of election outcomes. Employing the days just before voting takes place as the control period to compare post-election outcomes against, as for example, Kinari et al. (2019) and Pinto et al. (forthcoming), do, may lead to inaccurate conclusions.

on partisanship likely dominates any in-group benefits from engaging with like-minded citizens. Instead of enjoying political deliberation prior to elections as a vital aspect of democracy, a greater share of individuals worries about or even experiences disagreement and conflict over politics harming their social relationships. So far, this represents merely speculation about the actual origins of the well-being decrease during election campaigns. Thus, in Section 5, I examine some of the heterogeneity in this reduction that may provide suggestive evidence regarding the underlying reasons.

5 Heterogeneity and potential mechanisms

To investigate potential heterogeneity in the well-being changes around elections, I run the same regressions as before separately for each sub-sample of interest. Compared to interacting the relevant characteristic with each of the intervals, this represents a quite restrictive approach. Since all control variables and sets of fixed effects are estimated disjointedly within every sub-sample, this procedure may substantially reduce the amount of variation left to be exploited. On the other hand, any potentially arising heterogeneity should ideally be relatively robust.[54] Before commencing, I emphasize that the subsequent analyses are explorative. Even if we are to observe any heterogeneity, any conclusions regarding possible mechanisms remain highly speculative. Nonetheless, given the sizable negative well-being consequences during election campaigns and potential implications for the continued support of democracy among the public, which appears to be waning across many countries, I would argue that understanding the underlying mechanisms of my findings is of sufficient importance to justify pursuing even early, imperfect attempts to do so.

5.0.1 Education

I start by separately analyzing changes in well-being around elections across educational attainment. Education has been shown to be positively correlated with civic and political engagement in general (see, e.g., Dee, 2004; Huddy et al., 2015).[55] However, education itself does not seem to increase turnout in actual elections (see, e.g., Chevalier and Doyle, 2012). Differences in life-satisfaction changes around elections between education levels are more likely to arise from additional behavior than if highly educated individuals simply had higher turnout rates.

The results that are shown in Fig. 8 indicate that during the 90-day stretch before election day, individuals in the highest education category (defined as finished formal education at age 22 or older) experience a relative reduction in life satisfaction, 1.54%, that is almost twice as large as the relative decrease among those with lower educational attainment (finished formal education

[54]Nonetheless, due to the issue of low remaining variation for groups with low numbers of observations, I perform an alternative approach by demeaning the entire sample with the sets of fixed effects (thus again assuming they are homogeneous across all groups). Afterward, I run the regression separately for each sub-sample as usual but leaving out the fixed effects. The results of these robustness checks mostly align closely with those obtained when applying the fixed effects to every sub-sample individually. Fig. A.13 in the Appendix serves as an example that corresponds with the analysis across electoral systems presented in Fig. 10.

[55]The finding highlights an advantage of the choice to perform the regressions completely separately for each group. If highly educated individuals are more likely to engage in politics outside of the election period, then it seems sensible to compare the respective groups only with themselves across all points in time.

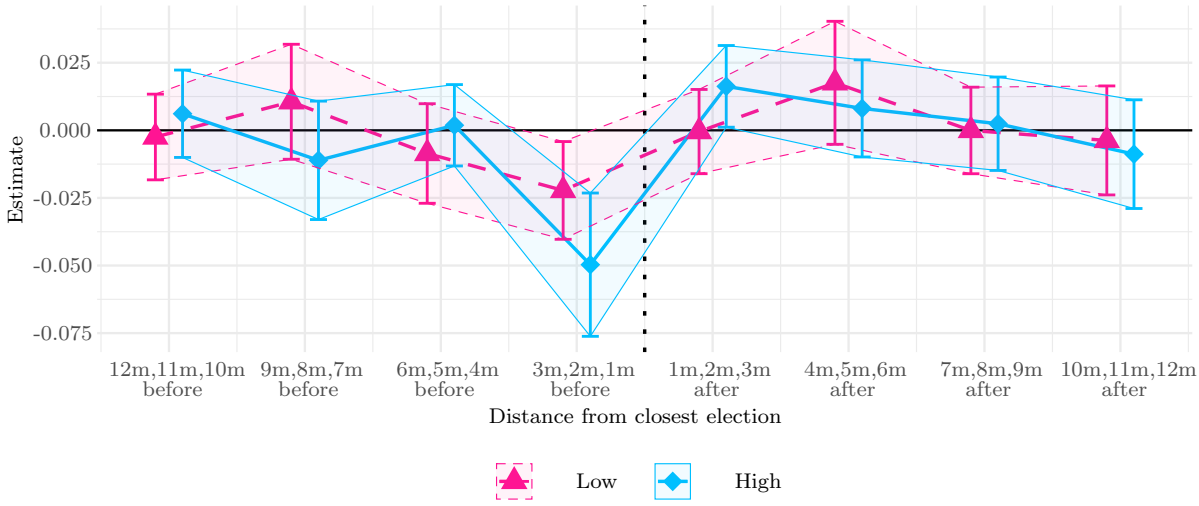


Figure 8: Net changes in life satisfaction around elections over educational attainment

Notes: Low consists of individuals who finished their formal education by age 18, high of individuals who finished their formal education after age 22. Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 2.91 for low and 3.22 for high. Controls include age, age^2, gender, occupation, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 0.70M and 0.26M observations with adjusted R^2s of 0.20 and 0.21. 95% confidence intervals (standard errors clustered on country-wave level) shown.

by age 18 or young) of 0.76%.[56] The coefficients of both groups are statistically different from zero at the 95% significance level, though. Given that the high education group consists of only about one third as many observations as the low education group, the much higher level of statistical significance attained by the coefficient of the former group and the size of the effect may be nonetheless taken as somewhat indicative that highly educated individuals "suffer" particularly during election campaigns. However, after election day, respondents with extensive formal education also report significantly higher satisfaction levels than in the reference period. I will not further speculate if this may be indicative of voting producing warm-glow utility only among the highly educated, or just a sense of relief that the election as a source of lower well-being may finally be in the past. For almost all other intervals, the changes in life-satisfaction relative to their respective reference period levels are very similar between the two groups. This once again bolsters the argument for the link between elections and well-being. We would only expect to observe some differences between groups in intervals where some regularly occurring event affects groups differently, which I find suggestive evidence for directly around election day.

What could the observed differences between education levels before election day potentially be indicative of (even if the coefficients are not statistically different from each other)? If voters perceived the choices they face in elections to be overwhelming complex, it seems reasonable to assume that this would be particularly noticeable among less-educated individuals. Given that the decrease in life-satisfaction is comparatively smaller for the group with lower educational attainment, this narrative seems not a very convincing one.

[56] However, the changes in life satisfaction before election day are not statistically different between low and high levels of education. Because the groups represent two independent samples, a simple Z-test, in this case $\frac{-0.022-(-0.050)}{\sqrt{(0.009)^2+(0.014)^2}}$, can be performed to obtain the level of statistical significance.

Increased coverage of politics on all types of media during election campaigns would also seem more likely to represent a nuisance to individuals with lower levels of political interest. Potentially high initial costs for increasing political knowledge only during the electoral process would also seem more likely to affect voters with less educational attainment. Thus, both these two latter explanations do not really conform with the observed pattern. However, the level of political engagement (which I proxy here, admittedly quite crudely, with education) within the social network seems important for political information acquisition dynamics. As Marshall (2018) shows, in politically engaged social groups, regardless of the personal level of political sophistication, all members invest in acquiring political knowledge during election campaigns for social image reasons. In contrast, these dynamics do not seem to take hold in social networks with lower average political knowledge. Thus, overinvestment in political information gathering due to social image concerns before elections that appear likely to apply to citizens in highly educated social networks may be partially responsible for the particularly sharp decrease in life satisfaction among individuals with the highest level of educational attainment.

Finally, the existing evidence regarding higher political engagement increasing with educational attainment, combined with the findings of greater ideological consistency and more unidimensional views among highly educated individuals (see, e.g., Bishop, 1976; Lupton et al., 2015), also highlight affective polarization and greater political disagreement with peers as a further possible reason for the discrepancy in the decreases of life satisfaction between levels of education.[57] Ulbig and Funk (1999) also find that individuals with lower educational attainment tend to be more likely to avoid conflict in discussion within their social network, especially when it comes to politics. Thus, highly educated individuals may face a higher likelihood of (temporarily) damaged relationships with family and friends and discrimination on a partisan basis.

5.0.2 Community size

Next, I run separate regressions for the size of the place of residence that respondents live in. Small refers to inhabitants of rural and small to mid-sized towns, whereas large represents residents of large cities. The results, presented in Fig. 9, show that the decrease in life satisfaction is only statistically different from zero at the 95% significance level among smaller sized communities. I do not find a significant reduction in well-being for individuals living in large cities before election day. While the lack of statistical significance of the latter group's coefficient may also be partly due to the lower number of observations, the relative decrease in large cities is also only about three-fifths of the relative reduction I find for rural areas and towns. Once again, based on a Z-test or the observation that the two coefficients' confidence intervals overlap, the difference between the groups is not statistically significant.

