

Softening the corrective effect of populism: populist parties' impact on political interest

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ABSTRACT

Populist appeals to 'pure people' have been theorised to mobilise previously disengaged citizens. However, this 'corrective consequence' has found weak support in empirical research. This finding is consistent with studies that suggest that novel campaign appeals and new political offerings have a negligible effect on turnout. As a consequence, this research proposes a distinction between the behavioural and attitudinal effects of populism and introduces a softened revision to the corrective argument: Despite a negligible behavioural impact on individuals' turnout propensity, the electoral success of populism is nevertheless associated with an attitudinal change: increased political interest. This proposition is supported by two empirical tests. The first detects this dynamic among 232,208 respondents in 136 national election studies from 16 Western countries (1970–2017). The second analysis, which uses an improved causal identification strategy, uncovers an analogous effect in the *GESIS Panel* (2014–17, Germany). Hence, the emergence of populist parties can stimulate political interest among citizens, even though it does not necessarily lead to increased turnout.

KEYWORDS Corrective effect of populism; political parties; political interest; voter attitudes

Populist parties target the marginalised 'pure people' who are often disengaged from democratic processes (Osuna and Javier 2021; Mudde 2007). Accordingly, scholars have portrayed populism as both *a threat* and *a corrective* for democratic regimes. 'Threat' stresses the negative effects of populist politics on the rule of law, and it often positions populism as the antithesis to democracy (e.g. Rummens 2017). The 'corrective' stream, initiated by the work of Mudde and Rovira Kaltwasser

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(2012), argues that populist appeals – particularly the moral call to protect ‘pure people’ from the ‘corrupt elite’ (Osuna and Javier 2021) – can mobilise (previously) disengaged citizens and thus improve the insufficient inclusiveness of contemporary democracies.

The corrective effect of populism led scholars to examine whether the proliferation of populist appeals can contribute to increasing levels of political participation – especially turnout rates. So far, limited influence (at best) has been discovered (see, e.g. Ardag *et al.* 2020; Houle and Kenny 2018; Huber and Ruth 2017; Huber and Schimpf 2017; Schwander *et al.* 2020; Zaslove *et al.* 2021). The most conclusive effects, though still weak, deal with non-electoral forms of political participation (Anduiza *et al.* 2019; Marx and Nguyen 2018; Pirro and Portos 2021) and specific segments of society (Immerzeel and Pickup 2015), and they appear in specific contexts (Leininger and Meijers 2021) – particularly in polarised systems (see Hartevelde and Wagner 2022).

Reflecting these findings, we argue that linking the contemporary growth of populist parties to an increase in electoral participation is tenuous, because neither novel campaign appeals nor new political offerings are expected to have a strong impact on the decision to vote (Kittilson and Anderson 2010; Smets and van Ham 2013). However, the specific style of populist communication is nevertheless strong enough to stimulate people’s attitudes (Marx and Nguyen 2018) and increase their (otherwise stable) interest in politics (Prior 2010). Therefore, we disentangle the behavioural and attitudinal effects of populism and propose a softened version of the corrective argument: An increase in the electoral strength of populist parties, and the related proliferation of populist appeals in public discourse, attracts the attention of citizens who are marginally involved with politics and provides them with a new motivation to (re)engage.

The softer version of the corrective argument finds empirical support in both of the tests we conduct. First, we analyse the answers from 232,208 respondents who were included in a database of 136 national election surveys fielded between 1970 and 2017 in 16 Western democracies. We employ three-level hierarchical linear mixed-effect models and identify populist parties based on the *V-Party* project (Lührmann *et al.* 2020). This analysis shows that the electoral success of these parties positively correlates with citizens’ interest in politics. Second, we use longitudinal *GESIS Panel* data (GESIS 2020) to improve causal identification. This test also reveals a *softer* corrective effect for populism that emerged in Germany between 2014 and 2017.

Our cross-national inquiry suggests that the growing strength of populist parties elevates political interest across population subgroups at equal rates, including: more and less educated; poorer and richer; more

and less satisfied with democracy; and centrist and ideologically extreme. The same finding emerges from our longitudinal data from Germany. The absence of any heterogeneous treatment effects suggests that socio-economically disadvantaged citizens are not more prone to becoming politically interested when populist parties increase their electoral appeal, despite the fact that these parties draw disproportionately high support from this group (e.g. Spruyt *et al.* 2016; Gidron and Hall 2020).

On the sources of populist success: a literature review

The emergence of populism is often associated with the insufficient inclusiveness of contemporary democracies (see, e.g. Marx and Nguyen 2018). Mouffe (2005) argues that mainstream political parties became too pragmatic and their conscious conflict-avoidance strategy made them unable to address the issues relevant for a growing part of society. In response, voters' dissatisfaction grew, producing increasing disengagement from politics (Gidron and Hall 2020). This resentment has been successfully targeted by populists (Spruyt *et al.* 2016; Meléndez and Rovira Kaltwasser 2019).

Additional reasons have contributed to the success of populism. One is its novel appeal. The prevailing definition considers populism to be a discourse or an ideology with a specific set of ideas (known as the *ideational approach*, see Hawkins *et al.* 2019). At its thin-centred core, populism separates society into two seemingly antagonistic groups: 'pure people' and the 'corrupt elite'. To make fully legitimate political decisions, politicians must follow the former's will (Osuna and Javier 2021). Even though populist parties adhere to various host ideologies (Mudde 2007), these play only a marginal role in activating the electorate(s) (Akkerman *et al.* 2017). It is mainly the anti-elite sentiment and people-centrism that leads voters across the ideological spectrum – from the left- to the right-wing – to express disagreement with mainstream politics by casting their ballot for a populist party (Rooduijn 2018).

Populists call for a deeper involvement of the 'average person' in political life (Osuna and Javier 2021). This moral call to protect 'the people' is released in a highly discredited and diminished political context (Hay 2007), where moral outrage increases individuals' political engagement (Goodwin *et al.* 2001). Stressing the need for morality in politics (Manucci and Weber 2017; Rooduijn 2014), the accentuation of injustice and the responsibility of the 'corrupt elite' should trigger moral outrage among voters (Marx and Nguyen 2018; Spruyt *et al.* 2016).

This response is also generated by the confrontational style of populist communication (Ylä-Anttila 2018). Populist leaders over-simplify political issues (Golder 2016; Manucci and Weber 2017), and use colloquial

language (Bischof and Senninger 2018) combined with calculated attacks on political conventions and correctness (Mudde 2007). Together, these explain why populists have been successful in using media, which is biased towards sensationalistic forms of communication (Rooduijn 2014), to reach and influence the public (Love and Windsor 2018). Through such communication strategies populists draw stark ideological differences between themselves and other political figures to convey the impression that party choice still matters (Immerzeel and Pickup 2015; Spruyt *et al.* 2016). Consequently, disillusioned voters direct blame towards mainstream political representatives, hoping that populists will increase the efficiency of democratic mechanisms (Marx and Nguyen 2018; Mudde 2007; Mudde and Rovira Kaltwasser 2012).

