Promoting democracy online: Evidence from a cross-national experiment*

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Abstract

Democracy promotion programs for ordinary citizens are vital for strengthening democracies, but key questions remain: (1) Can they be effective online? (2) Which arguments work best? (3) Do context or individual traits matter? This study explores these issues through online experiments in 33 countries with 40,000+ participants. Respondents watched one of three videos on civic rights, separation of powers, or economic/public goods – or a placebo. Results showed increased democratic support, knowledge, and willingness to defend democracy against hypothetical anti-democratic candidates. Arguments based on intrinsic principles (civic rights, separation of powers) were most persuasive, with positive effects lasting up to two weeks. Surprisingly, political and economic contexts and individual differences didn't consistently influence outcomes. These findings highlight the universal benefits of online, educational democracy promotion programs across diverse populations.

Keywords: Democracy promotion; support for democracy; experiments; adult civic education; social media advertisements.

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1 Introduction

Political scientists have long suggested that the stability and effectiveness of democratic regimes depend in part on the existence of a democratic political culture (Almond and Verba, 1963; Claassen, 2020). As Linz and Stepan (1996) famously argued, consolidation occurs when democracy is "the only game in town". One common means to strengthen and build a supportive democratic culture and citizen empowerment – especially popular among international donors – is through democracy promotion or civic education programs [1] that advance political knowledge, engagement, and support for democratic norms and values among ordinary citizens.[2] Consistent with the dictum that "good citizens are made, not born", theorists have stressed the fundamental importance of schools in cultivating democratic citizenship. As such, civic education is a mainstay of the curriculum in primary and secondary schools in democracies around the world. However, civic education is not limited to the classroom or formal school systems, nor is it limited to programs targeted toward young people. The urgent need to develop a supportive political culture and resilience against democratic backsliding has led to the proliferation of donor-sponsored democracy promotion programs targeted toward adults, especially in emerging democracies where the vast majority of the population is past school age.

By now there has been a sizeable amount of research on the effects of adult-oriented democracy promotion programs in a variety of country contexts (see Finkel et al. (2022) for a recent systematic review and meta-analysis), yet it has proven difficult to arrive at generalizations about their impacts. This is due mainly to the fact that evaluations have proceeded on a largely *ad hoc* basis by examining single-shot, single-country programs which are unique in terms of the content and the context of the interventions and in the democratic outcomes that were assessed. There have been no studies, for example, comparing the effects of identical interventions across different country contexts to assess possible macro-level conditioning factors on democracy promotion programs.

[1] We use the terms democracy promotion and civic education interchangeably. Our study focuses on adult educational interventions that promote democracy distinct from school civic education. We refer to democracy promotion as international efforts targeting ordinary citizens to promote democratic institutions and culture to empower citizens and civil society which is distinct from democracy promotion programs conducted at the elite, party, or policy levels (Risse and Babayan, 2015; Donno, 2024).

[2] As defined by The Council of Europe Charter on Education for Democratic Citizenship and Human Rights Education (p.7): "Education for democratic citizenship means education, training, awareness raising, information, practices, and activities which aim, by equipping learners with knowledge, skills, and understanding and developing their attitudes and behavior, to empower them to exercise and defend their democratic rights and responsibilities in society, to value diversity, and to play an active part in democratic life, with a view to the promotion and protection of democracy and the rule of law." Access: `https://rm.coe.int/CoERMPublicCommonSearchServices/DisplayDCTMContent?documentId=09000016803034e3`.

Similarly, there have been only a few studies comparing the effects of these interventions with different content within the same country to assess the effectiveness of different arguments or frames on democratic outcomes (Finkel, Neundorf and Rascón Ramírez, 2024; Gessler and Kaftan, 2023; Voelkel et al., 2023). We are aware of only one study that has examined whether the effects of democracy promotion interventions persist beyond a relatively short time after the intervention (Ferrali, Grossman and Larreguy, 2023). As a result, we know little about how these programs - and especially online democracy promotion – impacts may generalize across contexts or individuals, nor about the factors related to context or content that may facilitate or impede the effectiveness of interventions in the short or longer term. Lastly, existing work investigates vastly different outcomes, which makes a systematic assessment of the effectiveness of these programs difficult, as democracy promotion might positively impact some but not other aspects of democratic citizenship.

In this study, we advance our knowledge of the impact of democracy promotion programs by implementing an original series of online, video-based interventions across 33 predominately English and Spanish-speaking countries at various levels of democratic and economic development. We test the impact of these educational interventions on over 40,000 individuals recruited via Facebook and Instagram ads who were randomly assigned to one of three treatment arms or a placebo video that focused on space exploration. Each of the three approximately 3 minutes treatment videos represented different arguments in favor of democratic political systems: their protection of individual **rights** and liberties, **institutional** constraints on executive power and promotion of the rule of law, and their provision of positive economic, health, and environmental **outcomes**. We worked with a civil society organization in Turkey to ensure that our videos were similar to the ones produced and used by civil society organizations in such contexts. We assess the videos' impacts on a series of democratic outcomes, ranging from the importance of living in a democracy (measured pre and post-treatment), rejection of authoritarian regime alternatives, willingness to defend democracy against anti-democratic candidates, and knowledge about the core principles of liberal democracy. In one country we extended the assessment of effects for several weeks after the initial exposure to democracy promotion messages.

The results suggest that (1) online democracy promotion interventions have positive effects across this wide range of outcomes, (2) treatments emphasizing the importance of civic rights and institutional constraints in democracies were substantially more effective than emphasizing democracy's superior economic and social performance, and (3) effects can still be detected after two weeks. Finally, (4) we found evidence that democracy promotion works more universally than previously assumed; holding

the educational content constant, consistently positive impacts were registered from the interventions across widely varying country contexts and among individuals with widely varying socio-demographic and political characteristics.

2 Democracy promotion and civic education: State of the field

Following the fall of the Berlin Wall in 1989, international donors moved to promote democracy in "third-wave" transition societies by implementing voter education programs designed to facilitate free and fair elections. Over the past three decades, the scope and intended outcomes of democracy promotion programs have expanded considerably. Programs now focus among other things, on topics such as the social and political rights of women, neighborhood problem-solving activities, programs designed to dampen support for election violence and vote-buying, the cultivation of political tolerance, trust in democratic political institutions, promotion of the peaceful resolution of political disputes, voter mobilization, and engagement with the political process. These programs take various forms, from town hall meetings and community workshops to artistic creations to programs delivered via mass media and digital communications technologies.

As adult civic education programs have proliferated, so too have efforts among academic researchers to evaluate the effectiveness of these programs in developing supportive democratic attitudes such as political efficacy, values such as political tolerance and trust, and political participation. The field emerged in the late 1990s with observational studies conducted by Bratton in Zambia (Bratton et al., 1999) and Finkel and colleagues in the Dominican Republic, Poland, and South Africa (Finkel, 2003), with subsequent work conducted in Kenya and several other sub-Saharan contexts (Finkel and Smith, 2011; Kuenzi, 2006; Moehler, 2008). Over the past decade, the field has expanded to include a substantial number of rigorous experimental evaluations conducted in a wider range of democratizing and more autocratic contexts.

The evidence from these studies on the effectiveness of civic education interventions has been decidedly mixed. Increased political participation and participatory inclinations have been found in numerous adult program evaluations ranging from town hall meetings on democracy and security issues in Liberia (Mvukiyehe and Samii, 2017) to a Pakistani women's voter mobilization program (Gine and Mansuri, 2018), to a program consisting of discussions between members of parliament and community members in rural areas in Cambodia (Hyde, Lamb and Samet, 2023). Similar findings were reported by Finkel, Neundorf and Rascón Ramírez (2024) in one of the only studies of online democracy promotion con-

ducted to date: exposure to video-based interventions in Tunisia extolling the benefits of democratic political systems or the costs of authoritarian-led systems to increases in intentions to register in the 2019 national elections and to engage in campaign-related political participation.

At the same time, Chong et al. (2015) find that voters exposed to corruption-oriented accountability information appeared to withdraw more generally from the electoral process, and Vicente (2014) found that exposure to an anti-vote buying campaign decreased turnout in Sierra Leone. A recent online study in Morocco by Ferrali, Grossman and Larreguy (2023) similarly finds no overall effect on youth turnout of several online video interventions providing registration information and information about the importance of elections and the policy platforms of the various political parties.

The experimental evidence regarding the impact of civic education interventions on democratic attitudes and values such as efficacy, tolerance, and trust is similarly mixed. Collier and Vicente (2014), for example, found positive effects of a neighborhood civic education campaign on attitudes against electoral violence in Nigeria, and Paluck and Green (2009) and Blattman, Hartman and Blair (2014) found increased support for the peaceful resolution of ethnic and land conflicts in Rwanda and Liberia. Vicente (2014) shows the positive effect of a door-to-door voter education campaign on the reduction of vote-buying in Sierra Leone, and Finkel, Neundorf and Rascón Ramírez (2024)'s online experiment in Tunisia showed increases in political efficacy and some indicators of support for democratic regimes. Other studies, however, report negative or null effects of civic education campaigns on political and institutional trust, e.g., Finkel and Lim (2020)'s evaluation of the VOICE decentralization program in the Democratic Republic of Congo, Hyde, Lamb and Samet (2023)'s evaluation of the Cambodia constituency engagement program, and Sexton et al. (2022)'s assessment of a participatory budgeting intervention in Peru that led to increases in support for certain kinds of civil unrest.

Despite the field's advances, answers to its fundamental questions – do civic education democracy promotion programs work, for whom and under what conditions – have remained elusive. The main limitation is related to the scope of these studies, which have been conducted in very diverse contexts and have tested the impact of vastly different interventions on vastly dissimilar outcomes. Disentangling the extent to which a study's effects (or non-effects) reflect general processes, or instead can be attributed to idiosyncratic aspects of the intervention, context or outcomes is of paramount importance yet currently impossible given the kinds of designs that have been implemented in the field. Here we address this shortcoming by implementing the same interventions in the same online form across multiple countries, holding constant the outcomes we measure. This design allows us to advance this literature in two key

areas. First, given the comparative focus, we can measure the effectiveness of the same democracy promotion interventions *across* and *within* different country contexts. Second, we vary the framing of the interventions to compare the impacts of different kinds of messages and civic education content on democratic attitudes and participatory orientations.

3 Hypotheses

In this section, we outline our pre-registered expectations regarding the impact of democracy promotion programs on democratic support and knowledge across various contexts, treatments, and individuals. We begin with the premise that exposure to such programs will positively influence attitudes toward democratic political systems and deepen understanding of liberal democratic principles. Exposure to educational content may enhance democratic knowledge and attitude in several ways, by changing people's previous preferences, by providing new information to form an original opinion, or by activating previously held opinions that were dormant prior to the intervention.[3] This leads to our primary hypothesis:

Hypotheses 1 (H1) – Main effects: Democracy promotion treatments will increase individuals' support for, knowledge of, and willingness to defend liberal democracy against anti-democratic candidates and regime alternatives.

We also posit that the effects of democracy promotion will vary based on the specific content of the arguments employed. For instance, Finkel, Neundorf and Rascón Ramírez (2024) highlighted the differential impact of "positive" frames emphasizing democratic benefits versus "negative" frames focusing on the costs of authoritarianism. In our study, we test frames linked to the specific benefits of liberal democracy compared to autocracy. Two treatments focus on *political rights* and liberties and *institutional checks* that constrain executive power and uphold the rule of law. A third treatment highlights democracy's *outputs*, such as superior economic, health, educational, and environmental outcomes.[4]

This focus on the diverse benefits of democracy revisits debates from the early 1990s regarding the

[3] We will not directly test which of these three different mechanisms holds in given contexts; rather we demonstrate the impact of the interventions on the outcomes works through different channels. Here we demonstrate a composite effect of these mechanisms and discuss this in more detail throughout the results and concluding sections.

[4] A series of "Case for Democracy" policy briefs, published by the V-Dem Institute, discusses the evidence for dividends of democracy on these and related output dimensions: `https://v-dem.net/pb.html`. Our output intervention incorporated arguments and evidence from these policy briefs.

sources of democratic support during the third wave of democratization. While economic performance was shown to matter — Cordero and Simón (2016) and Wang (2023) show its influence on satisfaction with and support for democracy — arguments centered on outputs may be less effective for promoting democracy. Contrary to the belief that support for new democracies in regions like Eastern Europe and Latin America primarily stemmed from improved economic performance, evidence suggests that rights and institutional advantages resonate more powerfully with individuals, sensitizing them to the intrinsic values of democracy and encouraging long-term attitudinal change (Mattes and Bratton, 2007). Viewing democracy through a procedural lens increases awareness of rights and freedoms, fostering the rejection of regimes that fail to guarantee these principles. Thus, we advance the following hypothesis:

Hypothesis 1a (H1a) – Different frames: H1 will be stronger if the content of the democracy promotion treatments focuses on the institutional and rights advantages of democracy compared to the superior economic, social or other outputs produced by democratic systems.