The suggestive evidence for the electoral process having stronger negative effects in tight-knit communities, in which highly local social ties may also more likely form the basis for social relationships, could point towards political conflict being more harmful in smaller towns where

[57] Political polarization, in general, seems to be strongly increasing in the degree of education (Pew Research Center, 2019).

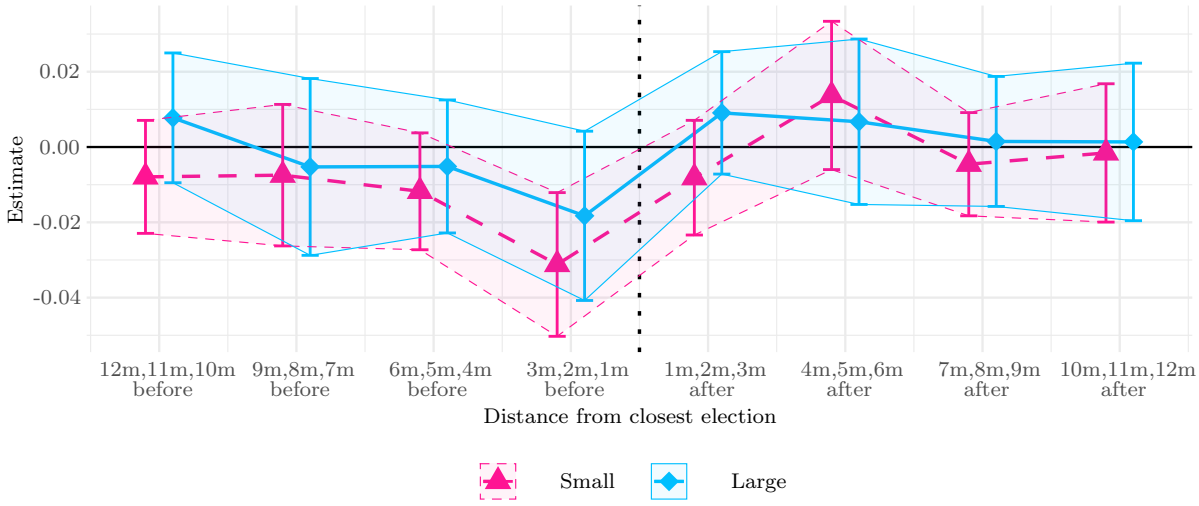


Figure 9: Net changes in life satisfaction around elections over community size

Notes: Small consists of residents of rural, small and mid-sized towns; large consists of residents of large cities. Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.03 for small and 3.01 for large. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 0.89M and 0.32M observations with adjusted R^2s of 0.23 and 0.23. 95% confidence intervals (standard errors clustered on country-wave level) shown.

resident interact more often and likely rely more on each other. The differences also bring to mind the findings by Funk (2010) and Gerber et al. (2008) that show social image concerns and pressure to participate in elections to particularly high when it concerns the immediate local community.

5.0.3 Electoral system

Studying the net effect of elections separately for different electoral systems, I find that the period just before an election occurs is associated with lower levels of life satisfaction in both proportional and majoritarian systems. However, individuals in majoritarian systems appear to suffer much more. With the single-member districts in these countries acting as a barrier to the number of viable parties, political cleavages likely become more pronounced than if the higher number of parties arising in proportional systems with more seats per district occupy more overlapping positions on the ideological spectrum. Fewer parties also facilitate stronger partisan attachments (Huddy et al., 2018) and, in turn, feelings of negative partisanship towards supporters of the few (or even single) opposing parties.[58]

As Huber et al. (2005) shows, citizens with lower levels of education are less likely to form party attachments in more complex political systems (greater number of relevant parties). In contrast, complexity matters less for individuals with high levels of educational attainment. I find a stronger reduction in life satisfaction before elections among more educated citizens (and

[58]Based on Fig. A.10 in the Appendix, I find that the decrease in life satisfaction is actually lower than average in France and the U.K., the only two countries using exclusively single-member districts. The large decrease in life satisfaction among majoritarian countries is thus driven mostly by countries like Germany and Italy, for example, that use mixed systems. The implications thereof may provide an interesting opening for future research.

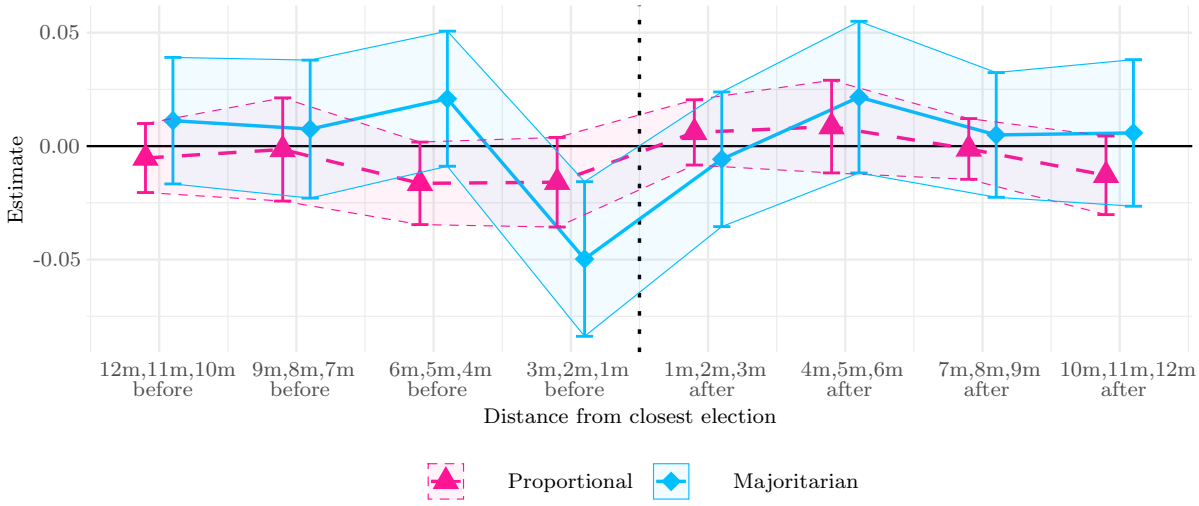


Figure 10: Net changes in life satisfaction around elections over electoral systems

Notes: Proportional consists countries in which all citizens vote in single-member districts, majoritarian consists of countries in which not all citizens vote in single-member districts. Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.08 in proportional and 2.92 in majoritarian. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 0.83M and 0.45M observations with adjusted R^2s of 0.24 and 0.17. 95% confidence intervals (standard errors clustered on country-wave level) shown.

thus more likely to be part of more politically engaged social networks) than among those with fewer schooling years in general. However, when analyzing the effect of education separately across proportional and majoritarian systems, low education citizens do not report statistically significant lower levels of satisfaction in the latter systems, only the most educated individuals. The decision environment is likely less complex in majoritarian systems since the number of realistically contesting parties is lower, and attributing responsibility for the current personal and country situation is easier if fewer parties are in government. Thus, given that individuals with lower educational attainment are significantly less satisfied in majoritarian, but not in proportional systems, speaks against the possibility that a large segment of politically less engaged voters becomes overwhelmed by the choices they face in elections.

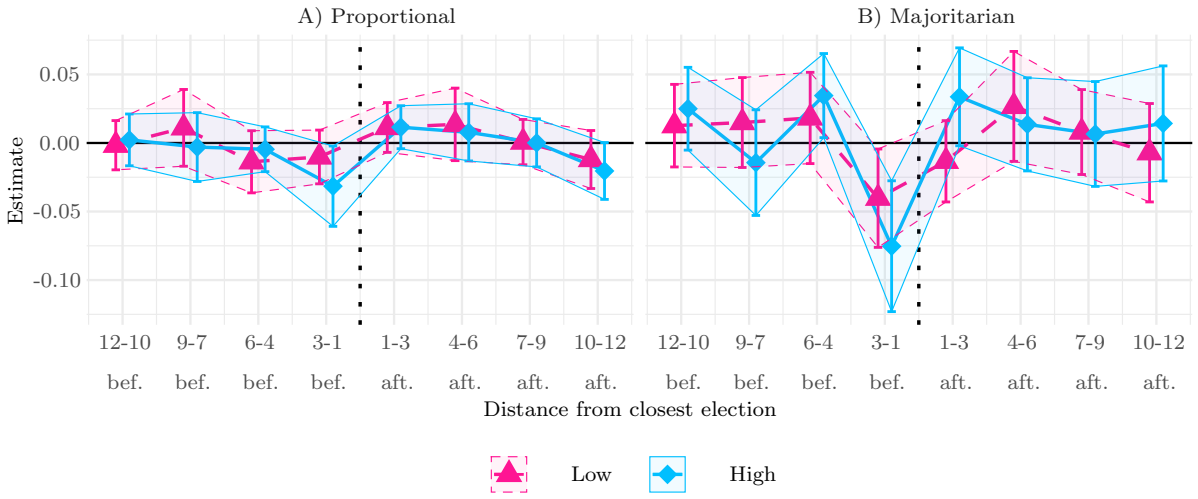


Figure 11: Net changes in life satisfaction around elections over electoral systems and educational attainment

Notes: Averages of life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) are 2.95, 3.28, 2.86 and 3.07. Controls include age, age^2, gender, occupation, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 0.42M, 0.19M, 0.28M and 0.07M observations with adjusted R^2s of 0.22, 0.23, 0.18 and 0.14. 95% confidence intervals (standard errors clustered on country-wave level) shown.