Populism as a corrective: softening the argument

We have argued so far that populism motivates dissatisfied citizens to become politically active. From this perspective, Mudde and Rovira Kaltwasser (2012) posit that the emergence of populist parties can act as a corrective: it can increase political participation among disadvantaged groups and improve democratic representation.

However, empirical investigations have not found much evidence linking the proliferation of populist appeals to increased turnout (see, e.g. Akkerman *et al.* 2014; Ardag *et al.* 2020; Houle and Kenny 2018; Huber and Ruth 2017; Huber and Schimpf 2017; Schwander *et al.* 2020). The most conclusive effects, which are still relatively weak, deal with non-electoral forms of political participation (Anduiza *et al.* 2019; Marx and Nguyen 2018; Pirro and Portos 2021), involve specific segments of society (Immerzeel and Pickup 2015), and appear only in specific contexts – like highly polarised systems (Harteveld and Wagner 2022). They have also been detected in some Central and Eastern European countries, where populists have already secured parliamentary representation (Leininger and Meijers 2021). A weak effect of populist appeals on turnout has been confirmed in an experimental setting (Ardag *et al.* 2020), where individuals with populist attitudes reported lower participation rates. However, emphasising populism in the actions of political elites did not make them more likely to vote. Hence, populist attitudes are indeed more characteristic for democratically passive citizens, but exposing individuals to populism by political leaders (in an experimental setup) does not boost their turnout.

These findings, which may appear surprising, beg an explanation. As summarised by Franklin (2004), the literature is dominated by three broad theoretical frameworks explaining individual-level turnout: the availability

of resources (e.g. time, money, civic skills); socialisation (e.g. family/social background, formative experiences); and institutional context (e.g. permissiveness of the electoral system). Novel campaign appeals and new political offerings are not included here, as they have been shown to be inconsequential (Kittilson and Anderson 2010). Therefore, empirical evidence supplies weak reasons to expect that the appearance of new populist parties will turn passive citizens into voters.

Nevertheless, populism can stimulate individuals at an attitudinal level. As argued by Prior (2018: 4), political interest ‘is an internal disposition, clearly distinct from a behaviour ... Just as participation can occur in the absence of political interest, it is possible to be interested in politics without participating in it.’ This distinction between the behavioural and attitudinal effects of populism is found in the empirical data as well: high levels of political interest are documented among populist party supporters (see Meléndez and Rovira Kaltwasser 2019; van Kessel *et al.* 2021; Van Hauwaert and van Kessel 2018), yet only a marginal effect on political participation is found (see Akkerman *et al.* 2014; Ardag *et al.* 2020; Houle and Kenny 2018; Huber and Ruth 2017; Huber and Schimpf 2017; Schwander *et al.* 2020).

In summary, we argue that linking the growth of populist parties to an increase in electoral participation is tenuous, except under very specific conditions. However, there are strong reasons to expect effects on political interest, which is more likely to be influenced by novel campaign appeals (Marx and Nguyen 2018). Given that political interest is a strong determinant of electoral participation (see Smets and van Ham 2013: 354–55), this might sound contradictory. There are several reasons why we argue it is not. First, an increase in political interest is not enough to become involved – people must also have sufficient time and resources (Prior 2018: 264–65). Second, political interest may increase predominantly among those who are already participating (Meléndez and Rovira Kaltwasser 2019; van Kessel *et al.* 2021; Van Hauwaert and van Kessel 2018), which limits the space for a positive effect on turnout. Third, the eventual effect on turnout can be delayed because, as we argue, it takes repeated encounters with populist appeals to produce a behavioural effect. All these strongly point to the need to disentangle the attitudinal and behavioural impacts of populism.

Prior (2018) distinguishes between two types of political interest: *situational*, where an environmental stimulus triggers an affective reaction; and, subsequently, *dispositional* interest, which includes the expectation of gratification in repeated interactions. Applied to our topic, a populist appeal can initially stimulate situational interest, specifically a fading attitudinal response, as depicted in the left part of Figure 1. Repeated encounters with this appealing populist rhetoric can reinforce the more

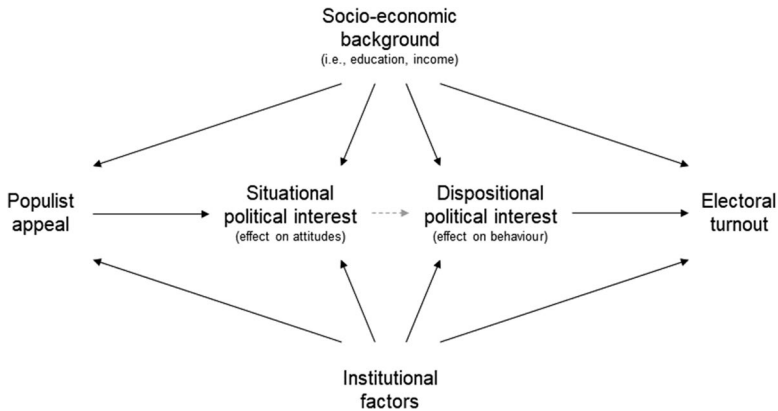


Figure 1. Populist appeals' influence on political interest and turnout. Notes: ⁽¹⁾ The arrow between *situational* and *dispositional* political interest is depicted as dashed and grey because only repeated encounters with populist discourse will generate sufficient situational interest to pass the threshold that spills over into dispositional interest, and lead to turnout. ⁽²⁾ Institutional factors (i.e. electoral system proportionality, polarisation) are features of the political context. All other factors depicted in the graphic refer to individual features.

perennial, dispositional type of interest. This is strong enough to impact one's decision to vote, as depicted in the right part of Figure 1. The reason why the connection is visualised as a dashed grey arrow is this uncertainty – whether situational interest will turn into dispositional interest and affect behaviour.

One reason the link is weak is that people are fairly successful in calibrating their political encounters according to their pre-existing political interest (Prior 2018: 319–20). Therefore, these encounters rarely modify one's level of political interest, which remains remarkably stable over the life cycle (Prior 2010). For our analysis, the overall stability of political interest implies that we conduct a hard test: measuring the impact of the proliferation of populism on an otherwise stable attitude.

Building on this theoretical discussion, we disentangle the behavioural and attitudinal effects of populism. Focussing on the attitudinal effects, we propose a softer version of the corrective argument:

Hypothesis 1. The presence or increasing electoral support of populist parties in a political system is associated at the individual level with a higher level of (situational) political interest.

Corrective effect among specific groups

In addition to the general effect, we argue that the proliferation of populist communication has a stronger influence on those who are more

responsive to anti-elite rhetoric (Marx and Nguyen 2018). Populist parties disproportionately draw their support from groups – lower socio-economic segments primarily defined based on income and education (Golder 2016; March and Rommerskirchen 2015; Spruyt *et al.* 2016) – who might be portrayed as ‘losers of globalization’ (Kriesi *et al.* 2008).¹ These people are most likely to be neglected by established parties (Mouffe 2005), which fuels their dissatisfaction with mainstream politics (Gidron and Hall 2020) and which can make them more responsive to populist appeals. Disinterested individuals are also less likely to engage with politics in media, though populist actors hold an edge in this regard (Rooduijn 2014). They have used social media to amplify their message in a very effective manner, such as in the 2016 Brexit and Trump campaigns. Tools like Facebook, Twitter, and Instagram provided (almost) unrestricted opportunity for their ideological messages to reach audiences that would normally not follow more traditional media (Engesser *et al.* 2017). Therefore:

Hypothesis 2. The positive effect of populist party strength on political interest is stronger for citizens from a lower socio-economic background, including those with lower income (H2₁) and those with lower levels of education (H2₂).