3.1 Context: Democratic and economic deficiency

We examine macro-level factors that may shape the impact of democracy promotion across national contexts, focusing on how democratic and economic development levels influence the effects of educational interventions. Our general expectation is that democracy promotion compensates for democratic deficits, being most effective in contexts with greater ”need.” This aligns with micro-level evidence of a ”compensation” effect, where civic education has stronger impacts on individuals with fewer political resources (Neundorf, Niemi and Smets, 2016; Lindgren, Oskarsson and Persson, 2019; Finkel, Neundorf and Rascón Ramírez, 2024; Campbell, 2019).

Thus, effects may be weaker in highly democratic countries due to limited ”need” and in extremely undemocratic contexts where citizens lack experience with or expectations of democracy. This leads to:

Hypothesis 2 (H2) – Democratic Levels: The impact of democracy promotion (H1) will be curvilinear, with the greatest effect in mid-range democratic contexts.

We further distinguish between democracy promotion’s effects on democratic *demand* (support for democracy and rejection of authoritarianism) and *supply* (perceptions of institutional performance) (Mattes and Bratton, 2007). Most countries claim to be democratic, e.g. in their constitutions (Márquez, 2016) or even their name (e.g. the *Democratic* Republic of the Congo), but that does not always mean

that they have the objective characteristics of liberal democracies (Dahl, 1972). Citizens' perception of the level of democracy in their countries is important to strengthen or weaken the legitimization of political regimes, especially of autocrats who claim to be democratic.[5] We expect that educational interventions that focus on the concept of (liberal) democracy as defined in the academic literature, will correct for the potential misperception of what democracy is, which will help people to better evaluate the performance of their political systems.[6] Consequently, we refine H2:

Hypothesis 2a (H2a) – Democratic Supply: Civic education democracy promotion treatments will decrease perceptions of democratic supply in less democratic contexts and increase them in more democratic contexts.

Economic development may also condition the impact of democracy promotion. In poorer contexts, democracy is less salient, immediate needs take precedence, and authoritarian regimes offering stability may be favored. Thus, we anticipate:

Hypothesis 3 (H3) – Economic Deficiency: Democracy promotion treatments (H1) will be less effective in contexts with lower levels of economic development and human security.

3.2 Individual-Level Heterogeneity

The macro-contextual patterns described above reflect "compensation effects," where democracy promotion has a greater impact in contexts with more pronounced democratic or economic "need." This pattern is supported by research on civic education, which shows stronger effects among individuals with fewer political and social resources, less political experience, and lower pre-existing democratic attitudes (Langton and Jennings, 1968; Neundorf, Niemi and Smets, 2016; Finkel and Smith, 2011; Campbell, 2019; Lindgren, Oskarsson and Persson, 2019; Finkel and Lim, 2020; Hoskins, Huang and Arensmeier, 2021). Extending this idea, we hypothesize:

[5] For example, Ozturk et al. (2023) show that becoming more negative about the state of democracy in Turkey, respondents are less likely to vote for the ruling AKP party, even among former AKP voters.

[6] For example, in the Democratic Republic of Congo, civic education increased the "demand" for democracy (support for rights, tolerance) but decreased "supply" perceptions due to dissatisfaction with poor institutional performance (Finkel and Lim, 2020).

Hypothesis 4 (H4) – Individual-Level Compensation Effects: Democracy promotion treatments (H1) will be more effective among younger individuals, those with lower levels of education and political interest, women, and individuals with lower pre-existing support for democracy.

4 Research design

We designed an online experiment, where we randomly exposed respondents to four short educational videos of which three presented various content promoting the virtues of democracy, while a fourth placebo video discussed the advantages of space exploration. The hypotheses, design, and planned data analyses of this study were registered on May 5, 2023,[7] before data collection commenced. The study received ethical approval (number: 400210195) from the University of XXX on May 17, 2022. In this section, we present more details on the research design.

4.1 Data collection

Data collection for this project took place between May 6 to October 5, 2023. Participants were recruited using paid advertisements on Facebook and Instagram, a recruitment strategy increasingly favored in comparative political science due to its ability to leverage the global accessibility of social media platforms and the adaptability of online survey tools (Boas, Christenson and Glick, 2020; Zhang et al., 2020). This method provided access to a broader and more diverse respondent pool, as well as greater geographical coverage, compared to traditional panel providers like YouGov or Qualtrics, especially in non-Western settings.[8] While random sampling from the general adult population would be ideal, it is impractical for a study of this scale. The use of social media recruitment significantly broadened the study's scope, allowing for consistent recruitment methods across countries and enabling systematic comparisons of factors such as political and economic development—comparisons that are difficult to achieve in single-country case studies (Finkel, Neundorf and Rascón Ramírez, 2024; Ferrali, Grossman and Larreguy, 2023). Additionally, this approach facilitated the inclusion of cases often overlooked in

[7] The registration can be accessed here: `https://osf.io/pmbe3?view_only=7ba8e9a115cc450c99dfd9feb8014888`. We registered four more hypotheses, which are not discussed here for space reasons. We discuss these additional hypotheses and present the pre-registered analyses in Appendix N.

[8] Professional online panels in non-Western countries often suffer from limited subscriber bases, leading to samples that overrepresent individuals with higher education. For instance, Boas, Christenson and Glick (2020) identifies this issue in India. Similarly, a recent study utilizing Qualtrics and Cint Marketplace panels in Turkey generated a sample where half the participants had university degrees, whereas only 26% of our Turkish sample held university education—closer to the national benchmark of 22% (Lutscher, Draege and Knutsen, 2024).

social science research, including autocracies and less-developed nations in the Global South.

10.7 Million people saw our recruitment advertisements on Facebook and Instagram. The advertisements encouraged people to participate in a survey, giving them the chance to win one $500 voucher for Amazon or a similar national online store in countries where Amazon is not available. Once social media users clicked on the advertisement, they were transferred to the consent form of the survey, which was programmed in Qualtrics. 62,518 people were assigned one of the treatments, and around 41,000 provided responses on all key variables. The costs for data collection included advertisements on social media as well as one $500 voucher.[9] We controlled the advertisement process through the Facebook Business Manager, which allowed us to manage the ads and to see progress in recruitment (e.g. how many people have seen our ad and have clicked on it). We used conversion as a campaign objective choice, which helped us to optimize Facebook's algorithm through people completing our surveys (Neundorf and Öztürk, 2023). Except for age (minimum of 18), we did not constrain the recruitment to any specific parameters, which implies that theoretically any adult using these platforms could see our ad and was hence invited to participate. Samples recruited through Facebook usually over-represent college-educated and male people. To create a more balanced sample, we used Facebook's targeting options, based on age, gender, and education.[10]

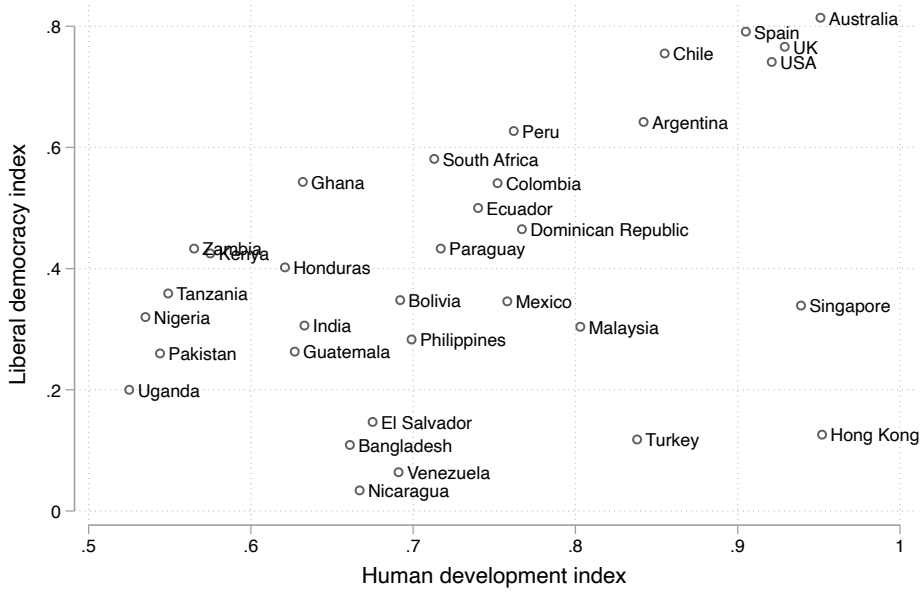
4.1.1 Case selection

One of the main objectives of this paper is whether the political and economic contexts in which respondents live moderate the effectiveness of democracy promotion interventions. We therefore selected countries for our study to represent variations in the level of liberal democracy, political trajectory, and economic development. There were two more practical constraints on case selection. First, since we used paid social media advertisements to recruit research participants, we selected countries where Facebook and Instagram are widely used, which we define as having at least 2 Million regular Face-

[9] The total cost for running advertisements on Meta to recruit our participants in all 33 countries is approximately $42,000 (excluding VAT).

[10] Facebook users represent the populations of their countries to a varying degree, ranging from 83% of the population that regularly uses Facebook in Singapore to 7% in Uganda. The average proportion of the national population active on Facebook in our sampled countries is 47.5%. In the Online Appendix G we provide a comparison of key demographics between our sample and the populations for each country. As the results show our samples are on average fairly representative in terms of age and gender, while over-representing highly educated people and those interested in politics. Our main models control for these demographics to estimate treatment effects more precisely.

Figure 1: Countries included in our sample



Data: V-Dem (v13) and United Nations 2021.

book users.[11] Second, to maximize (cultural) comparability across countries and minimize costs, we prioritized countries in which the general population understands English or Spanish, the two main languages used in our surveys. In addition, we included Turkey as an important example of a contemporary electoral autocracy.

Based on these considerations, we include 33 countries in our study.[12] Figure 1 plots our cases by their liberal democracy score (V-Dem) and their human development index (United Nations). The figure demonstrates that we have considerable variation in the two key contextual variables, studied here.

4.2 Experimental treatments

We used simple randomization to assign respondents to four experimental arms. Participants in each experimental condition were shown a different video.[13] The content of our educational videos focuses

[11] To determine the number of active Facebook users we used the following website: `https://worldpopulationreview.com/country-rankings/facebook-users-by-country`. The Facebook population in our sample ranges from 416 Million in India to 2.5 Million in Zambia and Nicaragua.

[12] List of countries: Argentina, Australia, Bangladesh, Bolivia, Chile, Colombia, Dominican Republic, Ecuador, El Salvador, Ghana, Guatemala, Honduras, Hong Kong, India, Kenya, Malaysia, Mexico, Nicaragua, Nigeria, Pakistan, Paraguay, Peru, Philippines, Singapore, South Africa, Spain, Tanzania, Turkey, Uganda, United Kingdom, the United States of America, Venezuela, and Zambia. Table A in the Online Appendix lists the number of valid observations in each of our cases and the dates of data collection.

[13] Appendix C provides external links to all videos as well as screenshots of an example video.

on different aspects of democracy: 1) checks and balances, e.g. **institutions** such as independent parliaments and courts, 2) the protection of civil and social **rights**, and 3) the economic and welfare **outputs** produced by democratic systems, e.g. economic growth, public goods. To contrast the effects of our political treatments, we also included a **placebo** video on the advantages of space exploration.[14] Each video represents a different experimental arm; they are not mixed. Our balance tests, presented in Online Appendix D, showed no indications of biases arising from randomization or survey attrition.

Each video is around 3 minutes long and consists of animated scenes, voice-overs (including subtitles), and the same background music across all treatment videos. We deliberately kept the content of the videos abstract and only referred to democracy and non-democracies in general terms to illustrate the different contents of each treatment arm. However, we included one country comparison between Lithuania and Belarus to illustrate the topic of each video. For example, in the output video, we present a graph of the diverging GDP per capita between the two countries after they gained independence in 1991, while in the rights video, we present a graph of the evolution of freedom of expression in the two countries in that period.

The videos present an idealized version of liberal democracy, which was also clarified at the beginning of the videos with the disclaimer that "democracy may have its problems, but it is still the best type of political regime". The tone of the videos was informative and upbeat (when discussing the advantages of democracy) through the use of music, the intonation of the narrator, and the use of positive words. The structure of the script was kept the same across all videos, starting with a general question about why democracy is better than non-democracies focusing on the themes of the treatment.[15]

We chose the medium of a short video for the treatment, as this is potentially highly scalable via social media as part of a democracy promotion campaign and the use of paid advertisement. Furthermore, we worked with a civil society organization in Turkey and a production company recommended by them to ensure that our videos had a similar production quality to the videos that are used by civil society organizations in such contexts. The main purpose of our experimental design is therefore to test the effectiveness of this type of low-cost educational intervention as used in the real world.

[14] We chose space exploration for the placebo treatment as it represents a universal topic, which is of interest to very different cultures and can easily be presented in non-political terms.

[15] We put considerable effort into ensuring equivalence between the four videos Dafoe, Zhang and Caughey (2018). As we demonstrate in Table A.2 in the Online Appendix, the videos do not differ in how informative respondents find them with an average score of 8.59 on a 0 to 10 scale.