6 Conclusion

This chapter contributes to the study of well-being and elections by providing the first systematic cross-country and long-term evidence for a link between the two. The absence of research on satisfaction in the public during election campaigns so far seems to be a particularly striking void in the study of democracy. As public involvement and interest in politics arguably peak during this period, it would appear vital to know how the populace is affected thereby. My empirical analysis shows that the apex of the campaign season is, in fact, the single span of time around elections during which aggregate well-being systematically and significantly changes. During the final month(s) before election day, average life satisfaction is substantially lower than it would otherwise be. These negative effects do not persist after voting takes place, however. Thus, elections appear to reduce aggregate well-being before the day of the vote without producing similarly systematic short-term gains afterward. These findings should also be taken into consideration in future research regarding the consequences of elections, as the time span before election day likely represents an unsuitable control period to compare outcomes after the election against.

Global dissatisfaction with democracy is at or near historic highs (Foa et al., 2020). Hence, now more than ever, it would be vital to understand the mechanisms for why the electoral process seems to regularly herald a season of public unhappiness instead of being a time of civic celebration. Based on some very preliminary exploratory analyses, social conflict and polarization could prove to be promising starting points in this regard. Designing electoral institutions that curtail the formation of stark political cleavages and affective polarization, such as greater proportionality in legislative allocation, could thus potentially play a role in

halting or even reversing the aforementioned trends. Furthermore, institutions that influence the costs of voting and political information (including surrounding social norms), may offer fruitful opportunities for research into the causes of lower well-being during election campaigns. However, given the suggestive evidence so far, it does not appear that voters become unhappy because they are overwhelmed by the complexity of the choices they face in elections. To this point, based on the existing research on the effects of direct democracy on civic engagement (see, e.g., Benz and Stutzer, 2004), it would also be interesting to study whether my findings for representative elections translate to ballot measures.

Yet, even if it turns out that the negative consequences of election campaigns are unavoidable, this would not speak against elections as a mechanism for societal decision-making. Rather, the short-term reduction in life satisfaction during the months immediately preceding an election may be understood as the investment costs required for reaping the long-term profits of democracy in terms of well-being.

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A Appendix

A.1 Empirical strategy

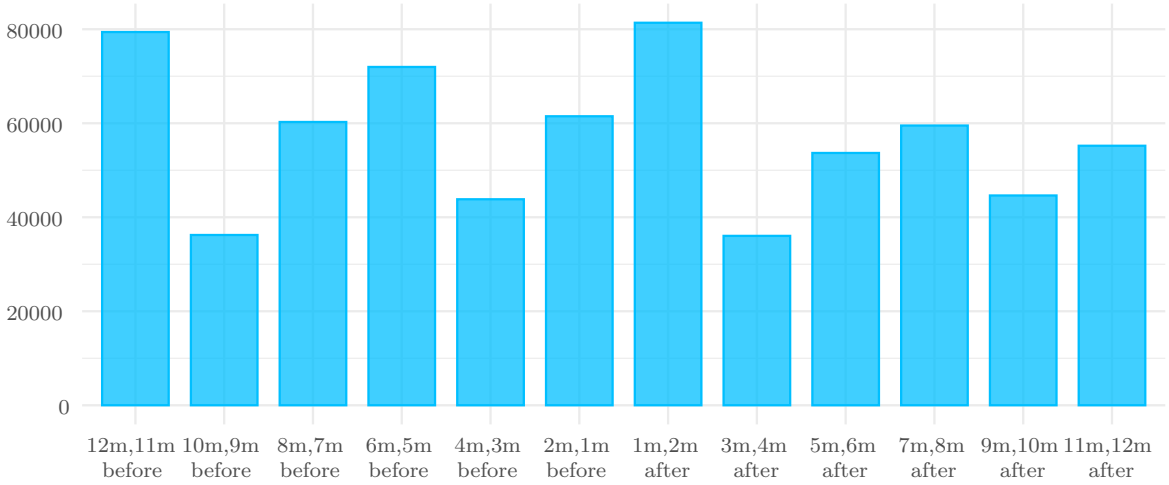


Figure A.1: Number of observations across 60-day intervals around closest election

Notes: The number of observations in the reference period (0.60M) is not shown. Across all intervals, 0.68M respondents are observed in the treatment period of the electoral process.

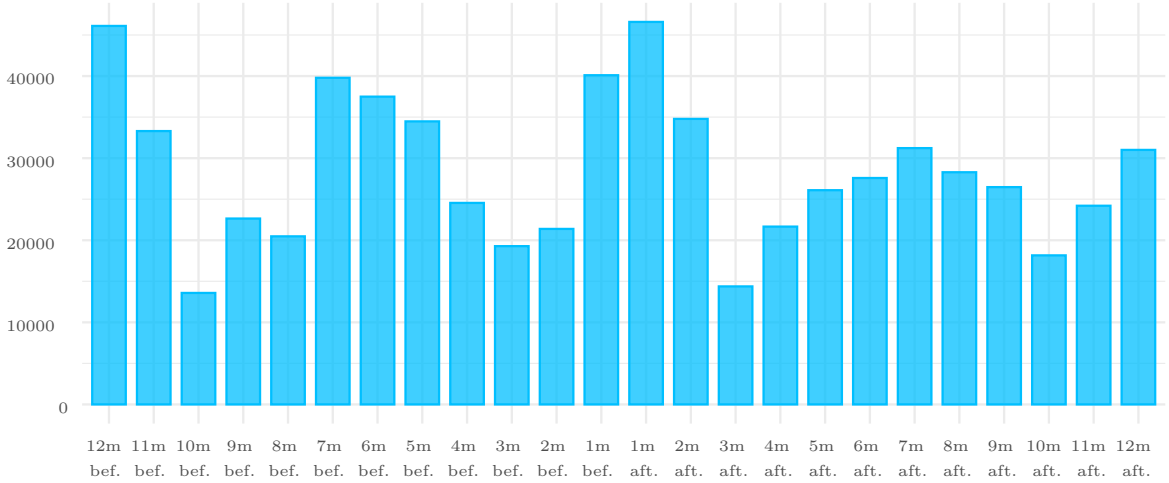


Figure A.2: Number of observations across 30-day intervals around closest election

Notes: The number of observations in the reference period (0.60M) is not shown. Across all intervals, 0.68M respondents are observed in the treatment period of the electoral process.