Measurement, data, and methods

Populism has been defined from different perspectives: a form of (thin-centred) ideology; a political strategy; and a discursive/performative style of political communication (Osuna and Javier 2021). Thanks to the concise definition readily applicable in empirical research (Mudde 2007: 35), the ideational approach (Hawkins *et al.* 2019) is the most frequently chosen perspective (Meijers and Zaslove 2021: 374). This approach was adopted in the *V-Party* project (Lührmann *et al.* 2020), which surveys experts on the dimensions of party identity. Our research relies on this database to identify populist parties.²

We use two dimensions of populist discourse: the tendency to consider ‘the people’ as the source of political authority, and the extent of anti-elite references. All parties are scored between 0 and 4 on each dimension for each year, which means an arbitrary threshold is needed to distinguish between populist and non-populist parties. We use 3 as the cut-off, indicating that this dimension is important in the party’s discourse and that it gets used more than half the time in speeches. We therefore code a party as populist if it has a score of 3 or 4 on at least one of the two dimensions – people-centrism and anti-elitism – provided that the other dimension has a score of at least 2. This procedure produces variation both between parties and across time in a parties’ populist nature.

Since populism operates at a systemic level, we aggregate party-specific information in three different ways. Our primary indicator captures how electorally successful populism has been: it measures the *cumulative vote share* of populist parties in the *V-Party* data for a given year. Hence, it can range from 0 (no populist party receives votes) to 100 (all parties receiving votes are populist). The second measure probes the strength of populist ideas in the *entire* party system by constructing a *weighted mean of parties' populism score*, using their vote shares as weights. The third measure is a simple dichotomous indicator for whether there was at least one populist party represented in parliament *at the moment of the election* (following the findings of Leininger and Meijers 2021).

Data

The individual-level information on which we test our hypotheses comes from a custom-built data set that merges all of the publicly available national election studies from a set of advanced industrial democracies. Wherever possible, preference was awarded to election studies retrieved from their country-specific repositories (e.g. FORS in Switzerland, the ESRC Data Archive in the UK, the Norwegian Social Science Data Services). Where this was not feasible, we retrieved data from the *World Values Surveys* or the *Comparative Study of Electoral Systems*. Our primary goal was to maximise the longitudinal coverage of elections while maintaining the cross-national comparability of question formats and scales. We managed to cover the elections presented in Table 1: 136 studies in 16 countries that provided a total of 232,208 complete individual-level responses.

For these studies we have information on individuals' level of interest in politics, as well as a minimal set of predictors for their political orientation.³ For political interest, comparability was maintained by using only items that referred to generic interest in political developments, unconnected to a specific time period. Throughout time and across countries, the original scales used to measure interest ranged in size from 3 to 11 points. To address this, we rescaled all of the versions of the indicator on a common 0–1 scale, provided that the endpoints of the scale used comparable labels.

Statistical controls

For controls at the individual level we rely on age, age squared, gender, income, and education. The pared-down specification is needed to prevent sample attrition, but also due to the difficulty of specifying a causal direction between two attitudes (e.g. trust and political interest). The

Table 1. Countries and elections in the sample.

| COUNTRY | YEARS |
|----------------|--|
| Australia | 1977, 1993, 1996, 1998, 2001, 2004, 2007, 2010, 2013 |
| Belgium | 1995, 1999 |
| Canada | 1974, 1979, 1984, 1988, 1997, 2000, 2004, 2006, 2008, 2011, 2015 |
| Denmark | 1977, 1981, 1984, 1987, 1990, 1994, 1998, 2001, 2005, 2007, 2011 |
| Finland | 1972, 1975, 1983, 1987, 2003, 2007, 2011, 2015 |
| France | 1978, 1988, 2002, 2007, 2012 |
| Germany | 1972, 1976, 1994, 1998, 2002, 2009, 2013 |
| Iceland | 1991, 1995, 1999, 2003, 2007, 2009, 2013 |
| Italy | 1972, 1983 |
| Netherlands | 1977, 1981, 1982, 1986, 1994, 1998, 2002, 2003, 2006, 2010, 2012 |
| New Zealand | 1981, 1990, 1993, 1996, 1999, 2002, 2005, 2008, 2011, 2014 |
| Norway | 1973, 1977, 1981, 1985, 1989, 1993, 1997, 2001, 2005, 2009, 2013 |
| Sweden | 1970, 1976, 1979, 1982, 1985, 1988, 1994, 1998, 2002, 2006 |
| Switzerland | 1979, 1995, 1999, 2003, 2007, 2011, 2015 |
| United Kingdom | 1974, 1979, 1997, 2001, 2005, 2010, 2015, 2017 |
| United States | 1972, 1974, 1976, 1978, 1980, 1982, 1984, 1986, 1988, 1990, 1992, 1994, 1996, 1998, 2000, 2004, 2008 |

Notes: ⁽¹⁾ Of the 136 studies included, 104 are post-election surveys, one is pre-election (Italy in 1983), and 31 are pre-post panels. ⁽²⁾ Of the latter group, the political interest item was asked prior to election day in 13 cases, it was asked after the election in 13 cases, and a clear determination could not be made in 5 cases. ⁽³⁾ These 5 cases (the Netherlands in 1977, 1981, 1986, and 2002; Switzerland in 1979) belong to survey series that are made public in a merged format. The survey documentation is unclear about the timing of the question, and access to the original codebooks or questionnaires could not be secured.

two most important controls are education and income. The level of education is measured with three categories, where *low* education denotes incomplete secondary school or less, *middle* represents completion of secondary school, and *high* represents at least some university study. Similarly, the income measure has three levels – the split is based on country-specific terciles constructed as closely as possible to an even 33.3% split.⁴

At the country-year level, we also use a standard set of controls, which includes the Gini index of net income inequality (from the SWIID data, version 8.1) (Solt 2020). This is associated with lower political engagement (Solt 2008) and plausibly impacts the intensity of populist discourse as well. We also add a proxy for union strength: union density, taken from version 6.0 of AIAS's database of trade union characteristics (ICTWSS) (Visser 2019). Union membership has been associated with increases in political engagement (D'Art and Turner 2007), and it can foster a conflictual perspective on society relations that makes an anti-elite populist message more popular.⁵ We also add two dichotomous indicators for time period, 1985–99 and 2000–17, and contrast them with the pre-1985 period. These control for the possibility of change in how respondents understand the political interest survey item over time.

At the country level, we include a standard set of predictors for political engagement: compulsory voting; mean district magnitude (a proxy

for electoral proportionality); an index of political corruption from the *V-DEM* data (Coppedge *et al.* 2019); and dichotomous indicators for bicameralism, presidentialism, and federalism. These latter three measures are all sourced from the *Comparative Political Data Sets* (Armingeon *et al.* 2018) and they have all been associated with greater political interest and behavioural engagement (Singh 2011; Solt 2008). Compulsory voting, electoral proportionality, and low corruption have also been associated with increased political engagement (Cancela and Geys 2016; Linde and Erlingsson 2013; Stockemer *et al.* 2013), warranting their inclusion.