4.3 Variables

4.3.1 Dependent variables

Democratic support is an abstract concept and requires several indicators to be measured. We follow Mattes and Bratton (2007) who distinguish outcomes relevant for democratic *demand*, i.e., support and defense of democratic systems and rejection of authoritarian alternatives, and outcomes related to democratic *supply*, or perceptions that the system is delivering an adequate degree of democracy in terms of institutional and regime performance. Based on these definitions, we measure our outcome of democratic support using four distinct dependent variables.[16] We, also included one outcome that measures respondents' knowledge of the core components of liberal democracy. The exact question wording and construction of dependent variables are available in the Online Appendix E. Appendix F further provides descriptive statistics of the outcomes.

First, we measure a **change in respondents' preference for democracy**, based on a question asked before and after they watched the treatment video to which they were randomly assigned. More precisely, people were asked to rate the importance of living in a democracy on a scale from 0 (not important at all) to 10 (very important).[17] In our sample, a general preference for democracy is widespread with an average value of 8.64 (pre-treatment). The change variable was constructed by subtracting the pre-treatment value from the post-treatment value. Hence, positive values indicate that respondents updated their preference to rate democracy as more important. On average, people positively update this preference by +0.30 points.

Second, we measure **rejection for authoritarian alternatives** using an index of three items that asked respondents how they would rate various types of political systems on a four-point scale: Having a strong leader, having only one party, or having the military rule the country.[18] Based on these items, we created an index using the average response, which ranges from 1 (support) to 4 (rejection). On average, respondents score 3.00 on this scale, indicating an overall negative leaning towards authoritarian alternatives.

[16] The order of these indicators was randomized to avoid that one dominating the results as it was asked straight after the treatments.

[17] This question is widely used in comparative public opinion surveys, such as the World Value Survey (WVS) and the European Social Survey (ESS).

[18] These items have been included in numerous comparative public opinion studies, such as the WVS, and are often used to measure authoritarian support. The three items are moderately correlated with Pearson's R ranging from 0.34 to 0.50.

Third, we create an index that measures respondents' **willingness to defend democracy** against three hypothetical anti-democratic candidates. The design of this index is inspired by recent studies that use conjoint experiments to demonstrate that people are willing to support (hypothetical) anti-democratic candidates to prioritize their party affiliations (Graham and Svolik, 2020; Svolik et al., 2023) or candidate's competence (Frederiksen, 2022). In these designs, the attributes of the paired candidates are randomized. In our case, we use static pairs, whereby respondents were asked which of two candidates they would vote for. We focus on three cross-pressured attributes: party support,[19] policy preference,[20] and competence. In all three cases, respondents' preferred outcome - party or policy they support and competence - is paired with an undemocratic trait of the candidate, which varies on three dimensions: media freedom, checks and balances, and electoral competition. The statements of these traits were taken from existing conjoint experiments (Frederiksen, 2022; Svolik et al., 2023).[21] The final index was constructed using the average score of the likelihood of voting for the pro-democratic candidate, which ranges from 0 (not likely at all) to 10 (Very likely), with an average of 4.86. We interpret this index as a willingness to defend democracy, as respondents are asked to act against their personal preferences. We would expect that through exposure to the treatment, respondents firstly learn about what is undemocratic and secondly the treatments activate the value of democracy to be important (over these other individual preferences).

We chose these three variables to capture a wide range of democratic commitments, going from a commitment to the idea of "democracy" to a deep commitment ingrained at the practical level. Our first outcome variable, the change in the respondents' preference for democracy, only measures support for democracy in a very abstract sense. This variable can not distinguish between a democratic commitment based on a genuine understanding of what democracy is versus an alleged democratic commitment that is not rooted in any form of understanding of the concept. Our second and third variables, on the other hand, do not use the word "democracy" or "authoritarianism". The second variable, authoritarian support, includes descriptions of certain institutional arrangements that violate democratic principles.

[19] We classified respondents' preferred party - which is used as the party of the undemocratic candidate - based on a pre-treatment feeling thermometer for the country's main political parties, which we defined as parties that received at least 5% in the last national election. The democratic candidate of the paired comparison was described to run for a randomly selected alternative party that was *not* the highest ranked party by the respondent. We decided to use a random selection of the non-preferred party to better mimic the conjoint design.

[20] We use gender equality as the policy area of interest, as this was considered a universal topic that applies to our various countries. To determine respondents' policy preferences, we included a pre-treatment question on the topic to classify people in favor and against gender equality. Respondents then saw a paired candidate profile of their policy stand with an undemocratic candidate.

[21] Appendix E provides more details on the construction of this index and the exact wording of all dimensions.

Compared to the first variable, this variable requires a more principled democratic position. Finally, our third outcome variable focuses on the practical level, measuring whether the respondent exerts any punishment of a politician taking certain undemocratic actions.

Fourth, the key to detecting democratic violations is citizens' **knowledge of the core elements of liberal democracy**. We, therefore, include an index measuring whether respondents correctly identify free and fair elections, judicial oversight of the government, and free speech as key characteristics for a country to be considered a democracy.[22] In our sample, respondents scored on average 8.21 on an index ranging from 0 to 10, indicating strong knowledge of liberal democracy.

Finally, to test H2a, we measure **democratic supply** using a single question, where respondents were asked the extent to which their country nowadays is a democracy. Responses ranged from 0 (not at all a democracy) to 10 (complete democracy), with an average of 5.72 in our sample.

All dependent variables were standardized by converting the individual responses into z-scores. The individual values reported by each individual are demeaned using the mean of the placebo for each country. This deviation is divided by the standard deviation of the placebo of the country. This conversion facilitates the comparison across models where dependent variables have been collected with different scales, and it also considers country-specific baseline values and their spread. To facilitate the interpretation of standardized coefficients, we presented all tables showing the treatment effects of the coefficients relative to the control mean of the dependent variable, using the non-standardized raw data. These are shown at the bottom of the tables.

4.3.2 Macro-level moderators

We consider the following country-level moderators. First, we rely on the country's 2022 **Liberal Democracy Index** (LDI) (`v2x_libdem`) using the Varieties of Democracy (V-Dem, v13) data Coppedge et al. (2023). The index ranges between 0 and 1, where higher values indicate higher levels of liberal democracy. The countries in our sample were divided into terciles representing the lowest, middle, and highest levels of democracy based on the entire set of V-Dem cases.[23] We choose the liberal democracy index instead of other measures of democracy, such as V-Dem's polyarchy score, as the index of liberal

[22] In Appendix K we further present results of three more characteristics related to populism, obeying leaders, and economic equality. Our treatments only impact respondents' evaluation that a country is considered a democracy if its "government obeys the will of the people above all else".

[23] In the Online Appendix M we further report the results based on the continuous measures of our macro indicators.

democracy more comprehensively captures the features of democracy that affect the lives of ordinary citizens, going beyond electoral institutions. Secondly, to test the impact of macro-level economic and social development, we use the **Human Development Index** (HDI) provided by the UN United Nations Development Programme (2022). Relying on measures of life expectancy at birth, expected years of schooling, and gross national income per capita, this index captures the level of human development in a country. We present our results using the tercile split of HDI of all available countries.

4.4 Modelling

The hypotheses are tested using simple linear regression with country-level clustered standard errors. In the Online Appendix H we further present results testing the main effects of our treatments using two alternative model specifications. First, we use country-fixed effects and second, we use a two-level multilevel (country-random effect) model, acknowledging individual and country constant and slope variation. As the results demonstrate, the findings presented below are robust to the modeling strategy.

5 Results

In this section, we discuss the results of our experiment. First, we present the main effects of the treatment on the various outcomes of democratic support and knowledge. Second, we present evidence of the longevity of the treatment effects. Third, we investigate the differences in the treatment effects across political and developmental contexts as well as varying individual characteristics.

5.1 Main treatment effects

Table 1 reports the main treatment effects of our pooled democracy promotion treatments – focusing on civil rights, democratic institutions, and the superior economic and welfare output of democracies – compared to the placebo group.[24] The coefficients represent the Average Treatment Effect on the Treated (ATT) on the main four standardized outcome variables. The impact of these treatments is significant and as expected based on our primary hypothesis that civic education treatments should increase individuals' support for democratic orientations and knowledge of liberal democracy.

First, being exposed to the treatments increases respondents' stated importance of living in a democ-

[24] Figure A.3 in the Online Appendix additionally reports the treatment effects descriptively by comparing proportions of pro-democratic responses between the placebo and treatment videos.

Table 1: Main results: Pooled treatment effects on democratic support

Dependent Variable (DV)	Change dem pref	Reject. of authoritarianism	Defending democracy	Knowledge lib. democracy
Treatment	0.265**	0.164**	0.045**	0.136**
	[0.019]	[0.014]	[0.013]	[0.014]
Covariates	YES	YES	YES	YES
Observations	41,395	41,001	42,003	39,518
R^2	0.322	0.085	0.009	0.153
Control Mean	0	0	0	0
% change using raw data	2.45	0.04	0.02	0.03
Control Mean (raw)	0.11	2.89	4.79	8.00
Min (raw)	-10	1	0	0
Max (raw)	10	4	10	10

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level controls: Level of liberal democracy and human development index. The full list of coefficients is presented in Appendix H, where we further present results without controls and two alternative model specifications. All outcomes are standardized.

racy.[25] The pre- to post-treatment change in this variable is 2.4 times higher in the treatment group compared to the placebo group when comparing the original scale. The effect size is about twice as large as the difference between below-secondary and university education. However, we should note that the control mean is quite close to zero (0.11) on a scale between -10 and +10, indicating only a minor update of a preference for democracy among the placebo group. Second, the treatments significantly increase the respondent's rejection of alternative authoritarian regimes.[26] Compared to the placebo group, support is about 4 percent lower in the treatment groups. The effect size is about a third of the difference between below-secondary and university education. Third, the impact of the treatments is weaker, but still significant, for the question of defending democracy – i.e., increasing the likelihood of voting for a hypothetical pro-democratic candidate who does not represent the respondent's preferred party or policy preferences against an anti-democratic candidate who does – by about 2 percent. Lastly, our treatments have a weaker, though significant, effect on the knowledge about the core elements of liberal democracy (an increase of 3 percent).

In a second step, we further disentangle the impact of the different content of our treatments, comparing the civic education videos that focus on intrinsic democratic principles (i.e., civil rights and

[25] In Appendix J we further present the treatment effect for each country separately. The treatments have a positive and significant impact in 28 out of 33 countries. Our study was initially powered and pre-registered considering the analysis of pooled data and not by country.

[26] In Appendix J we further present the treatment effect for each country separately. The treatments have a positive and significant effect in 26 out of 33 countries.

Table 2: Using different frames: Rights and institutions versus output treatments

Dependent Variable (DV)	Change dem pref	Reject. of authoritarianism	Defending democracy	Knowledge lib. democracy
Treatments (vs placebo)				
Rights/Institutions	0.165**	0.191**	0.067**	0.159**
	[0.015]	[0.015]	[0.013]	[0.014]
Output	0.162**	0.110**	-0.001	0.089**
	[0.015]	[0.016]	[0.014]	[0.018]
Covariates	YES	YES	YES	YES
Observations	41,395	41,001	42,003	39,518
R^2	0.30	0.09	0.01	0.15
Control Mean	0	0	0	0
Inst/Rights vs Output (p-value)	0.78	0.00	0.00	0.00

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level controls: Level of liberal democracy and human development index. P-values are calculated based on F-tests where we compare the coefficient of Rights and Institutions with the coefficient of Output. Table A.12 in the Appendix further reports the results for all three treatments separately. All outcomes are standardized.

democratic institutions) to the video that focuses on the economic and welfare performance and output of democracy. We hypothesized that the former would have a stronger effect than the latter (H1a). Table 2 reports the results of these analyses. The rights-institution treatments indeed have more substantial effects than the output treatment on three of the four outcomes, as indicated by the p-value of a joint test of the coefficients (last row of Table 2). Only the change in democratic preferences is equally affected by the different frames. For instance, the impact of rights-institution treatments is approximately 70% larger than the effect of output-content video on the rejection of authoritarianism ((0.191-(-.110))/0.110).[27]

These results deserve further discussion. Our outcome variables represent varying degrees of democratic commitment and understanding, ranging from a positive evaluation of the idea of "democracy" (expressed in a preference to live in a democracy) to the ability to recognize and willingness to punish an undemocratic candidate. Our results in this section demonstrate that the performance or outcome-based defense of democracy is effective at increasing public support for the abstract idea of "democracy" but does not help in developing deeper democratic commitments to the same extent. In other words, the output-based treatments create "democrats in name only." Our procedural treatments, emphasizing the intrinsic qualities and benefits of democracy, promote more extensive and meaningful democratic commitment and knowledge.