Table A.1: Example of empirical strategy for Danish elections

Calendar week	730–361 days before & 361–730 days after (reference)	360–271 days before	270–181 days before	180–91 days before	90 days before	90 days after	91–180 days after	181–270 days after	271–360 days after
03	2009				2005-02-08				
04	2009				2005-02-08				
05	1996, 2009				2005-02-08				
06	1996, 2009				2005-02-08				
09	1996								1988-05-10
10	1996					1990-12-12			1988-05-10
11	1996, 2014, 2018				2019-06-05	1990-12-12	1990-12-12		1988-05-10
12	1992, 1993, 2014, 2017, 2018		1990-12-12		2019-06-05		1990-12-12		1988-05-10
13	1992, 1993, 2006, 2017, 2018		1990-12-12		2019-06-05		1990-12-12, 2001-11-20		1988-05-10
14	1992, 1993, 2006	1998-03-11	1990-12-12				2001-11-20, 2007-11-13		1988-05-10
15	1993, 2000, 2006, 2018	1998-03-11	1990-12-12, 2007-11-13	1994-09-21	2019-06-05		2001-11-20, 2007-11-13		1988-05-10, 2015-06-18
16	2000, 2006, 2017, 2018	1998-03-11	1990-12-12, 2001-11-20, 2007-11-13	1994-09-21	2019-06-05	1998-03-11	2001-11-20, 2007-11-13		2015-06-18
17	1992, 2000, 2006, 2017, 2018	1998-03-11	2001-11-20, 2007-11-13	1994-09-21	2019-06-05	1998-03-11	2001-11-20, 2007-11-13		
18	1992, 2000, 2006	1998-03-11	2001-11-20, 2007-11-13	1994-09-21		1998-03-11	2001-11-20, 2007-11-13		
19	1992, 2000, 2013		2001-11-20, 2007-11-13	2011-09-15	2019-06-05	1998-03-11	2001-11-20		
20	1992, 2000, 2013		2001-11-20, 2007-11-13	2011-09-15	2019-06-05	1998-03-11	2001-11-20		
21	2009, 2013, 2017			2011-09-15	2019-06-05	1998-03-11	2005-02-08	2001-11-20	
22	2009, 2013, 2017				2015-06-18		2005-02-08	2001-11-20	
23	2009, 2013			2011-09-15	2015-06-18	2019-06-05	2005-02-08	2001-11-20, 2011-09-15	2015-06-18
24	2009, 2010, 2014, 2016, 2017			2011-09-15		2019-06-05	2005-02-08	2001-11-20, 2011-09-15	2011-09-15, 2015-06-18
25	2009, 2010, 2014, 2017	2015-06-18, 2019-06-05			2011-09-15	2019-06-05			2011-09-15
26	2009, 2010, 2017	2015-06-18, 2019-06-05							
27	2009	2019-06-05							
36	2009		2019-06-05						
37	2006, 2009		2019-06-05				2019-06-05		
38	2006		2019-06-05				2019-06-05		
39	1992, 2006, 2016, 2017				2001-11-20		2019-06-05		
40	1992, 2006, 2016, 2017				2001-11-20, 2007-11-13				2001-11-20
41	1992, 2003, 2006		2015-06-18	2005-02-08	2001-11-20, 2007-11-13				2001-11-20, 2007-11-13
42	1989, 1999, 2003		2015-06-18	2005-02-08	1990-12-12, 2001-11-20, 2007-11-13		2015-06-18	2005-02-08	1990-12-12, 2001-11-20, 2007-11-13
43	1989, 1999, 2003, 2017		2019-06-05	2005-02-08	1990-12-12, 2001-11-20, 2007-11-13		2015-06-18	2005-02-08	1990-12-12, 2001-11-20, 2007-11-13
44	1989, 1999, 2003, 2013, 2017		2019-06-05	2005-02-08	1990-12-12, 2001-11-20, 2007-11-13			2005-02-08	1990-12-12, 2001-11-20, 2007-11-13
45	1989, 1992, 1999, 2003, 2012, 2013, 2016, 2017		2015-06-18, 2019-06-05		1990-12-12, 2001-11-20	2011-09-15	2015-06-18	2005-02-08	1990-12-12, 2001-11-20, 2005-02-08
46	1989, 1992, 1999, 2000, 2012, 2013, 2016, 2017		2015-06-18, 2019-06-05		1990-12-12, 2001-11-20	2011-09-15	2015-06-18		2005-02-08
47	1989, 1992, 2000, 2012, 2013		2019-06-05		1990-12-12	2011-09-15			
48	1992, 2000, 2013, 2016	2001-11-20	2015-06-18		2005-02-08		2015-06-18		
49	2013, 2016	2001-11-20	2015-06-18, 2019-06-05	2019-06-05	1990-12-12, 2005-02-08		2015-06-18		

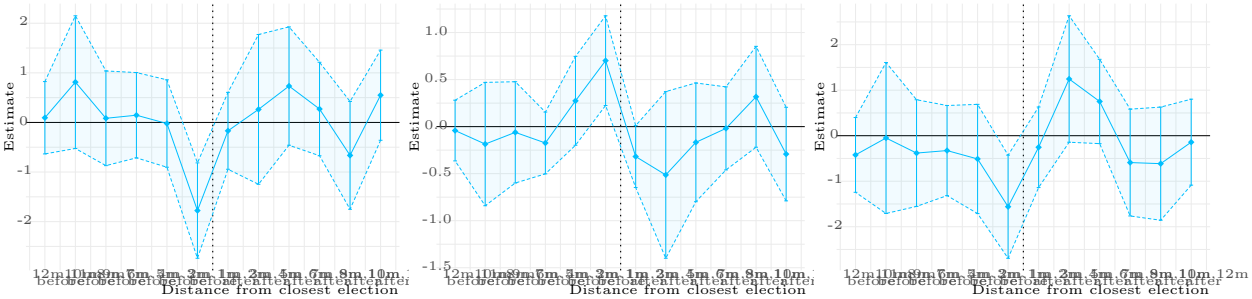
Notes: Dates within intervals refer to the date of the election for which observations exist in the corresponding interval.

Table A.2: Number of observations and distinct elections per country and 90 day interval

Country	Reference	360–271 days before	270–181 days before	180–91 days before	90 days before	90 days after	91–180 days after	181–270 days after	271–360 days after
Austria	12983 (5)	2538 (3)	1545 (1)	3173 (3)	1102 (2)	3691 (3)	904 (1)	4316 (4)	1106 (3)
Belgium	35394 (9)	2229 (5)	5492 (5)	1411 (2)	5579 (5)	2638 (3)	6437 (6)	2784 (2)	5746 (5)
Bulgaria	11283 (4)	1899 (1)	1434 (3)	2076 (1)	2976 (3)	2841 (2)	930 (2)	2846 (1)	2325 (3)
Cyprus	9568 (4)	1105 (3)	1368 (2)	457 (1)	1292 (3)	915 (2)	1773 (3)	523 (2)	1242 (2)
Czech Republic	23494 (4)	4745 (4)	2974 (3)	3759 (2)	1914 (2)	4543 (3)	3463 (3)	1369 (2)	3737 (4)
Denmark	35236 (9)	2880 (4)	8725 (5)	4121 (4)	9974 (7)	2869 (4)	7904 (6)	1830 (3)	6249 (7)
Estonia	25394 (5)	3256 (3)	2899 (3)	5130 (3)	1683 (2)	4325 (4)	2721 (3)	4126 (4)	976 (1)
Finland	33541 (6)	7012 (6)	2255 (3)	6438 (4)	1356 (1)	6207 (4)	979 (1)	6667 (5)	1992 (2)
France	28301 (6)	1405 (2)	4183 (4)	1137 (2)	8057 (6)	1201 (2)	4211 (4)	175 (1)	6535 (5)
Germany	64239 (8)	12924 (5)	5698 (5)	17438 (7)	4702 (3)	11529 (5)	4579 (3)	5197 (4)	5781 (5)
Hungary	22856 (4)	7330 (4)	1601 (3)	4547 (3)	2310 (3)	3020 (4)	1946 (2)	6133 (2)	983 (2)
Ireland	29853 (7)	2197 (2)	7189 (6)	2651 (2)	6280 (6)	1900 (4)	6147 (6)	2667 (2)	3501 (4)
Italy	30682 (7)	4954 (4)	2842 (3)	6741 (6)	5226 (5)	5832 (6)	5288 (6)	6386 (5)	4901 (6)
Latvia	17447 (3)	4317 (3)	1894 (3)	2816 (3)	672 (2)	5622 (3)	737 (1)	4565 (3)	575 (2)
Lithuania	25340 (5)	2865 (2)	1849 (2)	1884 (2)	2471 (3)	4094 (4)	1598 (1)	6761 (4)	1737 (3)
Luxembourg	10104 (7)	865 (2)	1263 (3)	1235 (4)	1350 (4)	1946 (3)	2343 (6)	1261 (3)	1354 (5)
Malta	11043 (4)	1099 (3)	1563 (2)	962 (3)	1443 (1)	1905 (3)	1407 (2)	1428 (2)	1387 (2)
Netherlands	30076 (8)	4397 (3)	1079 (2)	4675 (3)	3057 (4)	7152 (5)	1787 (2)	5766 (3)	1916 (2)
Poland	21692 (5)	5218 (3)	2612 (3)	6132 (5)	2724 (3)	3595 (3)	1563 (2)	2451 (4)	1428 (3)
Portugal	27634 (7)	8703 (4)	1782 (2)	4798 (4)	1692 (2)	6076 (5)	1549 (3)	4853 (5)	977 (2)
Slovakia	21016 (3)	3828 (2)	2043 (2)	3788 (2)	1235 (2)	1164 (2)	1664 (2)	2128 (1)	1812 (2)
Slovenia	18497 (5)	1940 (2)	5611 (3)	1874 (3)	3471 (3)	3032 (4)	5718 (2)	3415 (4)	5746 (4)
Spain	20230 (7)	847 (1)	3781 (3)	3670 (4)	1927 (3)	3557 (3)	862 (1)	3837 (4)	2969 (5)
United Kingdom	33519 (9)	4430 (4)	11229 (6)	5605 (5)	8271 (6)	6100 (5)	8828 (5)	4495 (3)	8391 (6)

Notes: The first number refers to the number of respondents observed in the country and interval, the number in brackets represents the number of distinct elections these observations come from.