Modelling strategy

In our data, individuals are nested in elections, and further nested in countries. Our optimal modelling strategy is a hierarchical linear model (HLM) (Gelman and Hill 2007), with country-years as the level-2 grouping, and countries as the level-3 grouping. Our level-3 sample size is insufficient for drawing inferential conclusions (McNeish and Stapleton 2016), so we avoid interpreting the significance of country-level predictors like corruption, presidentialism, and compulsory voting. Nevertheless, as the primary goal is to control for clustering at the country-year level, we continue with the three-level HLM. Level-2 predictors, including our populism indicators, do not run into such inferential challenges, as estimates are produced from a sample of 136 elections. All individual-level and country-year-level predictors were group-mean centred, while country-level predictors were grand-mean centred (Enders and Tofighi 2007).

Results

We first assess whether we can visually discern any relationship between populist party strength and aggregate shifts in political interest. We select six countries in areas that maximise the geographical variance in our data – three continents (i.e. Australia, Europe, North America), and three regional clusters in Europe (i.e. Denmark for Nordic countries, the Netherlands for Benelux, Germany as the case farthest to the East). [Figure 2](#) plots the evolution of aggregate-level political interest and the vote share of populist parties on the political spectrum over time. For comparison, we also add the official VAP turnout to the plot, rescaled on a 0–1 metric.⁶

The figure is more suggestive than conclusive, though evidence exists for a potential link between populist actors and political interest. One prominent example is the steady trend towards a more politically interested Danish polity in the post-1990 period. This accompanies a similarly steady rise in the cumulative vote share for populist parties, though we

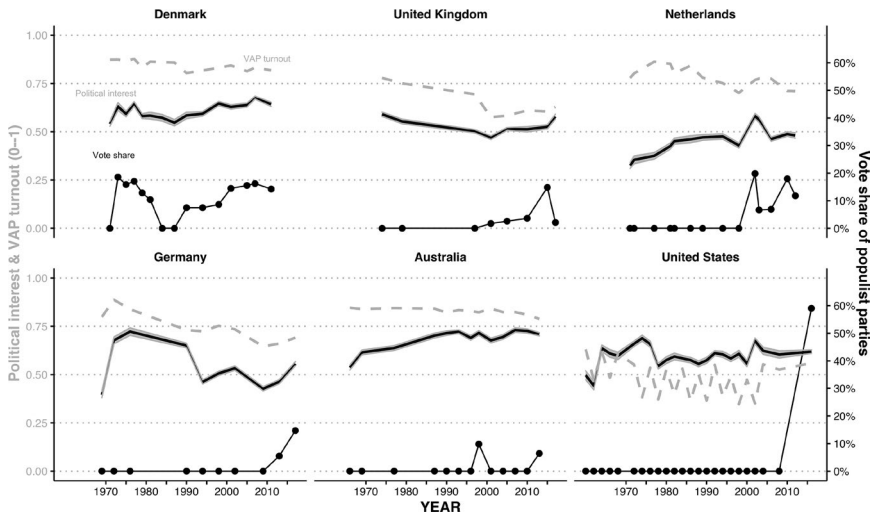


Figure 2. Longitudinal trends in support for populist parties and aggregate-level political interest. Notes: ⁽¹⁾ Dashed light-grey lines present aggregate VAP turnout numbers. Solid dark lines show aggregate-level political interest computed from individual data. Dark circles and connected lines show the cumulative vote share for populist parties. The grey area around the trend for political interest represents a 95% confidence interval. ⁽²⁾ VAP turnout obtained from IDEA data (<https://www.idea.int/data-tools/data/voter-turnout>). ⁽³⁾ Aggregate-level political interest is computed based on harmonised items available in our 136 studies, rescaled on a common 0–1 scale. Each data point represents the average level of political interest in a year. ⁽⁴⁾ The cumulative vote share for populist parties is computed based on V-Party data (<https://www.v-dem.net/vpartyds.html>). A party was classified as populist if it scored at least a 3 ('important') on either people-centrism or anti-elitism, provided that the score on the other dimension was at least 2. ⁽⁵⁾ The right vertical axis records the populist vote share; the left vertical axis records both VAP turnout and aggregate-level political interest. ⁽⁶⁾ The dramatic rise in populists' electoral success in the US is due to the Republican Party being classified as populist.

do not find this reflected in a higher turnout. Even smaller periods of time match our proposed association. A small rise in political interest, between 2005 and 2007, mirrors the rising electoral fortunes of populist parties in the country in the aftermath of the 'cartoon' controversy.⁷ As this event subsided from memory, though, and factoring in the penalty incurred for supporting the governing coalition, the Danish People's Party experienced electoral decline in 2011, which matches a small decline in aggregate political interest.

From the late 1990s, a similar connection can be observed in the UK: over about 15 years we see a gradual increase in both support for populist forces and political interest. No such link could be established in the short term, where rising support for UKIP between 2010 and 2015 has not generated any noticeable shift in political interest (the changes we

see on the plot are well within the margin of error). A similar, slow-moving process can be observed in Switzerland over approximately three decades, though, as before, it is not reflected in aggregate turnout rates. On the other hand, short-term dynamics are clearly visible in the Netherlands, such as the dramatic increase in political interest associated with the electoral success of the Lijst Pim Fortuyn in 2002. In the other countries displayed in [Figure 2](#), the presumed link between political interest and populist political actors is not visible, such as in Germany, Australia, and the US.

Multivariate analyses

We turn to the multivariate analyses for clearer answers, using the *cumulative vote share* of populist parties as the primary indicator. We present results from three-level hierarchical linear models with a continuous outcome measured on a 0–1 scale. First, we only test the plausibility of a direct link between populist parties and political interest ([Table 2](#)). We follow this with an investigation of the heterogeneous effects for groups at opposite ends of the socio-economic scale ([Table 3](#)).

The main take-home point of [Table 2](#) is the connection between the growing strength of populist parties and increases in citizens' political interest. The main piece of evidence comes from Model 2, where we find a statistically significant effect for the cumulative vote share of populist parties on individual-level political interest: $\beta = 0.132$, $SE = 0.056$, $p < .05$. A higher degree of electoral success for these parties is, on average, associated with a higher level of political interest for individuals. To check for consistency, we test two additional proxies: the average populism score in the party system and a dichotomous measure of whether a populist party is in parliament at the moment of the election. The results for the average populism score in Model 3 reinforce our confidence in the findings: we uncover a statistically significant impact of populist party discourse on political interest ($\beta = 0.112$, $SE = 0.039$, $p < .01$). For the existence of populist parties in the legislature, however, there appears to be no association (see Model 4). This discrepancy could be due to the different timing of the measurement. For this latter indicator, political interest is measured three to four years after populist parties enter parliament; this time gap might be sufficient to make key subgroups in the population disengage and become disillusioned with politics. All in all, though, this portion of the analysis provides initial support for Hypothesis 1.