It may be argued that, because the outcomes are attitudinal, they may not be consequential when it

[27] Additionally, we present the ATT for each treatment arm separately in Appendix I.

comes to taking concrete actions to stand up for democracy. In Turkey, we therefore added a behavioral outcome that asked respondents whether they would donate money to a non-partisan civil society organization (e.g. Turkish Democracy Foundation) working to promote Turkish democracy if they won 10,000 TL (350 USD) from a lottery. We do not report this outcome in the main paper as we only included it in one country, but results reported in Appendix K.1 confirm that our treatments successfully increased respondents' willingness to donate on average by 104 TL (3.60 USD) to a democratic cause. In Table A.13 in the Appendix, we further present results that our treatments positively impact intended turnout in the next election as well as intentions to engage in non-electoral political participation across all our 33 cases.

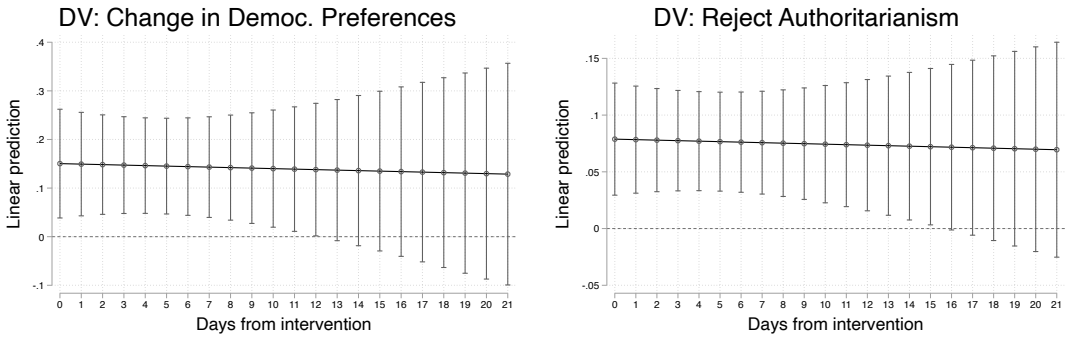
5.1.1 Longevity of effects

One potential threat to the validity of our results involves the demand effects of the treatments, where participants alter their responses or behavior based on what they perceive researchers expect or desire. Because respondents are exposed to the videos and asked outcome questions immediately afterward, they may simply report answers they believe align with the researchers' expectations. Another important concern is the longevity of the treatment effects. Do the effects persist once respondents return to their daily lives? Creating lasting impacts is a primary goal of most educational interventions, yet practical challenges often hinder efforts to assess the durability of these effects. One way to alleviate these concerns would be to show that the effects last beyond the short time frame from exposure to the initial post-treatment measurement. To address these concerns, we implemented an extensive recontact design in Turkey to examine whether the attitudinal changes induced by our treatments persisted weeks after the initial exposure.[28]

We recruited 14,712 respondents in Turkey for the recontact design and exposed them to our treatments. During the three weeks following the completion of the original survey and exposure to the civic education treatment, participants received three SMS messages and an e-mail inviting them to complete a follow-up survey, which measured only two outcome variables: individual-level change in democratic support and support for authoritarianism. Overall, we received 10,082 responses to these attempts from 6,905 respondents. Most of the responses were recorded during the four days after the exposure to the treatment. We provide more detailed information on the data collection process and the follow-up data,

[28] Unfortunately, due to practical and financial constraints it was not possible to recontact respondents in other countries.

Figure 2: Marginal effects of the civic education treatment on democratic support by days after the treatment



Note: Results are based on linear regression with pre-treatment individual-level controls. Results are clustered at the individual level.

including a detailed analysis of attrition, in Appendix L.

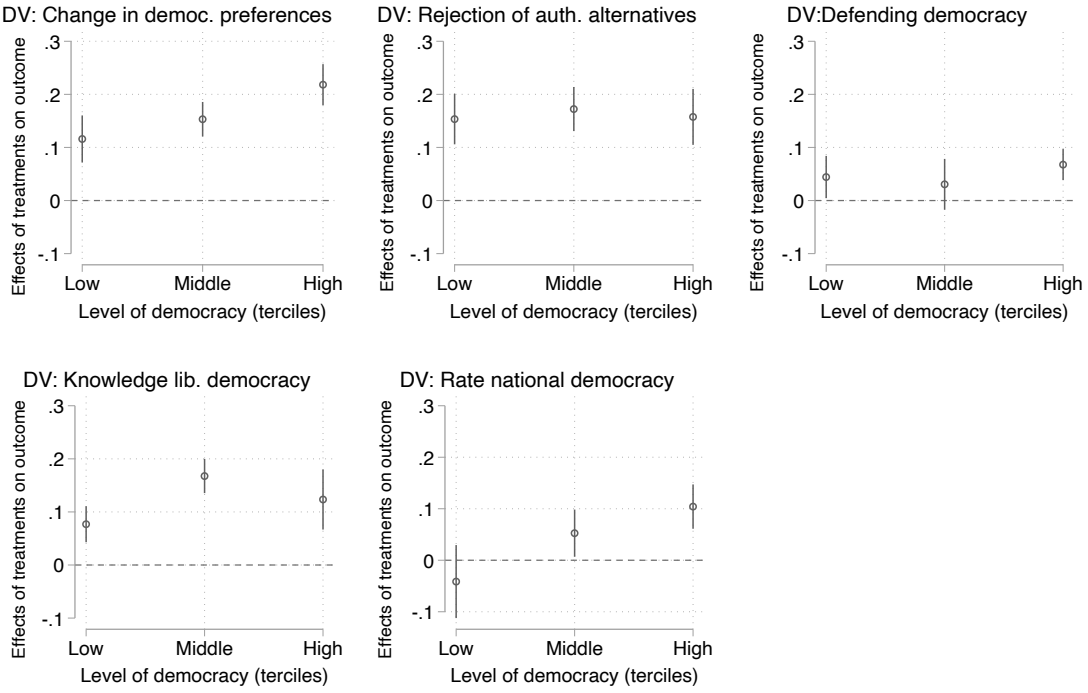
When analyzing the results, we first conducted a regression analysis to replicate the analysis of the main treatment effects over the entire sample of follow-up responses, clustering the standard errors at the individual level. We found that exposure to the treatment had an average effect of 0.144 (p=0.006) on the individual-level change in democratic support and 0.076 (p=0.001) on the rejection of authoritarianism. Results are available in Table A.16 in the Online Appendix.

After that, we conducted interaction analyses, exploring how the effect of the treatment changed over the three weeks after the exposure to the treatment. According to Figure 2, where we present the results of these interactions, the effect of the treatments on both outcomes lasted at least two weeks. Results do not indicate substantial attenuation of the effects during or in the immediate aftermath of this period. The treatment effects became statistically insignificant after the two weeks primarily due to insufficient statistical power in those days; only 20% of our observations were recorded 14 or more days after the exposure to the treatment. Nevertheless, the magnitude of these effects remains practically unchanged. These results provide confidence that short, educational videos promoting democracy impact citizens' views of democracy and its alternatives beyond the relatively artificial environment of an online survey.

5.2 Contextual differences in treatment effects

Next, we investigate the contextual variation of our 33 cases. We expected that democracy promotion interventions can compensate for democratic and economic deficits, that is, will be more effective in contexts with greater "need" (H2-H3). Figure 3 presents the results of this exploratory hypothesis, fo-

Figure 3: Marginal effects (and their 95% confidence intervals) of treatments by level of liberal democracy



Note: Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level control: Human development index.

cusing on the political context. Here we present the marginal effects (and their 95% confidence intervals) of the treatment effects by different levels of liberal democracy as observed in our 33 countries. The models estimated to produce these graphs are similar to the ones presented in Table 1 but additionally include an interaction term between the pooled treatments and terciles of V-Dem's liberal democracy index.

We expected that the treatment effects would be strongest in the mid-range of democratic development, where the need for civic education is the largest as well as most realistic in leading to political change (unlike in the most repressive authoritarian contexts). This seems to be the case only for knowledge of liberal democracy. Here we find a significantly higher effect between countries with a middle level of democracy compared to autocracies in our sample (low level). Figure 3 further reveals the contextual differences in how our civic education videos affect the change in democratic preferences pre-post treatment. Contrary to H2, the treatments seem to be weaker at low levels of liberal democracy and then increase in more democratic countries. This finding suggests that people living in more liberal

democracies are reminded of how important it is for them to live in a democracy.

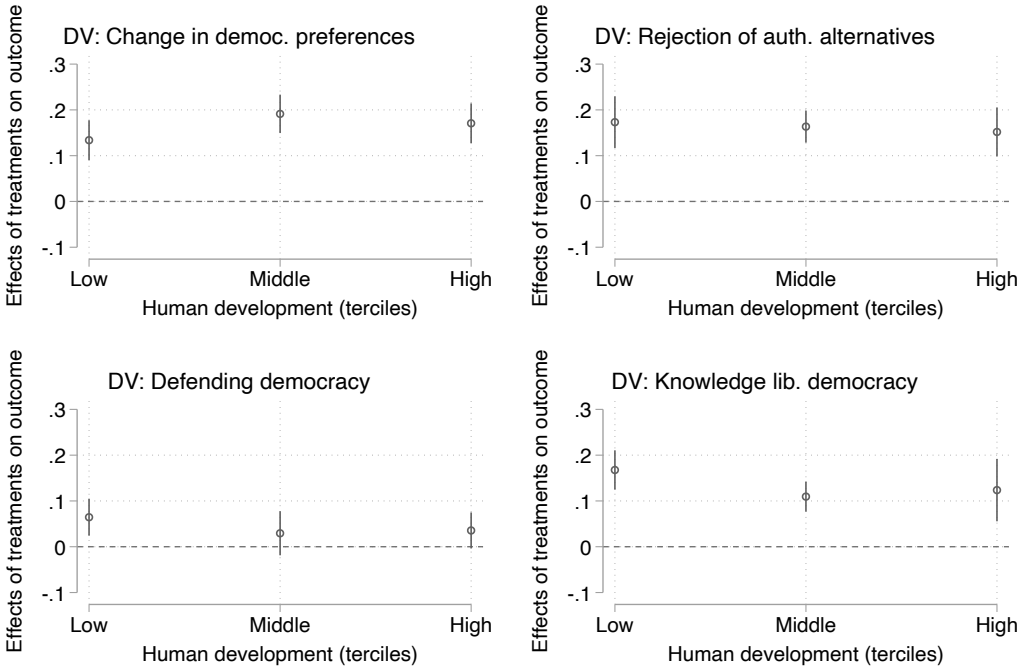
The level of democracy does not condition the strength of the treatment on the rejection of authoritarianism. Regarding the outcome of defending democracy, the results presented in Figure 3 show the opposite of what we expected. The impact of the treatments is weakly significant at low and higher levels of liberal democracy, while the treatments did not increase the support for pro-democratic candidates in countries with mid-level democratic development. To conclude, we find weak evidence for H2.

Next, we explore whether our civic education treatments affected the perception of how democratic one's country is. We expected that the treatment would decrease perceptions of democratic quality in less democratic contexts and increase perceptions of democratic quality in more democratic contexts (H2a). This hypothesis is tested in the bottom-right panel of Figure 3, examining the respondents' rating of their country's democracy. The results show that in more democratic contexts, the treatment increases respondents' perceptions of how democratic the country is while decreasing those perceptions in more authoritarian contexts. Figure A.11 in the Online Appendix confirms this pattern in a model interacting the treatments with a continuous indicator for liberal democracy. This provide positive support for H2a.

We now turn from the political to the economic and developmental context of our cases. We expect our treatments to be less effective in contexts with lower levels of economic and social development, which we measure using the UN's Human Development Index (HDI) (H3). We test this relationship by interacting our pooled treatments with a country's Human Development Index (HDI). Figure 4 plots the marginal effects of our treatments by HDI terciles and their 95% confidence intervals. We do not find that the HDI conditions the impact of civic education treatments on general support for democracy, the rejection of authoritarianism, or democratic knowledge. Furthermore, the treatments seem to weakly increase a respondent's willingness to defend democracy at low-level HDI, while the treatments are insignificant at middle and high levels of HDI, which is contradictory to what we expected based on our deficit theory. However, we also note that confidence intervals in all cases overlap, showing no significant contextual difference. We therefore conclude that there is no support for H3.

In Appendix N we further investigate two additional contextual effects. First, we do not find any systematic contextual differences in our treatments when conditioning these on the trajectory of liberal democracy (autocratizing, democratizing, and stable) of a country. Second, we explore whether pooling our treatments (as is done in the analyses presented here), might mask important contextual effects, whereby specific treatments (e.g. focusing on the performance outputs of democracy) work better in less developed countries. As we present in Appendix N.2 it does not seem to be the case that matching

Figure 4: Marginal effects (and their 95% confidence intervals) of treatments by level of human development



Note: Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level control: Liberal democracy index and human development index.

the content of civic education interventions to specific country contexts changes the magnitude of their impact.