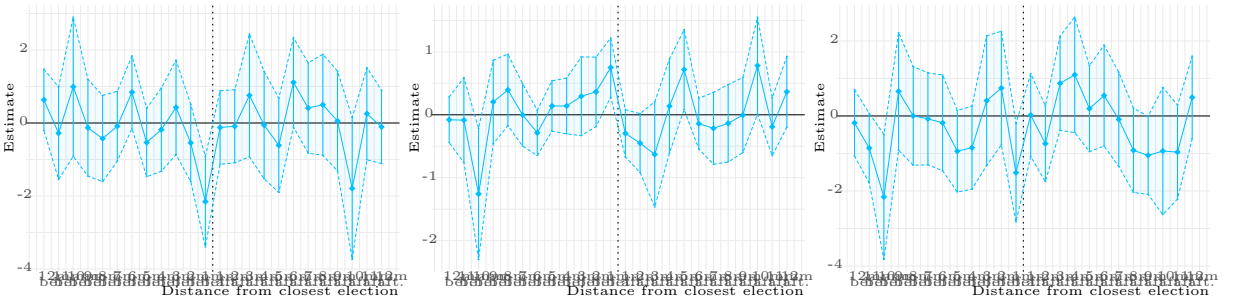
A.2 Additional results



(a) Fairly or very satisfied (b) Not at all satisfied (c) Very satisfied

Figure A.3: Net changes around elections with binary life satisfaction measures and 60-day intervals

Notes: The full regression results are shown in columns 2–4 of Table A.3 in the Appendix. Average likelihoods in the reference period (720–361 days before and 361–720 days after elections) are 0.82, 0.04, and 0.25. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 1.28M observations with adjusted R^2s of 0.15, 0.06, and 0.17. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.



(a) Fairly or very satisfied (b) Not at all satisfied (c) Very satisfied

Figure A.4: Net changes around elections with binary life satisfaction measures and 30-day intervals

Notes: The full regression results are shown in columns 2–4 of Table A.3 in the Appendix. Average likelihoods in the reference period (720–361 days before and 361–720 days after elections) are 0.82, 0.04, and 0.25. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions with interval-specific country weights based on 1.28M observations with adjusted R^2s of 0.15, 0.06, and 0.17. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

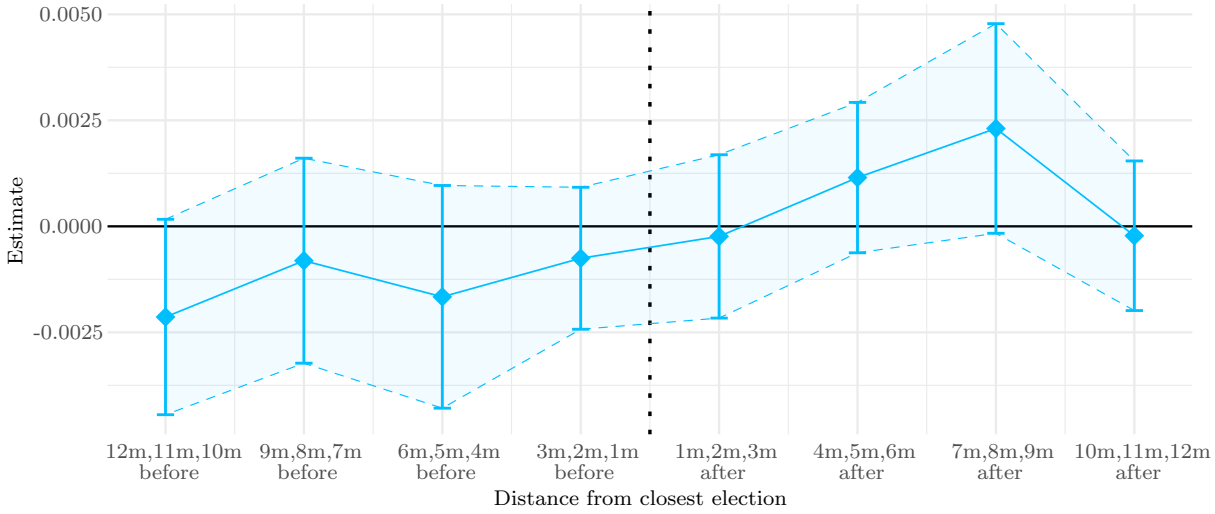


Figure A.5: Net changes in satisfaction around elections, interactions with time)

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Coefficients from interaction of intervals with year of interview (centered around zero). Controls include year of interview (centered around zero), age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week and day of the week fixed effects included. OLS regression with interval-specific country weights based on 1.28M observations with an adjusted R^2 of 0.22. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

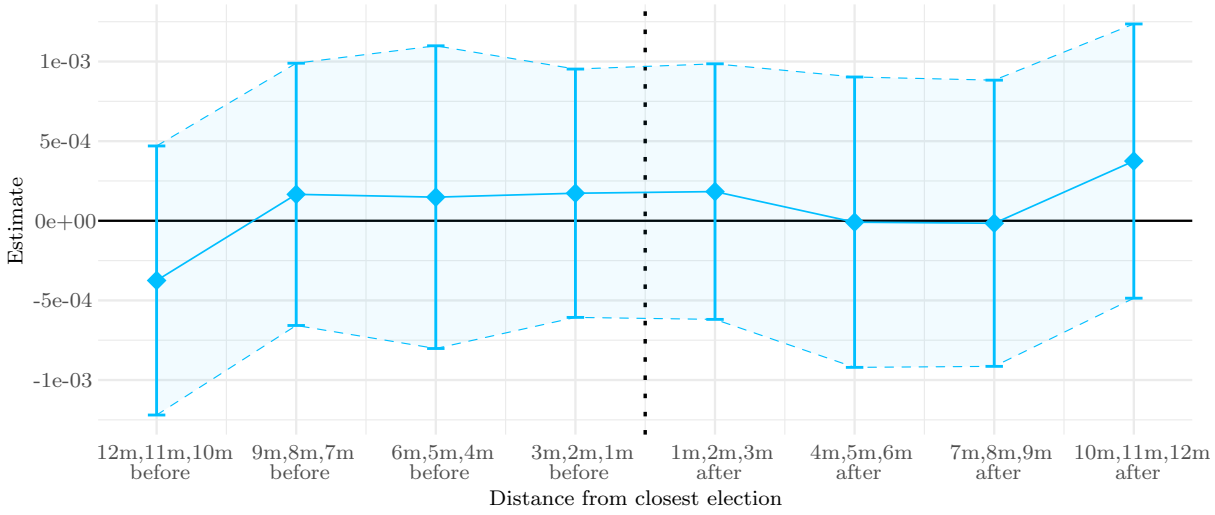


Figure A.6: Net changes in satisfaction around elections, interactions with age of respondent

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Coefficients from interaction of intervals with age of respondent (centered around zero). Controls include age (centered around zero), gender,occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regression with interval-specific country weights based on 1.28M observations with an adjusted R^2 of 0.22. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