In terms of the individual-level controls, we observe a by-now-familiar pattern. Younger, less-educated, and poorer women exhibit lower levels of political interest, compared to older, more educated, and affluent men. When turning to the country-level controls, some of the findings

Table 2. Direct positive effect of populist parties' strength on political interest among individuals.

| | Model 1 | Model 2 | Model 3 | Model 4 |
|----------------------------------|------------|------------|------------|------------|
| Populist parties (vote share) | | 0.132* | | |
| | | (0.056) | | |
| Average populism score | | | 0.112** | |
| | | | (0.039) | |
| Populist parties (in parliament) | | | | 0.006 |
| | | | | (0.015) |
| Age | 0.429*** | 0.429*** | 0.429*** | 0.429*** |
| | (0.019) | (0.019) | (0.019) | (0.019) |
| Age squared | -0.144*** | -0.144*** | -0.144*** | -0.144*** |
| | (0.019) | (0.019) | (0.019) | (0.019) |
| Male | 0.056*** | 0.056*** | 0.056*** | 0.056*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Low income | -0.068*** | -0.068*** | -0.068*** | -0.068*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Middle income | -0.037*** | -0.037*** | -0.037*** | -0.037*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Low education | -0.148*** | -0.148*** | -0.148*** | -0.148*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Middle education | -0.082*** | -0.082*** | -0.082*** | -0.082*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Income inequality | 0.003 | 0.000 | 0.005 | 0.005 |
| | (0.034) | (0.033) | (0.033) | (0.034) |
| Union density | 0.180 | 0.154 | 0.114 | 0.185 |
| | (0.100) | (0.099) | (0.099) | (0.101) |
| Compulsory voting | 0.058 | 0.059 | 0.056 | 0.058 |
| | (0.066) | (0.066) | (0.066) | (0.066) |
| Corruption | -0.935* | -0.961* | -0.937* | -0.938* |
| | (0.408) | (0.406) | (0.410) | (0.408) |
| Mean dist. mag. | -0.021 | -0.022 | -0.022 | -0.021 |
| | (0.013) | (0.013) | (0.013) | (0.013) |
| Bicameral legislature | -0.044 | -0.043 | -0.043 | -0.044 |
| | (0.053) | (0.053) | (0.054) | (0.053) |
| Presidential system | 0.060 | 0.061 | 0.060 | 0.061 |
| | (0.053) | (0.052) | (0.053) | (0.053) |
| Federal system | 0.005 | 0.003 | 0.003 | 0.004 |
| | (0.058) | (0.058) | (0.058) | (0.058) |
| Period: 1985-1999 | -0.002 | -0.008 | -0.001 | -0.002 |
| | (0.013) | (0.013) | (0.013) | (0.013) |
| Period: 2000-2017 | 0.048** | 0.040** | 0.043** | 0.047** |
| | (0.015) | (0.015) | (0.015) | (0.015) |
| (Intercept) | 0.521*** | 0.526*** | 0.522*** | 0.521*** |
| | (0.019) | (0.019) | (0.019) | (0.019) |
| AIC | 43919.607 | 43920.108 | 43918.253 | 43928.046 |
| BIC | 44137.070 | 44147.927 | 44146.071 | 44155.865 |
| Log Likelihood | -21938.803 | -21938.054 | -21937.126 | -21942.023 |
| Num. obs. | 232208 | 232208 | 232208 | 232208 |
| Num. groups: country year | 136 | 136 | 136 | 136 |
| Num. groups: country | 16 | 16 | 16 | 16 |
| Var: country year: (Intercept) | 0.003 | 0.003 | 0.003 | 0.003 |
| Var: country: (Intercept) | 0.004 | 0.004 | 0.004 | 0.004 |
| Var: Residual | 0.071 | 0.071 | 0.071 | 0.071 |

Notes: ⁽¹⁾ *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. ⁽²⁾ Three-level hierarchical linear models were used: individuals nested in country-years, and further nested in counties. ⁽³⁾ The outcome is an individual's self-reported level of political interest, rescaled on a 0–1 scale. ⁽⁴⁾ Income and education are proxied with a set of dummy indicators, with reference categories established as high income (top 33% in household income) and high education (at least some college). ⁽⁵⁾ Indicators of populist party strength are computed based on information found in the *V-Party* data.

Table 3. The effect of populist parties' strength on political interest among individuals with low income and low education.

| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 |
|--|-----------|-----------|-----------|-----------|-----------|-----------|
| Populist parties (vote share) | 0.130* | 0.135* | | | | |
| | (0.056) | (0.056) | | | | |
| Populist parties (vote share) * Low income | 0.049 | | | | | |
| | (0.041) | | | | | |
| Populist parties (vote share) * Low education | | 0.080 | | | | |
| | | (0.066) | | | | |
| Ave. populism score | | | 0.112** | 0.112** | | |
| | | | (0.039) | (0.039) | | |
| Ave. populism score * Low income | | | -0.022 | | | |
| | | | (0.028) | | | |
| Ave. populism score * Low education | | | | 0.014 | | |
| | | | | (0.047) | | |
| Populist parties (in parl.) | | | | | 0.006 | 0.008 |
| | | | | | (0.015) | (0.015) |
| Populist parties (in parl.) * Low income | | | | | 0.005 | |
| | | | | | (0.010) | |
| Populist parties (in parl.) * Low education | | | | | | 0.007 |
| | | | | | | (0.017) |
| Low income | -0.064*** | -0.066*** | -0.064*** | -0.066*** | -0.064*** | -0.066*** |
| | (0.003) | (0.002) | (0.003) | (0.002) | (0.003) | (0.002) |
| Middle income | -0.038*** | -0.035*** | -0.038*** | -0.035*** | -0.038*** | -0.035*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Low education | -0.149*** | -0.160*** | -0.149*** | -0.160*** | -0.149*** | -0.160*** |
| | (0.002) | (0.006) | (0.002) | (0.006) | (0.002) | (0.006) |
| Middle education | -0.082*** | -0.091*** | -0.082*** | -0.091*** | -0.082*** | -0.091*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| (Intercept) | 0.524*** | 0.528*** | 0.523*** | 0.525*** | 0.520*** | 0.525*** |
| | (0.019) | (0.019) | (0.019) | (0.019) | (0.019) | (0.019) |
| Num. obs. | 232208 | 232208 | 232208 | 232208 | 232208 | 232208 |
| Num. groups: country year | 136 | 136 | 136 | 136 | 136 | 136 |
| Num. groups: country | 16 | 16 | 16 | 16 | 16 | 16 |

Notes: ⁽¹⁾ *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. ⁽²⁾ Three-level hierarchical linear models were used: individuals nested in country-years, and further nested in counties. ⁽³⁾ The outcome is an individual's self-reported level of political interest, rescaled on a 0–1 scale. ⁽⁴⁾ Income and education are proxied with a set of dummy indicators, with reference categories established as high income (top 33% in household income) and high education (at least some college). ⁽⁵⁾ Indicators of populist party strength are computed based on information found in the *V-Party* data. ⁽⁶⁾ The full set of estimates, along with measures of model fit, can be found in Section 11 of the Online Appendix.

reinforce the prior results in the literature. We are unable to replicate the effect identified by Solt (2008) with respect to either presidentialism, federalism, or bicameralism, but we identify a similar null effect for compulsory voting. Neither are we able to uncover an effect for income inequality at an aggregate level, or for electoral proportionality. On the other hand, we find the expected effect for corruption (Stockemer *et al.* 2013): a higher level of political corruption depresses overall political interest among citizens. A likely transmission mechanism is citizens' sense

of external efficacy: pervasive corruption decreases the value of making a political choice because all parties are suspected to be tainted. We avoid reading too much into this, though, given that corruption is added at the country level, with a sample of only 16 cases.