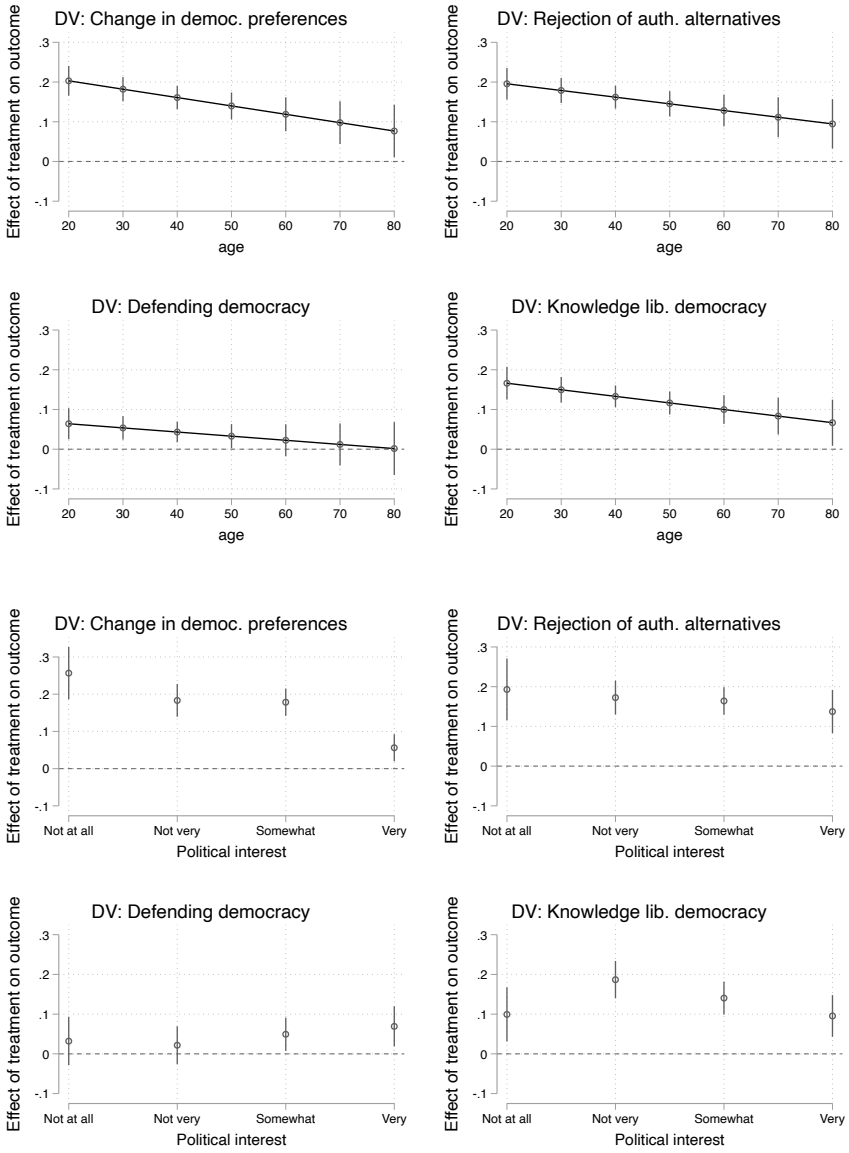
5.3 Individual differences in treatment effects

In this section, we move to the individual level and investigate whether the deficiency argument for the need for educational democracy promotion is stronger for those who need it more (H4). We present results of the individual heterogeneity in the effectiveness of the treatments. Figure 5 plots the marginal treatment effects (and their 95% confidence intervals) for different age groups and levels of political interest, measured on a standard four-point scale.[29] Following the "compensation" notion discussed above, we expected larger effects on respondents with fewer political and economic resources, e.g. younger or with lower levels of pre-existing commitment to democracy or political interest.

The results demonstrate that the treatments do have a stronger impact on the young across all the

[29] Although this analysis was pre-registered for age groups but not for political interest, we have incorporated it to provide a more comprehensive evaluation of the impacts of our interventions.

Figure 5: Marginal effects (and 95% c.i.) of treatments by age and political interest



different outcomes, and the right panel in Figure 5 further shows that the treatments have significantly stronger effects on the change in democratic preferences among the least politically interested. On the other hand, the treatments only affected the outcome of defending democracy among the most politically interested. We do not find any systematic conditional effects for gender, education, residence, or pre-existing democratic preferences, giving mixed evidence for H4. Results for these analyses are reported in Appendix O.

6 Discussion and conclusion

The results of this study offer hope for promoting democratic change and preventing democratic backsliding through online civic education democracy promotion interventions. Using a cross-national experiment, we demonstrated that our original treatments increased citizens' preference for democracy, their rejection of authoritarian alternatives, their willingness to vote for pro-democratic candidates even when this conflicted with personal partisan or policy preferences, and their knowledge of liberal democracy. These outcomes are significant for the resilience of democracy, as "for democracy to endure, their leaders and *citizens* must internalize the spirit of democracy" (Diamond, 2008, 294).

An important question is whether respondents underwent a genuine learning process. It might be argued that respondents simply repeated what they saw in the videos when answering post-treatment questions, aiming to provide "the right answer." However, based on the results, we are confident that respondents engaged in a genuine learning process. Most notably, the recontacting study in Turkey revealed detectable effects beyond the immediate measurement of outcomes after exposure to the treatments. We provide evidence that the impact of our interventions lasted for at least two weeks.

Moreover, respondents not only developed a more positive disposition toward "democracy" after watching our videos but also applied the information to new contexts. For instance, as shown in Table 1, respondents demonstrated a willingness to defend democracy by transferring pro-democratic attitudinal changes to an electoral scenario requiring them to choose between two candidates. Similarly, as shown in Figure 3, respondents were better able to evaluate the political regime in their countries after watching our videos, even though the videos did not reference any of the countries in the sample. Respondents in democratic countries positively updated their evaluations of their political regimes, while those in authoritarian countries became more critical (See Figure A.11 in the Appendix).

Surprisingly, our findings indicate that the impact of democracy promotion is consistent across a wide range of political and economic contexts, as represented by the 33 countries included in this study. This is a novel finding, as no prior study has implemented and compared civic education interventions across multiple countries using the same approach. We addressed this gap by using an online format, which allowed us to expose individuals from diverse countries to the same civic education interventions promoting the benefits of democracy. We found consistent effects across various outcomes. With some notable exceptions, these impacts were relatively universal and independent of both macro-contextual factors and individual-level characteristics.

One difference in effects that did emerge, however, concerned the impact of the different democracy frames presented in the three treatment arms. We found a consistent pattern whereby treatments emphasizing democracy's intrinsic qualities – the provision of individual rights, civil liberties, and institutional constraints on executive authority – resonate more strongly than an emphasis on the superior*performance* or *outputs* of democracy. This echoes previous work on the development of democratic support, e.g. Mattes and Bratton (2007, 202), who argue that people evaluate democracy "as much in procedural as substantive terms: *how* democracy works is just as or more important than *what* it produces. Democracy promotion efforts in the future would do well by crafting interventions that emphasize this point to maximize their impact.

The study highlights several directions for future research. First, it remains unclear how the impact of online interventions compares to traditional in-person formats. Online treatments may have weaker effects than offline programs but offer the advantage of reaching far larger audiences, potentially with greater frequency and repeat exposure. For instance, if our videos were distributed as part of a paid social media advertising campaign by an NGO or international donor, they could reach millions worldwide. The online format also facilitates access to disadvantaged individuals, who are often the targets of citizen-focused democracy promotion interventions (Gine and Mansuri, 2018; Finkel, Neundorf and Rascón Ramírez, 2024). Given the individual-level compensation effects observed in our results, this type of targeted outreach could foster democratic commitments among those most in need (see Appendix O).

Additionally, implementing online democracy promotion programs allows researchers and stakeholders to tailor messages to different audiences. We still know relatively little about how various types of civic education content work and what strategies are most effective in promoting democracy. While this study compared three content types focused on rights, institutional constraints, and economic and social outcomes, there is significant potential for further research into how best to frame messages promoting democratic regimes and processes. Although a strength of our design was the standardized content of the videos, which enabled testing generalizability across diverse contexts, impacts could potentially be enhanced by emphasizing specific aspects of democracy or addressing democratic deficiencies unique to particular countries or moments in time.

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Supplementary Material

Promoting democracy online: Evidence from a cross-national experiment

Contents

A. Appendix: List of countries and number of observations

Table A.1: List of countries, date of data collection, and valid number of observations

Countries	Start date	End date	N of obs.
Argentina	05/19/23	06/24/23	1,402
Australia	05/06/23	06/29/23	753
Bangladesh	05/07/23	06/17/23	1,357
Bolivia	05/19/23	06/25/23	1,096
Chile	05/19/23	06/17/23	1,476
Colombia	05/19/23	06/23/23	1,176
Dominican Republic	05/19/23	06/12/23	1,125
Ecuador	05/19/23	06/07/23	1,124
El Salvador	05/19/23	06/10/23	1,099
Ghana	05/07/23	06/17/23	1,318
Guatemala	05/19/23	06/05/23	1,056
Honduras	05/19/23	06/21/23	1,082
Hong Kong	05/06/23	06/29/23	764
India	05/06/23	06/27/23	1,285
Kenya	05/08/23	06/16/23	1,619
Malaysia	05/07/23	06/28/23	998
Mexico	05/19/23	06/21/23	1,179
Nicaragua	05/19/23	06/14/23	1,186
Nigeria	05/06/23	06/18/23	1,686
Pakistan	05/06/23	06/18/23	2,076
Paraguay	05/19/23	06/01/23	1,105
Peru	05/19/23	06/08/23	1,093
Philippines	05/18/23	06/29/23	1,343
Singapore	05/07/23	06/29/23	528
South Africa	05/07/23	06/16/23	1,897
Spain	05/30/23	06/27/23	1,063
Tanzania	05/09/23	06/10/23	1,210
Turkey	09/27/23	10/05/23	1,634
Uganda	05/10/23	06/11/23	1,650
Venezuela	05/19/23	06/18/23	1,542
Zambia	05/09/23	06/09/23	1,389
United Kingdom	05/06/23	06/28/23	1,081
United States	05/06/23	06/27/23	1,003

Note: The number of observations are based on the model predicting preference for democracy, presented in Table 1.

List of events that happened in each country during the data collection period: `https://www.dropbox.com/scl/fi/ihvzqxm25lzgyvq6apocn/Country-events_Civics-Study.pdf?rlkey=oknj9ubw3xm6f6rzqhijgl4bt&dl=0`

B. Appendix: Details Facebook/Instagram recruitment of research participants

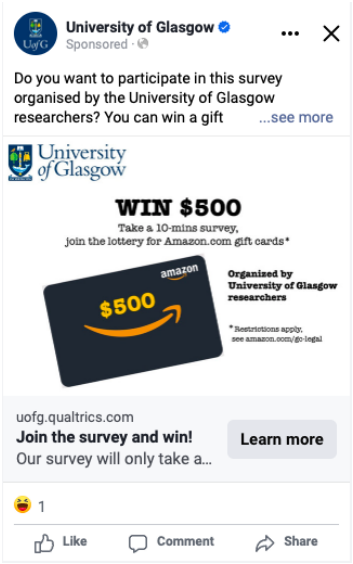
Respondents were recruited via advertisements on Facebook and Instagram. Once social media users saw and advertisement, they were invited to take part in a survey to enter a prize draw of a $500 voucher for an online store, such as Amazon, or an alternative national online store in countries, where Amazon is not available. Figure A.1 shows an example advertisement.

We controlled the advertisement process through the Facebook Business Manager, which allowed us to manage the ads and to see progress in recruitment (e.g. how many people have seen our ad and have clicked on it). We used conversion as a campaign objective choice, which helped us to optimize Facebook's algorithm through people completing our surveys **?**. Except for age (minimum of 18), we did not constrain the recruitment to any specific parameters, which implies that theoretically any adult using these platforms could see our ad and was hence invited to participate. Samples recruited through Facebook usually over-represent college-educated and male people. To create a more balanced sample, we used Facebook's targeting options, based on age, gender, and education.

Once respondents clicked on the link on the advertisement, they were taken to a Qualtrics landing page, where they were first shown the Participant Information Sheet (PIS). The PIS included details about the research, the intended purpose (two research papers), the duration of the survey (10-15min), funding details of the project, and contact details for the research team and ethics board. We also provided participants with information on how their data will be stored and that participation is voluntary and can be ended at any time. They also received details about the prize draw used for compensation. Only after people read these details and consented to their participation, were they taken to the survey.

The costs for data collection included advertisements on social media as well as a chance to win a $500 voucher for Amazon or a similar national online store in countries where Amazon is not available. The total cost for running advertisements on Meta to recruit our participants in all 33 countries is approximately $42,000 (excluding VAT).

Figure A.1: Facebook/Instagram Advertisment Example (USA)



C. Appendix: Details treatment videos

C.1. Links to treatment videos

Treatment videos have been provided. File names are as follows. Alternatively, the videos are also available on YouTube.