Table A.3: Full regression results for Figs. 5 and 7

	LS (1–4)	Satisfied	Lowest	Highest
Interval around election				
12m, 11m, 10m before	-0.0044	0.2629	-0.1172	-0.8233**
	(0.0070)	(0.3708)	(0.1656)	(0.4037)
9m, 8m, 7m before	-0.0019	0.1677	-0.0119	-0.3745
	(0.0095)	(0.4342)	(0.2446)	(0.5678)
6m, 5m, 4m before	-0.0087	-0.1441	-0.0031	-0.7316*
	(0.0080)	(0.3888)	(0.1587)	(0.4184)
3m, 2m, 1m before	-0.0297***	-1.267***	0.5937***	-1.108**
	(0.0092)	(0.4062)	(0.2083)	(0.5188)
1m, 2m, 3m after	0.0003	-0.1281	-0.2796*	-0.1229
	(0.0068)	(0.3699)	(0.1545)	(0.4280)
4m, 5m, 6m after	0.0116	0.2971	-0.1339	0.7272*
	(0.0091)	(0.4868)	(0.3448)	(0.3981)
7m, 8m, 9m after	-0.0015	0.2884	-0.1344	-0.5685
	(0.0068)	(0.3875)	(0.1861)	(0.4237)
10m, 11m, 12m after	-0.0021	0.0004	-0.0824	-0.2920
	(0.0085)	(0.4357)	(0.2029)	(0.4228)
Male	-0.0200***	-0.5951***	0.4113***	-0.9958***
	(0.0018)	(0.0919)	(0.0429)	(0.1078)
Age	-0.0203***	-0.9304***	0.2594***	-0.8440***
	(0.0005)	(0.0257)	(0.0105)	(0.0222)
Age^2	0.0002***	0.0092***	-0.0026***	0.0081***
	(0.0000)	(0.0002)	(0.0001)	(0.0002)
Occupation (ref. cat.: homemaker)				
Management	0.1300***	6.504***	-1.89***	4.607***
	(0.0046)	(0.2339)	(0.1093)	(0.3063)
Manual	-0.0345***	-1.058***	-0.7340***	-3.124***
	(0.0047)	(0.2502)	(0.1065)	(0.2625)
Business owner or self-employed	0.0956***	5.14***	-1.617***	2.803***
	(0.0049)	(0.2647)	(0.1238)	(0.2810)
Primary sector	0.0065	0.6163	-1.039***	-1.001*
	(0.0087)	(0.4740)	(0.1967)	(0.5278)
Professional	0.1498***	7.521***	-2.018***	5.443***
	(0.0050)	(0.2877)	(0.1265)	(0.3301)
Retired	-0.0323***	-2.024***	0.4951***	-0.7080***
	(0.0047)	(0.2470)	(0.1144)	(0.2578)
Service sector	0.0386***	2.858***	-1.557***	-0.5584**
	(0.0042)	(0.2187)	(0.1063)	(0.2613)
Student	0.1657***	7.692***	-2.691***	6.183***
	(0.0281)	(1.516)	(0.8095)	(1.107)
Unemployed	-0.3380***	-18.66***	6.597***	-8.549***
	(0.0074)	(0.4249)	(0.2196)	(0.2886)
White collar	0.0656***	5.251***	-2.068***	-0.7637***
	(0.0042)	(0.2202)	(0.1112)	(0.2693)
Age finished education (ref. cat.: 14 or younger)				
15-18	0.0643***	3.715***	-1.701***	1.013***
	(0.0043)	(0.2598)	(0.1170)	(0.1780)
19-21	0.1390***	7.39***	-2.744***	3.771***
	(0.0046)	(0.2828)	(0.1243)	(0.2120)
22 or older	0.1895***	9.238***	-3.013***	6.7***
	(0.0050)	(0.2994)	(0.1304)	(0.2356)
Still studying	0.0606**	3.705**	-1.03	1.326
	(0.0285)	(1.526)	(0.8152)	(1.117)
Marital status (ref. cat.: divorced)				
Married	0.2542***	12.28***	-3.736***	9.405***
	(0.0036)	(0.2112)	(0.1146)	(0.2463)
Unmarried	0.1244***	7.056***	-2.33***	3.05***
	(0.0037)	(0.2085)	(0.1134)	(0.1912)
Widowed	0.0573***	3.152***	-1.358***	1.224***
	(0.0039)	(0.2311)	(0.1204)	(0.1950)
Annual GDP p.c. growth	0.0015*	-0.0082	-0.0879***	0.0705
	(0.0009)	(0.0455)	(0.0250)	(0.0447)
Unemployment rate	-0.0162***	-0.9521***	0.3049***	-0.3627***
	(0.0010)	(0.0566)	(0.0316)	(0.0480)
Year FE	✔	✔	✔	✔
Country-specific calendar week FE	✔	✔	✔	✔
Day of the week FE	✔	✔	✔	✔
Observations	1,283,035	1,283,035	1,283,035	1,283,035
R^2	0.22373	0.14875	0.05683	0.17026

Notes: * p<0.1, ** p<0.05, *** p<0.01.

A.3 Results for sensitivity analyses and robustness checks

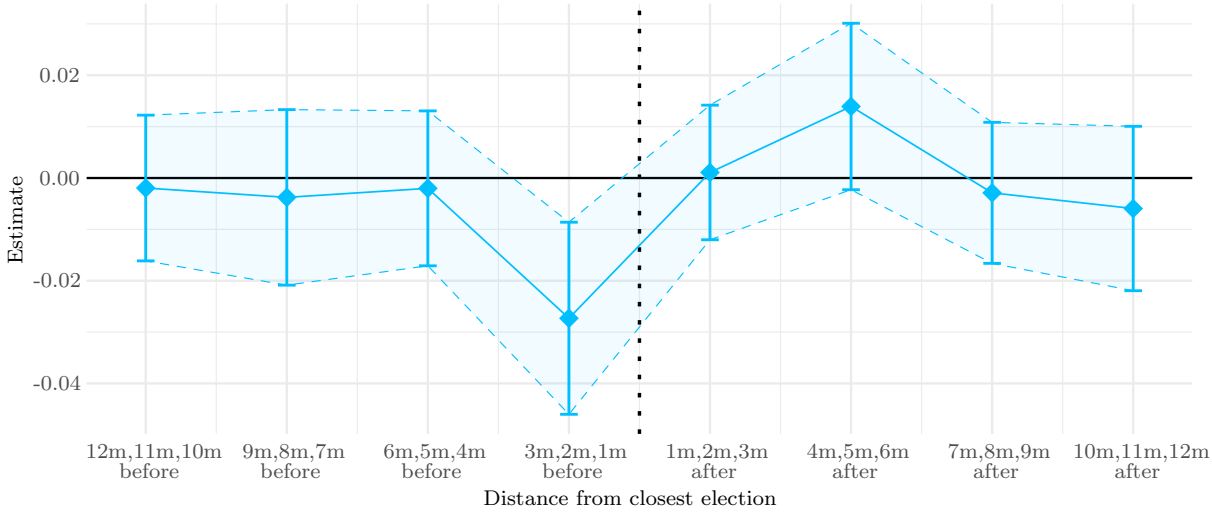
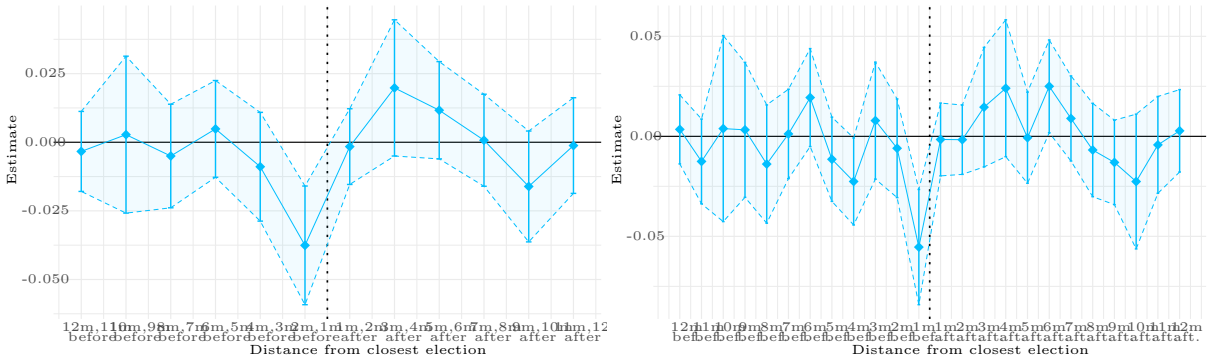


Figure A.7: Net changes in satisfaction around elections (without interval-specific country weights)

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regression based on 1.28M observations with an adjusted R^2 of 0.22. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.



(a) 60-day intervals

(b) 30-day intervals

Figure A.8: Net changes in life satisfaction around elections with 60-day and 30-day intervals (without interval-specific country weights)

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.02. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions based on 1.28M observations with adjusted R^2s of 0.22 and 0.22. 95% confidence intervals (standard errors clustered on 1,460 country-wave clusters) shown.

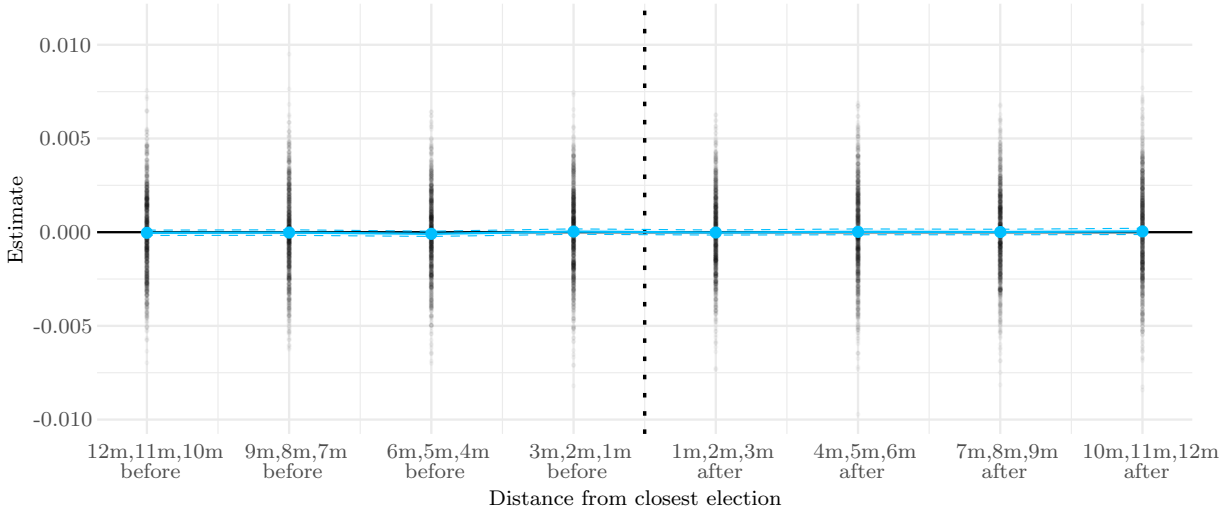


Figure A.9: Net changes in life satisfaction around elections, with distance to election randomly reshuffled and corresponding regressions run 1,000 times

Notes: Each black dot represents the coefficient estimate for the difference in life satisfaction in the corresponding interval to the reference period in one of the 1,000 iterations. The blue dots portray the average estimate in each interval. The blue dashed lines indicate the lower and upper bounds of the 95% confidence intervals of the 1,000 estimates within each interval. No country-specific interval weights have been applied (the results are therefore best compared with those of Fig. A.7.