We also fail to find an effect for union density. Though the estimate is always positive, it is never statistically significant. On the other hand, we detect an effect for one of our time period indicators, suggesting change over time: either respondents' understanding of a standard survey question has changed or a secular rise in the levels of political interest not captured by our time-varying predictors has occurred.⁸

We continue by investigating whether there is an effect heterogeneity among socio-economic groups based on income and education, as suggested in Hypothesis 2. Given the socio-demographic groups targeted by populist radical-right parties with their appeals (Werts *et al.* 2013) and from which they draw disproportionate support (Golder 2016; March and Rommerskirchen 2015; Spruyt *et al.* 2016), we posit that the effect we uncover in Table 2 will be stronger for citizens with a lower socio-economic status. The models we present in Table 3 test this by means of a set of cross-level interactions between our main indicators for populist party strength and, sequentially, individual-level income and education.

The results in Table 3 indicate that there is no difference in the effect of populist party strength between voters at different income or education levels (the full set of results are in Table 17 in Section 11 of the Online Appendix). Though the interaction effects with income and education are positive in two out of the three cases, as we expected, the effects never reach conventional levels of significance. As they stand, our results offer good support for Hypothesis 1, but no support for Hypothesis 2. The gap between wealthier and more educated voters and their more disadvantaged peers is *the same* in contexts where populists have experienced varying electoral success.

Sensitivity checks

Before concluding, we submit our admittedly mixed results to a few robustness tests. First, in Section 6 of the Online Appendix, Table 7 displays estimates obtained with a different system for coding political interest, specifically as an ordinal-level indicator. We recode political interest into four categories, using a standard set of cut-offs: 0–0.25, 0.25–0.50, 0.50–0.75, and 0.75–1 (see Figure 2 in the Online Appendix for a side-by-side comparison of the original and transformed distribution). We then re-estimate some of the specifications we have tested so far, using a three-level hierarchical ordinal mixed-effects model with a

logit link function. Overall, our results remain consistent with those presented in our main analysis.⁹ The interaction effect between education or income and populist parties is again positive, but not statistically significant.

We also check whether using a lagged version of our main predictor, the cumulative vote share of populist parties, changes any of our results. We estimate three mixed-effect models with the lagged version of the predictor in Table 8 in the Online Appendix. The results are only partly consistent with those already reported: the effect of party strength on political interest is positive, but no longer statistically significant. Neither of the interactions with education or income are significant by conventional standards. We also try an alternative source for data on the cumulative vote share of populist parties: the *PopuList 2.0* data (see Table 10 in Section 7 of the Online Appendix). Although the estimates of the effect of populist party strength are in the same direction as reported here, they are no longer statistically significant due to a greatly reduced sample size (i.e. 82 country years). Table 9 in Section 6 of the Online Appendix checks whether sequentially dropping countries from the sample changes the estimate of interest – we find that it does not. We lose significance for our estimates only when we drop Canada, though the direction of effect remains similar.

Finally, we check whether there is additional effect heterogeneity based on four individual-level indicators: age, gender, satisfaction with democracy, and left–right self-placement. We test four such sets of models (Tables 11–14 in Section 8 of the Online Appendix), with ideological placement coded in two ways: as ideological extremity (i.e. distance from the theoretical centre of the scale) and right-leaning preferences (i.e. a dichotomous measure). The full set of estimates shows that there is little effect heterogeneity at play. In most specifications, in fact, we fail to replicate the statistically significant effect of populist party vote share and the average populism score on political interest, though the estimates are consistently positive. Table 11 and 14 are the only sets of models where these two indicators have a statistically significant effect, though, the former case is on a sample of only 71 surveys from 13 countries.

Effect mediation

We have argued that growing populist party strength is unlikely to directly impact turnout – its effect, properly conceptualised, is carried through a longer sequence of attitude activation where political interest plays a key role. We target this logic head-on in Table 4, where we present estimates from three-level multilevel path models that explain individual-level turnout. Our key predictor is populist party strength, which is measured with

Table 4. Three-level multilevel path models of populist parties' effect on turnout and political interest.

| | Model 1 | Model 2 | Model 3 |
|--|---------------------|---------------------|---------------------|
| DV: Turnout | | | |
| Populist parties (vote share) | 0.462 (0.347) | | |
| Populist emphasis (system score) | | 0.501* (0.229) | |
| Populist parties (in parliament) | | | -0.059 (0.089) |
| Political interest | 1.227*** (0.014) | 1.227*** (0.014) | 1.227*** (0.014) |
| DV: Political interest | | | |
| (Intercept) | 0.546*** (0.025) | 0.546*** (0.025) | 0.546*** (0.025) |
| Populist parties (vote share) | 0.150* (0.059) | | |
| Populist emphasis (system score) | | 0.122** (0.040) | |
| Populist parties (in parliament) | | | 0.009 (0.016) |
| Indirect effect of populism on turnout (through political interest) | 0.171* (0.068) | 0.151** (0.049) | 0.011 (0.019) |
| Num. obs. | 220195 | 220195 | 220195 |
| Num. groups: country year | 134 | 134 | 134 |
| Num. groups: country | 16 | 16 | 16 |

Notes: ⁽¹⁾ *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. ⁽²⁾ The reduction in sample size compared to the models reported in the main manuscript is due to the addition of individual-level turnout as outcome. ⁽³⁾ Specifications are three-level multilevel path models of self-reported turnout at the individual level, with a *probit* link function, and with a direct effect from populist party strength on both political interest and turnout. Political interest in an endogenous variable in these specifications, measured on a 0–1 scale. ⁽⁴⁾ All models were estimated with Mplus 8.4, using a Bayesian estimator; no thinning was performed for the two chains used. Due to non-normality in the posterior distributions, estimates are sometimes significant even with a t-value below 1.96. ⁽⁵⁾ Statistical controls at all levels are excluded from the table; full tables of results are presented in Section 9 of the Online Appendix. ⁽⁶⁾ All turnout questions refer to self-reported behaviour in past elections; in some specific cases (i.e. the Netherlands in 1982 and 2003, Italy in 1985) the question refers to an election from the preceding year.

the same proxies: vote share in the recent election, average populist score in the party system, and the parliamentary representation of populist parties in the *previous* election. Unlike previous models, though, the multilevel path specifications also include an indirect effect pathway to turnout, via political interest. This pathway is of interest here.