- Institutions treatment: "Movie S1 - Institution treatment.mov"; `https://youtu.be/8TU2nXdqk94`
- Output treatment: "Movie S2 - Output treatment.mov"; `https://youtu.be/SKFG9iweUpQ`
- Rights treatment: "Movie S3 - Rights treatment.mov"; `https://youtu.be/YBePPaG0Bg8`
- Placebo: "Movie S4 - Placebo treatment.mov"; `https://youtu.be/4kJVYefF7CY`

Figure A.2: Stills of treatment videos on civil rights

C.2. Manipulation and attention checks of treatment videos

Table A.2: Assessment how informative the video content is

Treatment	N of obs	Average (0-10)	Std. dev.
Placebo	10,396	8.42	1.82
Output	10,261	8.62	1.81
Institutions	10,323	8.68	1.70
Rights	10,355	8.65	1.78
Total	41335	8.59	1.78

Table A.3: Attention check: Identifying still picture of video

	Passed attention check	
Treatment	No	Yes
Placebo	11.3	88.7
Output	7.2	92.8
Institutions	20.3	79.7
Rights	24.1	75.9
Total	15.7	84.3

D. Appendix: Balance tests

Table A.4: Balance test: Treatments versus Placebo, including country fixed effects

	Treatment group		
	Output	Institutions	Rights
Age	0.001	0.002	0.001
	[0.001]	[0.001]	[0.001]
Female	-0.017	-0.001	-0.042
	[0.029]	[0.029]	[0.029]
Secondary educ.	-0.024	0.068	0.034
	[0.060]	[0.061]	[0.060]
Degree	0.034	0.148*	0.084
	[0.063]	[0.064]	[0.063]
Urban	-0.033	-0.034	-0.023
	[0.029]	[0.029]	[0.029]
Proficcient language	0.065	0.009	0.022
	[0.036]	[0.035]	[0.036]
Pre-treat democ pref.	-0.004	0.004	0.008
	[0.007]	[0.007]	[0.007]
No very interested	0.074	0.061	0.028
	[0.047]	[0.047]	[0.047]
Somewhat interested	0.052	0.055	-0.008
	[0.045]	[0.045]	[0.045]
Very interested	0.075	0.083	0.02
	[0.050]	[0.050]	[0.050]
Turnout	0.009	-0.024	-0.038
	[0.034]	[0.033]	[0.033]
Country FE	YES	YES	YES
Constant	0.008	-0.126	0.039
	[0.123]	[0.124]	[0.123]
Observations	41,395	41,395	41,395

Significance levels: ** p$<$ 0.01, * p$<$ 0.05. Results are based on multi-nominal logistic regression with the placebo group as the baseline. The table reports the regression coefficient and their standard errors.

Table A.5: Balance test: Treatments versus Placebo, excluding country fixed effects

	Treatment group		
	Output	Institutions	Rights
Age	0.002*	0.003**	0.002*
	[0.001]	[0.001]	[0.001]
Female	0.007	0.014	-0.015
	[0.028]	[0.028]	[0.028]
Secondary educ.	-0.024	0.057	0.015
	[0.057]	[0.058]	[0.058]
Degree	0.029	0.128*	0.034
	[0.058]	[0.059]	[0.058]
Urban	-0.02	-0.013	-0.004
	[0.028]	[0.028]	[0.028]
Proficcient language	0.055	-0.013	0.017
	[0.032]	[0.032]	[0.032]
Pre-treat democ pref.	-0.011	-0.001	0.002
	[0.007]	[0.007]	[0.007]
No very interested	0.074	0.057	0.026
	[0.047]	[0.047]	[0.046]
Somewhat interested	0.044	0.036	-0.019
	[0.044]	[0.044]	[0.044]
Very interested	0.069	0.059	0.01
	[0.050]	[0.049]	[0.049]
Turnout	0.002	-0.03	-0.027
	[0.032]	[0.032]	[0.032]
Country FE	NO	NO	NO
Constant	-0.09	-0.215*	-0.101
	[0.090]	[0.091]	[0.090]
Observations	41,395	41,395	41,395

Significance levels: ** p$<$ 0.01, * p$<$ 0.05. Results are based on multi-nominal logistic regression with the placebo group as the baseline. The table reports the regression coefficient and their standard errors.

E. Appendix: Question-wording and construction of dependent variables

Change in preference for democracy (asked pre- and post-treatment): "How important is it for you to live in a country that is governed democratically? 0) not important at all; 10) very important."

The *change* variable was constructed as follows: DemPref_post - DemPref_pre.

Rejection of authoritarian alternatives: "Below we are describing various types of political systems and ask what you think about each as a way of governing this country. Please tell us what you think about them. [order of sub-questions will be randomized] 1) very good; 2) fairly good; 3) fairly bad; 4) very bad.

- Strong leader: Having a strong leader who does not have to bother with parliament and elections
- One party: Having only one political party is allowed to stand for election and hold office.
- Military: Having the army rule the country."

The final index was created using the rowmean of the three items, which takes into account item non-response. The scale was reversed to go from least to most democratic.

Willingness to defend democracy: "Next, we would like to show you different hypothetical candidates running in a national election. We will then ask you which of these candidates you would vote for. Please compare Candidate A and B carefully. How likely are you to vote for either of these candidates? 0 = Certainly vote for A; 10 = Certainly vote for B."

Respondents saw two candidates based on the following cross-pressured dimensions:

1. Party preference
    - *Preferred:* "Candidate X runs for (PARTY SUPPORTED BY THE RESPONDENT)."
    - *Not preferred:* "Candidate X runs for (PARTY NOT SUPPORTED BY THE RESPONDENT)."
2. Candidate competence
    - *Competent* "As a health minister, Candidate X was praised for his work, improving health services across the country."
    - *Incompetent*: "As finance minister, Candidate X was criticized for introducing policies that produced high levels of inflation and unemployment."
3. Policy preference[1]
    - *Against gender equality:* "Candidate X is advocating the introduction of stricter laws to regulate jobs in times of crisis to favor men."

[1] The statements regarding this policy preference are paired with a pro-democratic or anti-democratic statement depending on whether the respondent is in favor of this policy option, which was measured using pre-treatment support to the following statement ""When jobs are scarce, men should have more right to a job than women." All non-democratic candidate traits were paired with the policy option less preferred by the respondent.

- *In favor of gender equality:* "Candidate X is in favor of the introduction of a new law to make pay more equal between men and women."

Each of the dimensions above was paired with one of the following three democratic dimensions.[2] In all cases, the description of the candidate paired the preferred party, policy, and competence option with the undemocratic option listed here:

1. Media freedom
    - *Democratic*: Candidate X also said it is unacceptable to harass journalists even though they do not reveal sources.
    - *Undemocratic*: Candidate X also supported a proposal for the government to monitor politically critical posts on social media.
2. Checks and balances
    - *Democratic*: Candidate B also said court rulings by judges appointed by opposing parties should be respected.
    - *Undemocratic*: Candidate X also said the government should discipline judges who publicly criticize it.
3. Electoral competition
    - *Democratic*: Candidate X also supported a proposal to maintain all parties' right to campaign wherever they choose.
    - *Undemocratic*: Candidate X also encouraged their supporters to violently disrupt campaign rallies of their political opponents.

Knowledge liberal democracy: "How important are the following for a country to be considered a democracy? 0= Not important at all; 10 = very important."

- Checks and balances: Courts are able to stop the government from acting beyond its authority.
- Elections: There are regular elections with several parties competing against each other.
- Liberties: There is complete freedom for everyone to peacefully criticize the government

The final index was created using the rowmean of the three items, which takes into account item non-response.

Democratic supply - Country's democracy rating: "In your opinion, to what extent is (COUNTRY) nowadays a democracy? 0= Not at all a democracy; 10 = complete democracy."

[2] We randomized which democratic dimension was paired with the cross-pressured dimensions. This we avoided a potential interaction between any of the dimensions. All respondents saw all six dimensions. Just the pairing varied.

F. Appendix: Descriptives

Table A.6: Descriptive statistics of individual-level data

Variable	Obs	Mean	Std. dev.	Min	Max
DEPENDENT VARIABLES (DV)					
Change in democ. Pref.	41,395	0.30	1.73	-10	10
Rejection of authoritarian alternatives	40,087	3.00	0.84	1	4
Defending democracy	40,722	4.86	1.91	0	10
Knowledge liberal democracy	39,379	8.21	1.80	0	10
Rate country's democracy	41,247	5.72	2.99	0	10
PRE-TREATMENT CONTROLS					
Age	41,395	38.88	16.42	18	99
Female	41,395	0.47	0.50	0	1
Highest education					
Below secondary	41,395	0.07	0.25	0	1
Secondary	41,395	0.46	0.50	0	1
University	41,395	0.48	0.50	0	1
Urban	41,395	0.59	0.49	0	1
Language: proficient	41,395	0.73	0.44	0	1
Democratic preference	41,395	8.64	2.07	0	10
Political interest	41,395	1.70	0.97	0	3
Turnout in last na. Election	41,395	0.67	0.47	0	1

Note: The number of observations of pre-treatment controls are based on the model predicting preference for democracy, presented in Table 1.

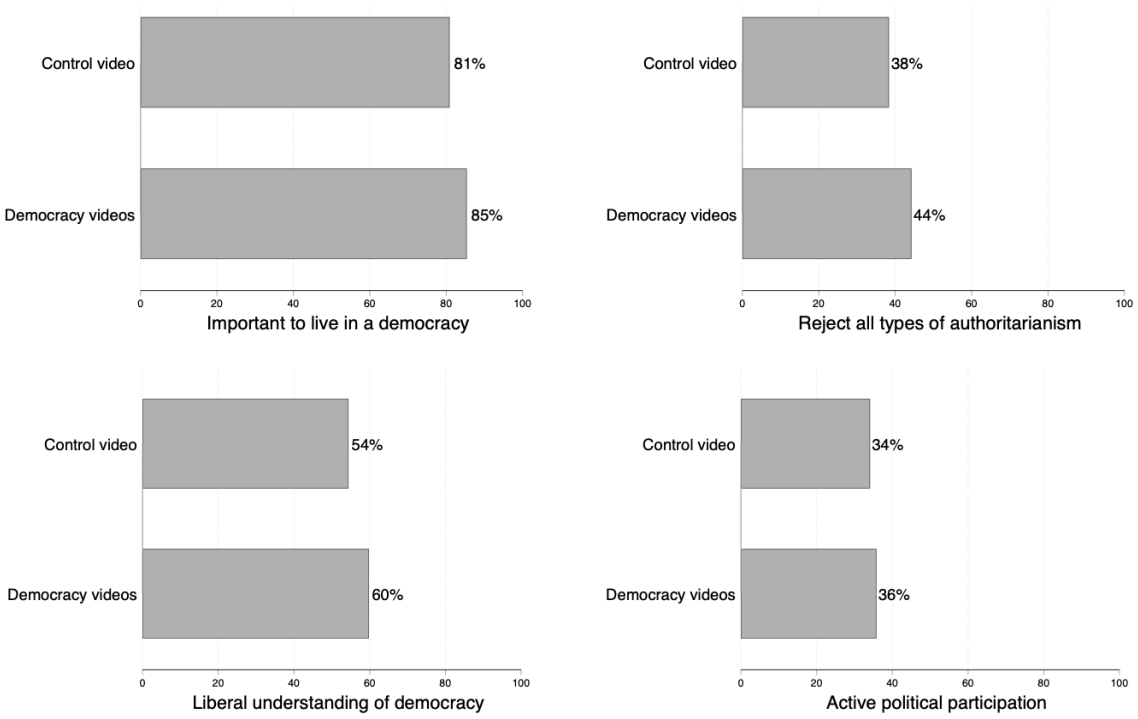
Table A.7: Correlation of macro indicators (sample of 32 countries)

	LibDem	Change in dem.	HDI	Const. Leg.	Civil lib.
Liberal democracy	1				
Change in lib democracy	0.34	1			
Human development index	0.46	-0.03	1		
Constraints on legislative	0.84	0.28	0.07	1	
Civil liberties	0.87	0.46	0.36	0.69	1

Table A.8: Intra-country correlations (ICC) of outcomes

	ICC
Change in democ. pref.	0.005
Rejection of auth. alternatives	0.141
Defending democracy	0.018
Knowledge liberal democracy	0.067
Rate country's democracy	0.185

Figure A.3: Descriptive treatment effects on pro-democratic attitudes (in %)



Notes: For questions with a scale from 0 (least democratic) to 10 (most democratic), we coded values higher than seven as democratic. The only exception is "rejecting authoritarianism", which has a scale of four response options. In this case, we show the proportion of respondents selecting "bad" or "very bad" for all three types of authoritarian regimes (strong man rule, army rule, single-party regime). All variables except "important to live in a democracy" are built of three questions; in these cases, we require responses to every single question to show democratic commitment.

G. Appendix: Sample comparisons to nationally representative samples

We compare the characteristics of our sample to reference data, which provides (weighted) representative data from each country included in our study. We calculated, for example, the percentage of women in our sample and the (weighted) reference data. On average we have 1% fewer women than what we would expect based on the reference data. Table A.9 summarizes the differences in several sample characteristics:

- Female respondents
- Those who respond 'very interested' or 'somewhat interested' in politics.
- People aged 34 and under (young).
- People aged 55 or older (old).
- People who do not have any qualifications or education higher than primary level.
- People who have an undergraduate degree or a higher degree.