Table A.4: Stepwide addition of fixed effects and control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	3.025***							
	(0.0123)							
Interval around election								
12m, 11m, 10m before	0.0122	0.0193	0.0109	-0.0016	-0.0015	-0.0043	-0.0044	-0.0040
	(0.0345)	(0.0343)	(0.0097)	(0.0085)	(0.0085)	(0.0078)	(0.0070)	(0.0062)
9m, 8m, 7m before	-0.0014	0.0055	-0.0031	0.0014	0.0017	-0.0025	-0.0019	-0.0000
	(0.0370)	(0.0375)	(0.0111)	(0.0107)	(0.0107)	(0.0099)	(0.0095)	(0.0081)
6m, 5m, 4m before	0.0048	0.0056	-0.0003	-0.0066	-0.0065	-0.0071	-0.0087	-0.0106
	(0.0345)	(0.0339)	(0.0114)	(0.0090)	(0.0090)	(0.0086)	(0.0080)	(0.0067)
3m, 2m, 1m before	-0.0134	-0.0186	-0.0149	-0.0319***	-0.0316***	-0.0304***	-0.0297***	-0.0224***
	(0.0351)	(0.0329)	(0.0130)	(0.0102)	(0.0101)	(0.0097)	(0.0092)	(0.0085)
1m, 2m, 3m after	0.0142	0.0096	0.0074	0.0030	0.0030	0.0007	0.0003	0.0025
	(0.0358)	(0.0345)	(0.0094)	(0.0085)	(0.0085)	(0.0079)	(0.0068)	(0.0067)
4m, 5m, 6m after	0.0156	0.0140	0.0052	0.0106	0.0108	0.0103	0.0116	-0.0008
	(0.0413)	(0.0396)	(0.0115)	(0.0101)	(0.0101)	(0.0097)	(0.0091)	(0.0078)
7m, 8m, 9m after	-0.0022	0.0052	-0.0034	-0.0102	-0.0103	-0.0046	-0.0015	-0.0009
	(0.0344)	(0.0345)	(0.0093)	(0.0088)	(0.0088)	(0.0079)	(0.0068)	(0.0068)
10m, 11m, 12m after	0.0003	-0.0016	0.0067	-0.0102	-0.0101	-0.0046	-0.0021	-0.0065
	(0.0350)	(0.0362)	(0.0114)	(0.0099)	(0.0099)	(0.0091)	(0.0085)	(0.0084)
Male						-0.0202***	-0.0200***	-0.0201***
						(0.0018)	(0.0018)	(0.0018)
Age						-0.0204***	-0.0203***	-0.0203***
						(0.0005)	(0.0005)	(0.0005)
Age^2						0.0002***	0.0002***	0.0002***
						(0.0000)	(0.0000)	(0.0000)
Occupation (ref. cat.: homemaker)								
Management						0.1298***	0.1300***	0.1298***
						(0.0045)	(0.0046)	(0.0046)
Manual						-0.0356***	-0.0345***	-0.0348***
						(0.0047)	(0.0047)	(0.0047)
Business owner or self-employed						0.0952***	0.0956***	0.0954***
						(0.0049)	(0.0049)	(0.0049)
Primary sector						0.0042	0.0065	0.0059
						(0.0087)	(0.0087)	(0.0087)
Professional						0.1484***	0.1498***	0.1494***
						(0.0050)	(0.0050)	(0.0051)
Retired						-0.0343***	-0.0323***	-0.0327***
						(0.0047)	(0.0047)	(0.0047)
Service sector						0.0377***	0.0386***	0.0380***
						(0.0042)	(0.0042)	(0.0042)
Student						0.1617***	0.1657***	0.1571***
						(0.0276)	(0.0281)	(0.0272)
Unemployed						-0.3446***	-0.3380***	-0.3384***
						(0.0074)	(0.0074)	(0.0074)
White collar						0.0647***	0.0656***	0.0653***
						(0.0042)	(0.0042)	(0.0042)
Age finished education (ref. cat.: 14 or younger)								
15-18						0.0638***	0.0643***	0.0657***
						(0.0042)	(0.0043)	(0.0041)
19-21						0.1389***	0.1390***	0.1405***
						(0.0047)	(0.0046)	(0.0045)
22 or older						0.1891***	0.1895***	0.1908***
						(0.0050)	(0.0050)	(0.0048)
Still studying						0.0626**	0.0606**	0.0698**
						(0.0280)	(0.0285)	(0.0275)
Marital status (ref. cat.: divorced)								
Married						0.2548***	0.2542***	0.2542***
						(0.0036)	(0.0036)	(0.0036)
Unmarried						0.1254***	0.1244***	0.1264***
						(0.0037)	(0.0037)	(0.0036)
Widowed						0.0572***	0.0573***	0.0571***
						(0.0039)	(0.0039)	(0.0039)
Annual GDP p.c. growth							0.0015*	0.0007
							(0.0009)	(0.0008)
Unemployment rate							-0.0162***	-0.0159***
							(0.0010)	(0.0010)
Year FE		✔	✔	✔	✔	✔	✔	
Country FE			✔					
Country-specific calendar week FE				✔	✔	✔	✔	✔
Day of the week FE					✔	✔	✔	✔
EB wave FE								✔
Observations	1,283,035	1,283,035	1,283,035	1,283,035	1,283,035	1,283,035	1,283,035	1,283,035
R^2	-0.00021	0.00486	0.16238	0.16858	0.16862	0.22213	0.22373	0.22545

Notes: * p<0.1, ** p<0.05, *** p<0.01.