The top section of Table 4 presents the estimates for the direct effect of populist party strength on individual-level turnout (the full set of results can be checked in Table 15 in Section 9 of the Online Appendix). Though admittedly mixed, the evidence points with greater weight towards no direct effect: among the three proxies, two have inconsistent directions and never cross a conventional significance threshold. The average systemic level of populism is significantly associated with turnout. This measure incorporates discourse changes from *all* parties on the spectrum; thus, the result may also reflect mainstream parties' adoption

of populist rhetoric after observing populist parties' electoral success. The inconsistency of the evidence leads us to conclude that there is likely no direct effect for populist strength on the turnout decision.¹⁰ The middle section of the table presents estimates from a similar model for the second endogenous variable in the specification: political interest. The results are more consistent than for turnout, with both vote share and the systemic populism score having a positive and significant effect on political interest: both greater strength of populist parties and greater emphasis on populist appeals among all parties are associated with a higher level of political interest.

The bottom section of [Table 4](#) computes the indirect effect of political interest. For a key indicator of populist party strength, namely its vote share, the results confirm our theoretical account. Though its direct effect on turnout is too imprecisely estimated to reach statistical significance, its indirect effect is positive and statistically significant: the growing electoral success of populist parties is associated with higher turnout via an increased level of political interest. The same dynamic is present for our systemic populism score proxy, but it is clearly not at play for the parliamentary representation of populist parties. All in all, the uncovered effects confirm the key transmission mechanism we posit: populist party electoral success leads to increased interest in politics, which only then results in a higher turnout propensity among individuals.

Towards better causal identification: the German case

The evidence presented so far is cross-sectional and comes with the usual limitations to causal inference. Maximising temporal and country coverage placed limits on how many individual-level predictors of political interest could be included. This exposes us to unobserved heterogeneity, which might bias our estimate for political interest. To address this, we re-test the relationship in Germany, using the longitudinal yearly data of the *GESIS Panel* (*GESIS 2020*) between 2013 and 2017.¹¹ A set of standard socio-demographic indicators, along with the respondents' interest in politics and closeness to political parties, are asked every year. This allows us to track variations in political interest over the period of time in which a major populist party, the *Alternative für Deutschland* (AfD), achieved mainstream success in Germany (see [Lees 2018](#)).¹²

We address the problems posed by unobserved heterogeneity with a fixed-effects model. The specification is presented in Equation 1, and includes unit fixed-effects (α_i) to control for this unobserved heterogeneity.¹³ This implies that, although we are better able to control for unobservables, our estimates are still based on cross-sectional variation, as in our earlier multilevel models. Because all respondents are 'exposed' to the

same level of national-level popularity of the AfD, we cannot use this predictor in a similar multilevel approach. Instead, we use an individual-level dichotomous indicator for whether a respondent self-reports being closest to AfD among all of the parties in that year – this is our predictor for political interest. All individuals who report not feeling close to any party are coded as missing in this setup. To assess whether any effect of closeness to AfD on political interest varies across subgroups defined by education or income, we also include these two markers of socio-economic status in our models, along with their interaction with closeness. Finally, we add a set of standard socio-demographic controls (X), such as marital status or union membership.¹⁴

$$Interest_{it} = \alpha_i + \beta_0 Close_{it} + \beta_1 SES_{it} + \beta_2 SES_{it} * Close_{it} + \tau X_{it} + s_{it} \quad (1)$$

The results from the models are presented in Table 5, and confirm Hypothesis 1 again: closeness to the largest populist force in Germany (i.e. the AfD) is associated with a higher level of political interest. Model 1 in Table 5 reveals this effect to be about 0.11 points, which represents a modest increase that is statistically significant. The following two

Table 5. Two-way FE models for the closeness to AfD effect on political interest in Germany.

| | Model 1 | Model 2 | Model 3 |
|-----------------------------|-------------------|-------------------|-------------------|
| Education | −0.056 (0.066) | −0.057 (0.077) | −0.056 (0.066) |
| Income | 0.004 (0.033) | 0.004 (0.034) | 0.005 (0.035) |
| Married (or in partnership) | 0.119 (0.200) | 0.119 (0.200) | 0.127 (0.199) |
| Member of union | 0.134 (0.132) | 0.134 (0.130) | 0.137 (0.131) |
| Close to AfD | 0.110* (0.044) | 0.110* (0.044) | 0.110* (0.044) |
| Close * Education | | −0.008 (0.102) | |
| Close * Income | | | 0.036 (0.022) |
| Resid. SE | 0.175 | 0.175 | 0.174 |
| Num. obs. | 7657 | 7657 | 7657 |

Notes: ⁽¹⁾ *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. ⁽²⁾ The panel is unbalanced, with 2,939 individuals tracked for 1-4 waves. ⁽³⁾ All models contain unit FEs; the weighted FE estimator proposed by Imai and Kim (2021) is used. ⁽⁴⁾ Autocorrelation robust standard errors are used. ⁽⁵⁾ Data comes from the *GESIS Panel*, 2013–17, which tracks a representative cross-section of the German population between two electoral cycles. Political interest is measured on a 5-point scale, from 1 ('not at all') to 5 ('very strong'). ⁽⁶⁾ Income is measured on a 9-point scale, from 1 ('less than 900 EUR') to 9 ('6,000 EUR or more'). ⁽⁷⁾ Education is measured on a 4-point scale, from 1 ('primary education') to 4 ('higher education'). ⁽⁷⁾ Closeness to AfD is a dichotomous indicator: 1 designates 'close to AfD'; 0 designates 'close to other parties'. All individuals who report not feeling close to any party were coded as missing.

models test whether this effect is stronger for sub-groups in the population, but we find no such dynamic at play for either educational or income groups. In this, we find no support for Hypothesis 2 in the German context. A peculiar finding about the effect of education and income deserves mention: neither predictor has an effect on political interest, which contradicts most empirical analyses. We believe this is due to the predominantly cross-sectional variation in these variables in 2014–17. About 60% of the variation in education is between individuals, and this rises to 77% for income; both of these are neutralised by the unit-fixed effects in the specification. Other analyses (e.g. Denny and Doyle 2008) uncover the strong effects of education due to the use of cross-sectional samples, which make adding unit fixed-effects impossible.¹⁵

Conclusions

A unifying feature for all populist parties is their demand to protect the interests of ‘pure people’ from the actions of the ‘corrupt elite’. To pursue this goal, populists call for deeper political involvement of ‘average people’ (Osuna and Javier 2021). By increasing electoral participation of previously disengaged citizens, populism could act as a corrective and increase democratic inclusiveness (Mudde and Rovira Kaltwasser 2012).

However, evidence that supports such an effect is mixed at best (see Anduiza *et al.* 2019; Ardag *et al.* 2020; Houle and Kenny 2018; Huber and Ruth 2017; Huber and Schimpf 2017; Immerzeel and Pickup 2015; Leininger and Meijers 2021; Spruyt *et al.* 2016; Schwander *et al.* 2020). Neither campaign appeals nor political offerings have been found to influence people’s decision to vote – thus, these (mostly) null results are consistent with the scholarship on electoral turnout (Kittilson and Anderson 2010; Smets and van Ham 2013). Building on the distinction of situational and dispositional political interest by Prior (2018), we disentangle the behavioural and attitudinal effects of populism, and develop a *softer* version of the corrective argument: The growing intensity of populist appeals attract the attention of previously disengaged citizens; however, instead of turnout, it contributes to an increase in political interest (which is otherwise found to be remarkably stable – see Prior 2010).