Table A.9: Differences between study sample and representative data

Country	Reference Data	Year	Weight variable	% Female	Pol. interested	Young	Old	No/prim ed	Uni ed
Argentina	WVS-7	2017	W_WEIGHT	2%	22%	-12%	11%	-13%	8%
Argentina	LAPOP21	2021	wt	4%	NA	-17%	18%	-5%	-21%
Australia	WVS-7	2018	W_WEIGHT	7%	25%	15%	-1%	-1%	24%
Bangladesh	WVS-7	2018	NA - No weighting	-13%	4%	18%	-3%	-42%	45%
Bolivia	WVS-7	2017	NA - No weighting	-6%	20%	-15%	8%	-17%	10%
Bolivia	LAPOP21	2021	wt	-6%	NA	-11%	11%	-12%	3%
Chile	WVS-7	2018	W_WEIGHT	12%	25%	-8%	12%	6%	3%
Chile	LAPOP21	2021	wt	12%	NA	-15%	18%	4%	-17%
Colombia	WVS-7	2018	NA - No weighting	2%	33%	-18%	19%	-14%	3%
Colombia	LAPOP21	2021	wt	2%	NA	-11%	22%	-14%	-7%
Dominican Republic	LAPOP21	2021	wt	2%	NA	7%	2%	-28%	0%
Ecuador	WVS-7	2018	NA - No weighting	-1%	19%	-3%	6%	-15%	5%
Ecuador	LAPOP21	2021	wt	1%	NA	-8%	12%	-15%	0%
El Salvador	LAPOP21	2021	wt	-4%	NA	-2%	7%	-9%	0%
Ghana	AfroB2022	2019	Combinwt_new_hh	-15%	NA	28%	-13%	-37%	54%
Guatemala	WVS-7	2019	NA - No weighting	-10%	3%	-15%	8%	3%	-38%
Guatemala	LAPOP21	2021	wt	-8%	NA	-4%	2%	-20%	-15%
Honduras	LAPOP21	2021	wt	-2%	NA	0%	3%	-35%	8%
Hong Kong	WVS-7	2018	W_WEIGHT	21%	27%	21%	-32%	-14%	29%
India	WVS-6	2012	NA - No weighting	-3%	16%	19%	1%	-41%	49%
Kenya	WVS-7	2021	NA - No weighting	-9%	17%	0%	1%	-15%	41%
Kenya	AfroB2022	2019	Combinwt_new_hh	-9%	NA	18%	-9%	-38%	46%
Malaysia	WVS-7	2018	NA - No weighting	5%	18%	20%	-4%	-4%	44%
Mexico	WVS-7	2018	W_WEIGHT	-3%	38%	-15%	17%	-20%	11%
Mexico	LAPOP21	2021	wt	-1%	NA	-14%	17%	-20%	5%
Nicaragua	WVS-7	2020	NA - No weighting	-5%	2%	-10%	5%	-21%	8%
Nicaragua	LAPOP21	2021	wt	-4%	NA	-14%	10%	-23%	8%
Nigeria	WVS-7	2018	W_WEIGHT	-10%	12%	-2%	2%	-26%	45%
Nigeria	AfroB2022	2021	Combinwt_new_hh	-9%	NA	11%	-2%	-33%	48%
Pakistan	WVS-7	2018	NA - No weighting	4%	7%	4%	6%	-42%	61%
Paraguay	LAPOP21	2021	wt	-1%	NA	-2%	0%	-23%	1%
Peru	WVS-7	2018	W_WEIGHT	-1%	37%	-7%	12%	-12%	6%
Peru	LAPOP21	2021	wt	0%	NA	-12%	19%	-8%	-21%
Philippines	WVS-7	2019	W_WEIGHT	18%	-10%	4%	-3%	-34%	34%
Singapore	WVS-7	2020	W_WEIGHT	-4%	28%	42%	-24%	-11%	14%
South Africa	AfroB2022	2022	Combinwt_new_hh	1%	NA	4%	3%	-14%	16%
Spain	ESS10SC	2020	dweight	5%	16%	9%	-5%	-1%	-16%
Tanzania	AfroB2022	2022	Combinwt_new_hh	-18%	NA	26%	-12%	-73%	63%
Uganda	AfroB2022	2019	Combinwt_new_hh	-14%	NA	27%	-13%	-55%	47%
United Kingdom	WVS-7	2022	W_WEIGHT	12%	22%	1%	5%	0%	27%
United States	WVS-7	2017	W_WEIGHT	14%	14%	24%	-12%	3%	9%
Venezuela	WVS-7	2021	NA - No weighting	-5%	21%	-23%	18%	-14%	24%
Zambia	AfroB2022	2022	Combinwt_new_hh	-4%	NA	13%	-4%	-37%	40%
AVERAGES				-1%	18%	2%	3%	-20%	16%

H. Appendix: Full set of coefficients and alternative model specifications testing H1

Table A.10: Full results - Part 1: Table 1 without and with controls

Dependent Variable (DV)	Change democratic preferences				Rejection of auth. alternatives			
Treatment	0.167**	0.164**	0.170**	0.170**	0.164**	0.164**	0.161**	0.161**
	[0.015]	[0.014]	[0.010]	[0.012]	[0.015]	[0.014]	[0.011]	[0.014]
Age		0.002**	0.002**	0.002**		0.006**	0.005**	0.006**
		[0.001]	[0.000]	[0.000]		[0.001]	[0.000]	[0.001]
Female		0.024	0.028**	0.028*		-0.100**	-0.100**	-0.100**
		[0.013]	[0.009]	[0.011]		[0.022]	[0.010]	[0.022]
Education (ref: below secondary)								
Secondary		0.068**	0.070**	0.069**		0.296**	0.363**	0.359**
		[0.023]	[0.018]	[0.020]		[0.028]	[0.020]	[0.025]
University		0.06	0.070**	0.069**		0.514**	0.634**	0.626**
		[0.031]	[0.019]	[0.021]		[0.035]	[0.021]	[0.030]
Urban		-0.039**	-0.019*	-0.020*		0.058**	0.051**	0.051**
		[0.012]	[0.009]	[0.009]		[0.016]	[0.010]	[0.017]
Language: proficient		0.031	-0.002	-0		0.093**	0.065**	0.067*
		[0.027]	[0.011]	[0.013]		[0.025]	[0.012]	[0.027]
Democratic preference		-0.277**	-0.284**	-0.283**		0.062**	0.062**	0.062**
		[0.012]	[0.002]	[0.011]		[0.005]	[0.002]	[0.005]
Political interest (ref: not at all)								
Not very		0.051*	0.042**	0.042*		0.125**	0.129**	0.129**
		[0.019]	[0.014]	[0.019]		[0.024]	[0.016]	[0.023]
Somewhat		0.122**	0.112**	0.112**		0.272**	0.286**	0.285**
		[0.021]	[0.014]	[0.020]		[0.030]	[0.015]	[0.028]
Very		0.157**	0.148**	0.148**		0.324**	0.345**	0.343**
		[0.024]	[0.015]	[0.025]		[0.029]	[0.017]	[0.028]
Turnout in last elec.		0.077**	0.063**	0.064**		-0.070**	-0.085**	-0.084**
		[0.016]	[0.010]	[0.013]		[0.025]	[0.011]	[0.024]
Liberal democracy		0.213*		0.265**		-0.16		-0.189
		[0.094]		[0.090]		[0.097]		[0.105]
HDI		-0.344		-0.443**		0.057		0.021
		[0.178]		[0.170]		[0.144]		[0.156]
Country-level		Clustered SE	FE	RE		Clustered SE	FE	RE
Constant	0.000	2.249**	2.225**	2.372**	0.000	-1.318**	-1.337**	-1.333**
	[0.000]	[0.200]	[0.038]	[0.181]	[0.000]	[0.124]	[0.042]	[0.139]
Observations	43,300	41,395	41,395	41,395	43,285	41,001	41,001	41,001
R^2	0.005	0.3	0.311		0.005	0.085	0.098	

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression.

Table A.11: Full results - Part 2: Table 1 without and with controls

Dependent Variable (DV)	Defending democracy				Knowledge liberal democracy			
Treatment	0.050**	0.045**	0.046**	0.046**	0.143**	0.136**	0.130**	0.130**
	[0.012]	[0.013]	[0.011]	[0.013]	[0.014]	[0.014]	[0.010]	[0.013]
Age		0.002**	0.002**	0.002**		0.002**	0.002**	0.002**
		[0.000]	[0.000]	[0.000]		[0.001]	[0.000]	[0.001]
Female		-0.138**	-0.135**	-0.136**		-0.164**	-0.171**	-0.170**
		[0.014]	[0.010]	[0.014]		[0.017]	[0.009]	[0.016]
Education (ref: below secondary)								
Secondary		-0.013	-0.012	-0.013		-0.001	-0.025	-0.023
		[0.019]	[0.021]	[0.019]		[0.036]	[0.020]	[0.031]
University		0.037	0.04	0.039		0.112*	0.077**	0.081*
		[0.023]	[0.022]	[0.023]		[0.044]	[0.021]	[0.034]
Urban		0.009	0.012	0.011		0.048**	0.037**	0.038**
		[0.010]	[0.010]	[0.009]		[0.014]	[0.009]	[0.013]
Language: proficient		0.022	0.02	0.021		0.051*	0.104**	0.099**
		[0.012]	[0.013]	[0.011]		[0.020]	[0.011]	[0.017]
Democratic preference		0.009**	0.008**	0.008**		0.139**	0.143**	0.143**
		[0.003]	[0.002]	[0.003]		[0.007]	[0.002]	[0.007]
Political interest (ref: not at all)								
Not very		0.024	0.024	0.024		0.104**	0.108**	0.108**
		[0.018]	[0.016]	[0.018]		[0.017]	[0.015]	[0.017]
Somewhat		0.062**	0.061**	0.061**		0.239**	0.245**	0.244**
		[0.015]	[0.016]	[0.016]		[0.016]	[0.014]	[0.018]
Very		0.111**	0.106**	0.108**		0.393**	0.404**	0.403**
		[0.020]	[0.018]	[0.021]		[0.024]	[0.016]	[0.025]
Turnout in last elec.		-0.038*	-0.030*	-0.033*		0.046**	0.058**	0.057**
		[0.015]	[0.012]	[0.014]		[0.015]	[0.011]	[0.014]
Liberal democracy		0.016		0.021		-0.14		-0.215
		[0.031]		[0.031]		[0.098]		[0.111]
HDI		-0.035		-0.046		0.107		0.148
		[0.063]		[0.057]		[0.174]		[0.183]
Country-level	Clustered SE		FE	RE	Clustered SE		FE	RE
Constant	-0.000	-0.119	-0.118**	-0.113	-0.000	-1.550**	-1.679**	-1.602**
	[0.000]	[0.067]	[0.044]	[0.065]	[0.000]	[0.146]	[0.040]	[0.140]
Observations	44,483	42,003	42,003	42,003	41,750	39,518	39,518	39,518
R^2	0.000	0.009	0.011		0.004	0.153	0.161	

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression.