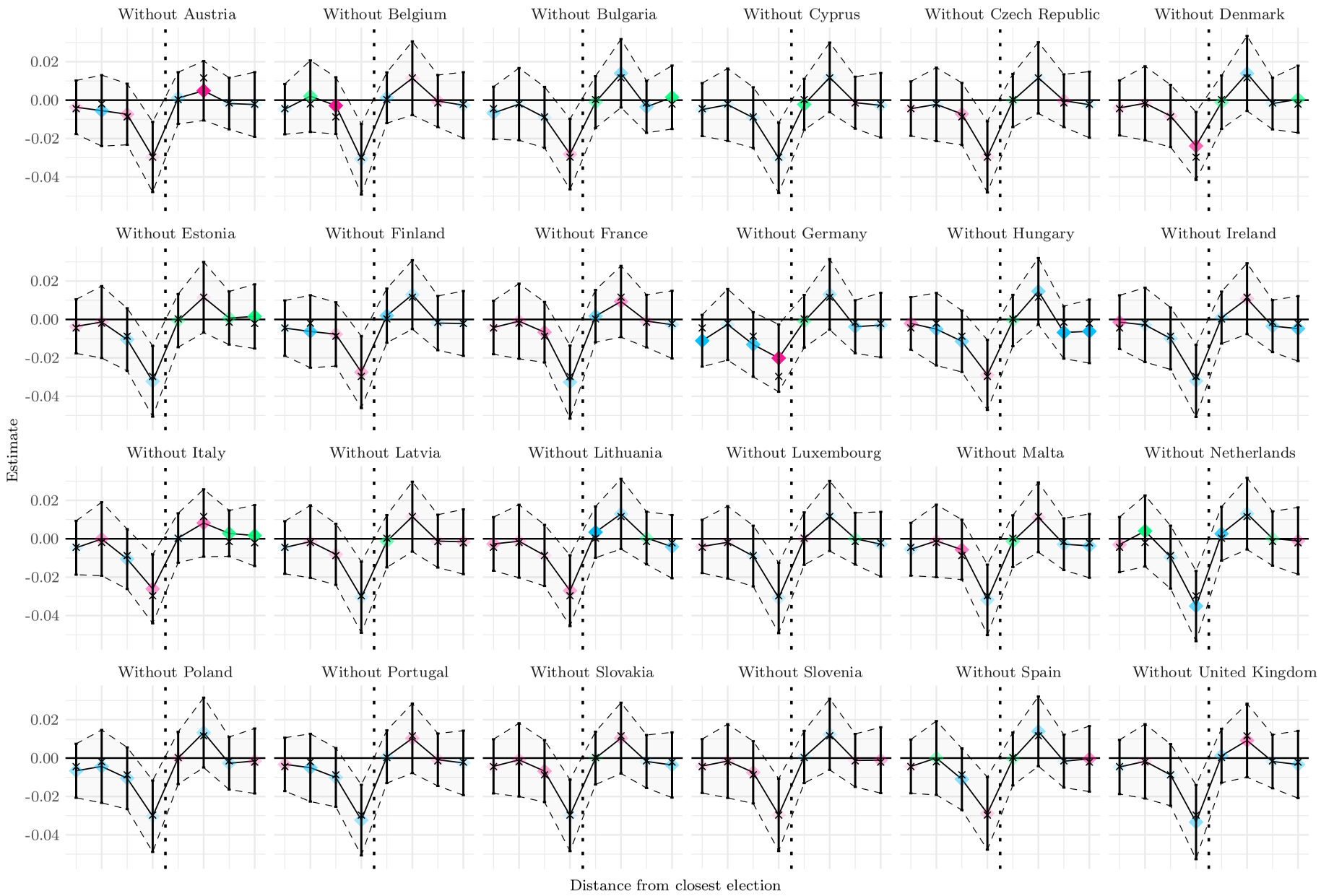


Figure A.10: Net changes around elections leaving each country out once

Notes: Diamonds in green indicate that leaving the particular country out changes the sign of the coefficient compared to the original regression (the estimates of which are always marked by black crosses), red symbolizes a coefficient with the same sign but moving closer to zero, blue means the coefficient moves further away from zero if the country is excluded. The opacity of the coefficient represents how much the coefficient from the specific regression changes in absolute terms relative to the largest absolute shift (which has zero transparency) in each interval over all 24 regressions.

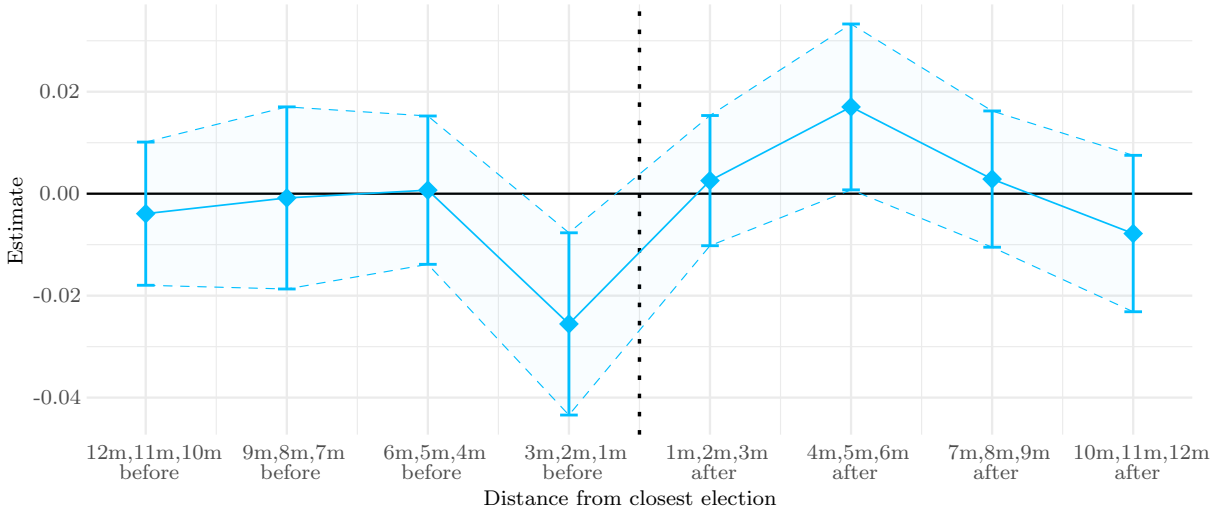
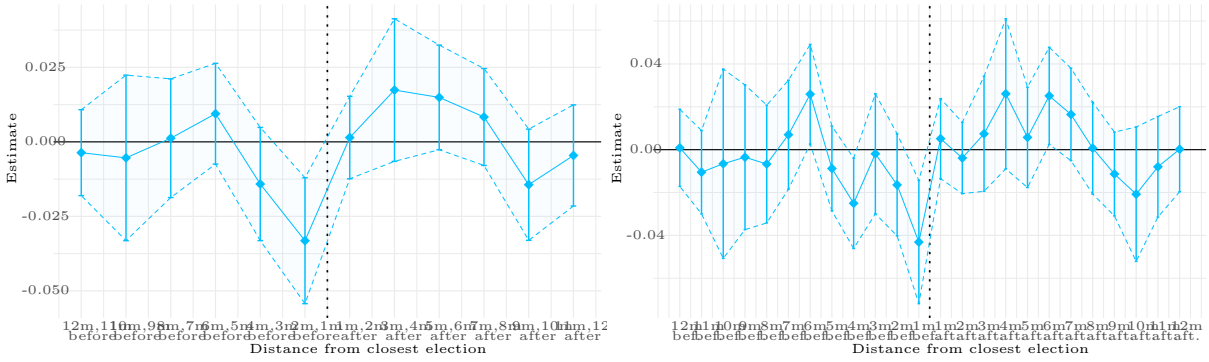


Figure A.11: Net changes in satisfaction around elections with all 28 countries (without interval-specific country weights)

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.01. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regression based on 1.45M observations with an adjusted R^2 of 0.24. 95% confidence intervals (standard errors clustered on 1,650 country-wave clusters) shown.



(a) 60-day intervals

(b) 30-day intervals

Figure A.12: Net changes in life satisfaction around elections with 60-day and 30-day intervals and all 28 countries (without interval-specific country weights)

Notes: Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.01. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. Country-specific calendar week, year, and day of the week fixed effects included. OLS regressions based on 1.45M observations with adjusted R^2s of 0.24 and 0.24. 95% confidence intervals (standard errors clustered on 1,650 country-wave clusters) shown.

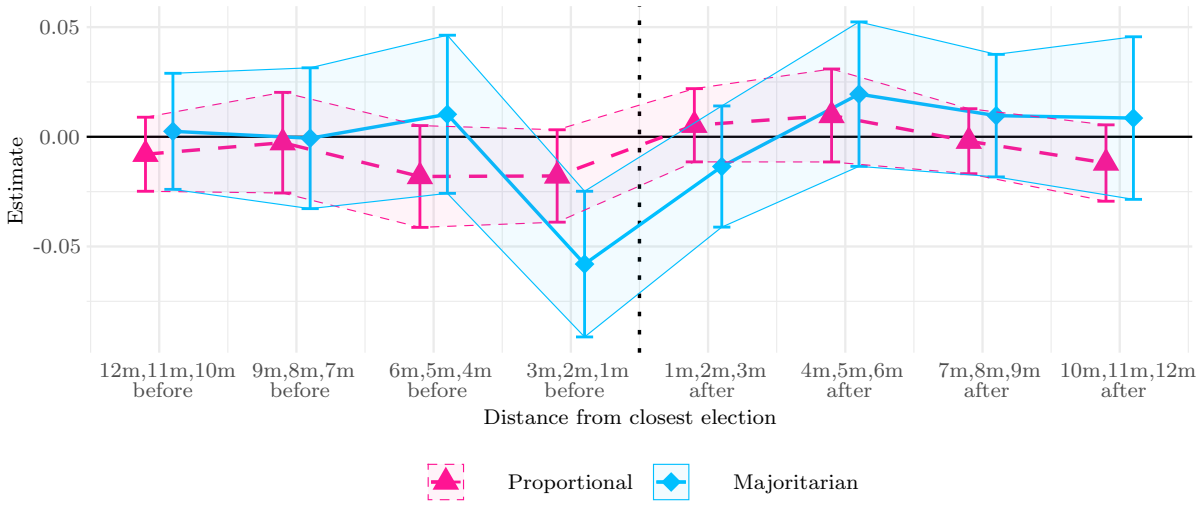


Figure A.13: Net changes in life satisfaction around elections over electoral systems, with global fixed effects

Notes: Proportional consists countries in which all citizens vote in single-member districts, majoritarian consists of countries in which not all citizens vote in single-member districts. Average life satisfaction on the 1–4 scale in the reference period (720–361 days before and 361–720 days after elections) is 3.08 in proportional and 2.92 in majoritarian. Controls include age, age^2, gender, occupation, age finished education, marital status, GDP p.c. growth and unemployment rate in the country during the year of the interview. All variables globally centered around country-specific calendar week, year, and day of the week fixed effects. OLS regressions with interval-specific country weights based on 0.83M and 0.45M observations with adjusted R^2s of 0.06 and 0.08. 95% confidence intervals (standard errors clustered on country-wave level) shown.