Our results show that the increasing electoral support of populist parties (operationalised in multiple ways) can boost individuals’ interest in politics, as Hypothesis 1 posited. Our cross-country results are buttressed by similar results from Germany, where psychological closeness to the *Alternative für Deutschland* (AfD) is associated with increased political interest at the individual level. Thus, our research implies that populists – by addressing the problems of the ‘ordinary person’,

emphasising the need for the restoration of popular sovereignty, and restoring the voice of people who feel abandoned by political elites (Osuna and Javier 2021) – likely contribute to people's interest in politics.

In Hypothesis 2, we reflected the literature that concluded that populist parties enjoy increased support among disadvantaged sub-groups in the population (Werts *et al.* 2013; Golder 2016; March and Rommerskirchen 2015; Spruyt *et al.* 2016), and thus expected the stronger effect among them. However, this expectation did not find support in the empirical data – neither in the cross-country analysis, nor in the German one. Therefore, the growing strength of populist parties elevates political interest across a number of sub-groups in the citizenry at comparable rates.

These results generate a natural question: Should we be concerned about the effects we uncover, given their small magnitude? We think the answer is 'yes'. First, similar to political interest, additional attitudes can be shaped by populist appeals. Though the effects reported here are weak, when factoring in potentially similar influences on political efficacy (Marx and Nguyen 2018) or the evaluations of the quality of representation, populist appeals have a cumulatively more important influence.

Second, the absence of a discernible impact on turnout does not imply a corresponding absence of an effect on politics. Populist appeals could still spill over into an increased likelihood of attending a local protest, donating to political causes, or activity on social media (Anduiza *et al.* 2019; Pirro and Portos 2021). Coordinated efforts by other political forces to isolate populist parties might reduce the appeal of voting for them. Populists' own messaging might induce the belief in their sympathisers that 'normal' politics is 'rigged'. However, other politically relevant behaviours could likely still be shaped by such appeals.

We find cause for concern from a third perspective as well. The effect we identify on political interest, along with that on efficacy mentioned above, might not immediately boost populists' election prospects, but it could ensure staying in power. Such attitudes represent a source of continuous psychological engagement in politics that could serve to encourage the turnout decision. The 2020 US elections and the 2021 German elections are cases in point. In spite of a term marred by institutional decay, mismanagement, norm subversion, documented lies, and an election-year pandemic with high mortality, Donald Trump nevertheless managed to *increase* his share of the popular vote from 46.1% in 2016 to 46.9%, as can be seen in [Figure 2](#). In Germany, the AfD lost votes, from a high of 12.6% in 2017 to 10.3% in 2021; however, this result was obtained in an election extremely unfavourable to the AfD, with immigration absent from the public agenda, and with a competent government response to the

Covid-19 pandemic. If these two cases are accurate bellwethers, our results would predict that populist support will not wane considerably in the near future.

From the perspective of the *supply side of politics* we have only factored in people-centrism and anti-elitism as unifying features of populist parties' discourse. At the same time, such parties also adhere to a 'host' ideology that extends anti-elitism and people-centrism with additional issues (Mudde 2007) that may further interact with the potential of populist appeals to increase political interest. Research suggests that issues like migration and economic inequality could be relevant orientations among supporters of right-wing and left-wing populist parties (see, e.g. Akkerman *et al.* 2017; Rooduijn 2018). Future research should examine whether these issues can alter the link between populism and political interest identified in our work.

In conclusion, we believe our findings have concerning wider implications. Though populism may help to increase the number of politically-involved citizens, their message about the value of 'common sense' over expertise and political knowledge (Ylä-Anttila 2018) may further contribute to the proliferation of post-factual politics – a challenge currently faced by many democratic regimes.

Notes

1. See also Rooduijn (2018) for an argument that the populist voter does not exist.
2. While there are alternative databases (e.g., Global Party Survey (GPS), Populism and Political Parties Expert Survey (POPPA), PopuList, Chapel-Hill Expert Survey), none of them provides a historical assessment of populism that goes far enough back in time. However, we re-run our main models with the data from the PopuList 2.0 source in Section 7 of the Online Appendix. Though the direction of the effects remains unchanged, we lose statistical significance due to a smaller sample size.
3. The coding scheme employed for these variables matches that used in the True European Voter (TEV) project (Schmitt *et al.* 2021).
4. Due to space constraints, the Online Appendix provides more information on the coding of these controls in Section 2. Section 3 describes how the data harmonization was done. To explore potential heterogeneity in the effects, Section 8 shows self-reported satisfaction with democracy and placement on an ideological scale to investigate effect heterogeneity. No such effects were uncovered.
5. We planned to include an index of political polarization computed from the Manifesto Project (MARPOR) data, but this correlated at 0.825 with union density, and it was therefore excluded.
6. Figure 1 in Section 1 of the Online Appendix shows the distribution and bivariate associations between these three indicators. We add union density, the Gini index, and a secondary indicator of the strength of populist discourse.

7. In September 2005 the Jyllands-Posten newspaper published 12 cartoons of the Prophet Muhammad. This led to protests around the world. Domestically, the event triggered a public debate on self-censorship, free speech, and the role of religion in a secular liberal democracy.
8. We are grateful to one of the reviewers for suggesting the inclusion of these indicators.
9. This sensitivity check is restricted to our main indicator: the cumulative vote share obtained by populist parties.
10. In Section 5 of the Online Appendix we conduct a more standard replication, using three-level hierarchical generalized linear models. Table 6 shows that the direct effect on turnout on two of our three measures of populist party strength is insignificant. Yet again, only the systemic populism level is positively and significantly associated with turnout.
11. We are very grateful to Giorgio Malet for suggesting this strategy.
12. Though experts consider *Die Linke* to be populist (Loew and Faas 2019), this party is excluded from our analysis. The party has existed in its current form since 2007, so *GESIS Panel* respondents would have been exposed to its discourse at the beginning of the panel. This biases the estimate of the effect of party closeness on political interest.
13. We use the weighted fixed-effect estimator proposed by (Imai and Kim 2021). An alternative to this strategy is a dynamic cross-lagged panel model (Allison et al. 2017), where closeness at time $t-1$ predicts interest at time t , after controlling for interest at time $t-1$ and other covariates (a corresponding equation for interest at time $t-1$ is also specified). We opted not to use this approach due to the short nature of our panel (four waves at most), and the relative stability of political interest over this period. Including lagged versions of political interest in the specification would bias the estimates for the other predictors in the model (Achen 2000), including closeness to AfD. Furthermore, given our low number of waves, the lagged dependent variable exposes us to Nickell (1981) bias.
14. Limitations of space make it impossible to detail how each indicator was coded, but the interested reader can consult Section 4 in the Online Appendix.
15. A sensitivity check performed in Section 10 of the Online Appendix uncovers the effect of similar direction when using a lagged version of closeness to AfD, though it is only significant at the 90% level.

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