I. Appendix: Treatment effects for each treatment separately

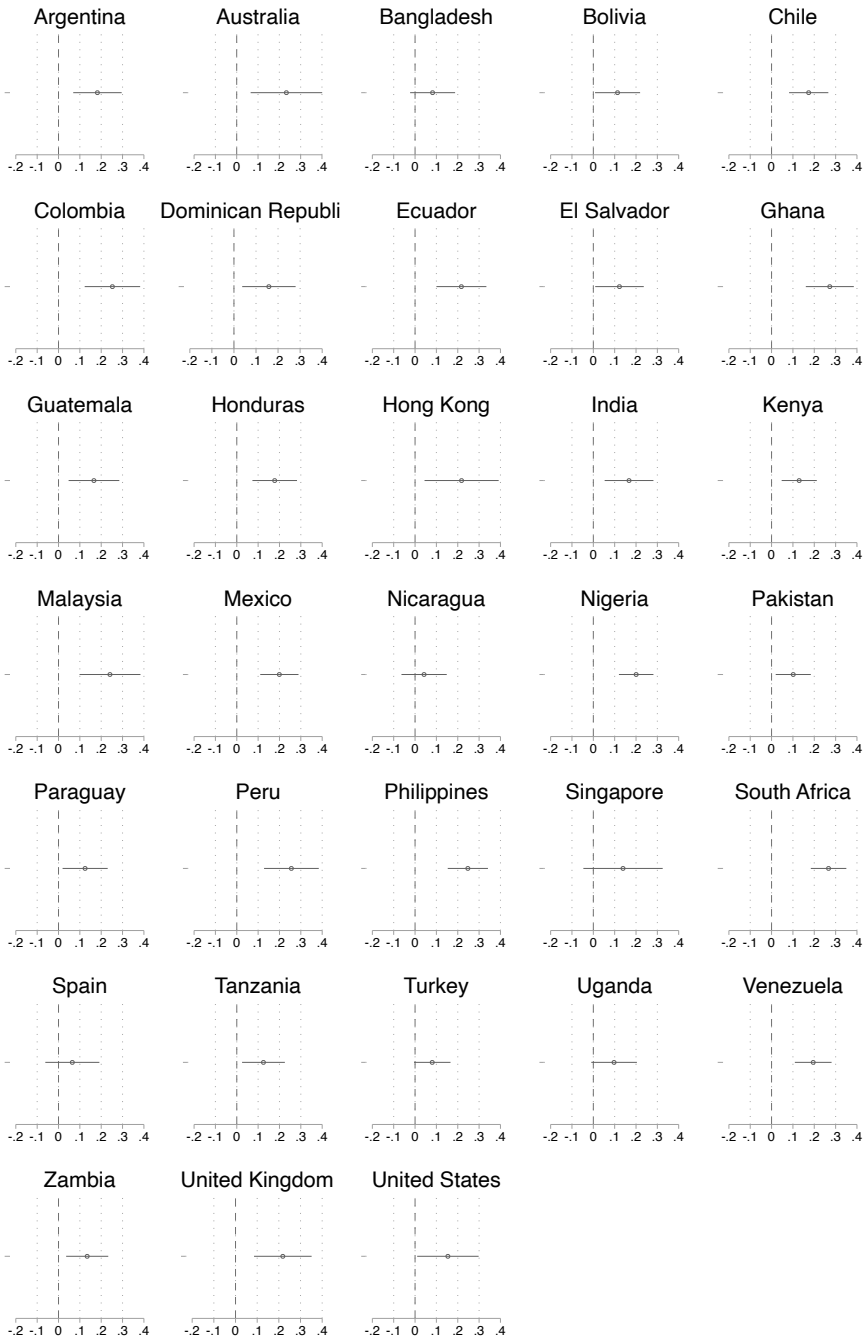
Table A.12: Treatment effects for each treatment separately

Dependent Variable (DV)	Change dem pref	Authoritarian support	Defending democracy	Knwoledge lib. democracy
Treatment (vs placebo)				
Institutions	0.134**	0.220**	0.067**	0.160**
	[0.014]	[0.018]	[0.017]	[0.014]
Rights	0.196**	0.163**	0.067**	0.158**
	[0.018]	[0.017]	[0.014]	[0.017]
Output	0.162**	0.110**	-0.001	0.089**
	[0.015]	[0.016]	[0.014]	[0.018]
Covariates	YES	YES	YES	YES
Observations	41,395	41,001	42,003	39,518
R^2	0.30	0.09	0.01	0.15
Control Mean	0	0	0	0

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regressions with country-clustered standard errors.

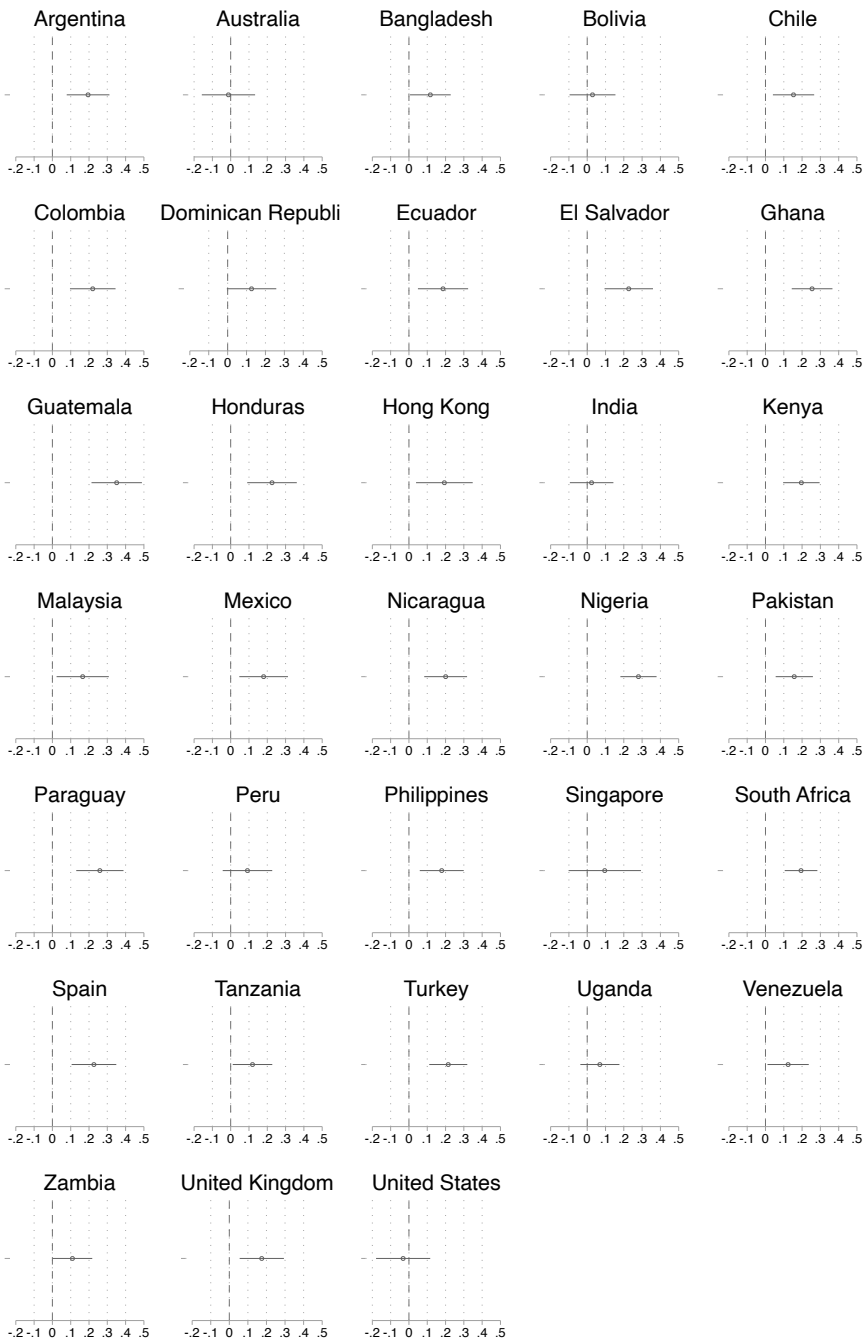
J. Appendix: Main effects (H1) by country

Figure A.4: H1: Marginal effects (and their 95% confidence intervals) of treatments on change in democratic preferences by country



Note: Results are based on linear regression with pre-treatment individual-level controls.

Figure A.5: H1: Marginal effects (and their 95% confidence intervals) of treatments on rejection of authoritarianism by country



Note: Results are based on linear regression with pre-treatment individual-level controls.

K. Appendix: Treatment effects for additional outcomes

Table A.13: Treatment effects for alternative outcomes

Outcome:	Intended Turnout	Political Participation	Knowledge: Democracy is... Populist	People obey	Econ. equality
Treatment	0.043**	0.049**	0.088**	-0.015	0.032
	[0.011]	[0.010]	[0.012]	[0.013]	[0.016]
Covariates	YES	YES	YES	YES	YES
Observations	42,020	41,182	38,928	38,858	39,006
R^2	0.126	0.139	0.075	0.012	0.024
Control Mean	0	0	0	0	0
Control Mean (raw)	7.91	6.745	7.841	6.302	6.769
Min (raw)	0	0	0	0	0
Max (raw)	10	10	10	10	10

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regressions with country-clustered standard errors.

K.1. Willingness to donate for democratic institutions

The outcome variables we have used in this project focus primarily on democratic attitudes. Can civic education treatments also affect behavioral outcomes? To answer this question, we have included an additional outcome variable in the Turkish survey we conducted with 14,712 respondents. For more details on this survey, see the Online Appendix Section L.

We asked our respondents in Turkey whether they would donate money to "a non-partisan civil society organization working to promote Turkish democracy, such as *Turkish Democracy Foundation* or *Istanpol Institute*," if they won 10,000 TL (350 USD) from a lottery. If they gave a positive answer to this question, we asked them how much they would donate. In response to the first question, 55% of our respondents said that they would donate some money. The mean amount of donations among these respondents was 3454 TL (120 USD). We then created an outcome variable, ranging from 0 to 10,000 and documenting how much each respondent would donate to a democratic cause. Those who responded negatively to the first question received a value of zero.

Table A.14 presents the results of regression analyses exploring how exposure to the treatment influences the willingness to donate money for democratic purposes. These results demonstrate that our treatment has a statistically significant effect on the willingness to donate money to a democratic cause.

Table A.14: Testing donation outcome

	(1) Base	(2) W. Controls
Treatment	104.128*	105.183*
	(45.064)	(46.564)
Age		−17.439***
		(1.790)
Female		−240.183***
		(49.357)
Education		65.595*
		(31.751)
Support for democracy		103.247***
		(11.354)
Political Interest		241.575***
		(28.094)
Observations	13,484	12,388

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

L. Appendix: The follow-up survey in Turkey

L.1. Data collection

Turkey was the last country to be surveyed for the cross-national analysis. We combined the data collection in this country for the cross-national analysis with the data collection for the follow-up analysis. A power calculation based on the cross-national data collected before the Turkish study showed that we needed at least 6,000 respondents to arrive at statistically significant results in Turkey. Assuming an attrition rate of 50%, we targeted recruiting at least 12,000 respondents in Turkey. We ended up recruiting 14,712 social media users who completed the entire survey. This number does not include 612 (465) observations dropped because their IP address (contact number) was the same as at least one other observation.

We started the data collection on 27 September 2023. We reached 3,000 participants, which was necessary for the cross-national study, by 30 September 2023. This first group of respondents completed the same questionnaire as those from other countries who participated in the cross-national study. The remaining participants in Turkey completed a shorter questionnaire we designed to minimize the attrition rate during the follow-up period. The pre-treatment part of this questionnaire only included questions on gender, age, education level, democratic preferences, and political interest. The treatment was the same as the one we used in the cross-national analyses, except the videos were in Turkish. Importantly, however, half of the respondents were assigned to the placebo condition to maximize the statistical power. All of our analyses below group respondents assigned to different versions of the treatment videos together and compare them to the respondents assigned to the placebo video. The data collection for the original survey ended on 10 October 2023.

There are no established expectations in the literature regarding how long civic education treatments are effective. Thus, we formed our recontact strategy with minimal expectations regarding the longevity of our effects. Our first goal was to test whether the effects lasted in the four days after the respondents took the survey. For this reason, all of the 14,712 survey respondents received an SMS message from us four days after they completed the original survey. We then sent two more SMS messages and an email to our respondents between 14 October and 21 October.

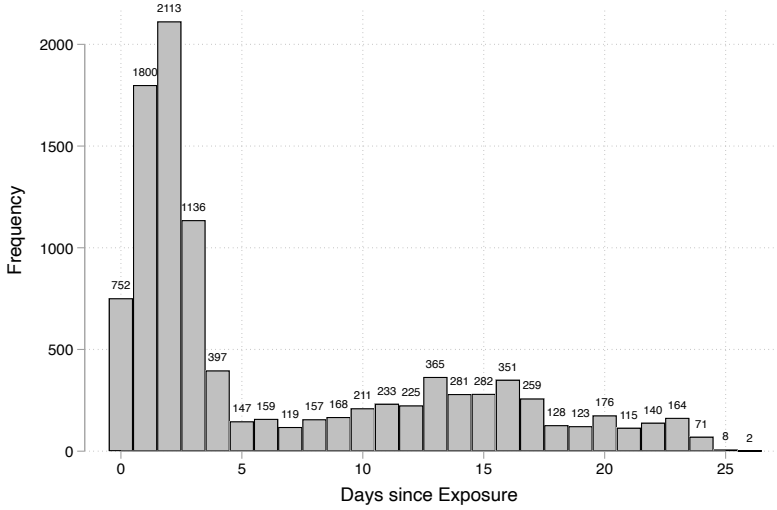
Each of our four messages included a personalized and unique link to our follow-up survey hosted on Qualtrics. The unique response ID that each respondent was assigned when they participated in the original survey was embedded in these personalized links to follow-up surveys. We merged follow-up surveys and original surveys using these response IDs.

Our first recontact attempt, which took place in the first four days after the completion of the original survey, was also the most successful one. 61% of all follow-up observations were recorded during this period. Overall, we collected 10,082 responses from 30 September 2023 until 23 October 2023. Figure A.6 documents the distribution of all observations, based on how many days passed between the exposure to the treatment and the follow-up response.

At the individual level, our data comprises 6905 respondents, 4,604 of whom responded to our follow-up surveys only once, 1,545 of whom responded to the follow-up surveys twice, 636 of whom responded three times, and 120 of whom responded four times. This also means that 47% of respondents who took the original survey responded to at least one of the follow-up surveys.

To minimize the attrition rate, we limited our follow-up surveys to questions measuring the two outcome variables that are foundational to the purposes of this study: individual-level change in democratic support and support for authoritarianism.

Figure A.6: Distribution of observations during the recontact period

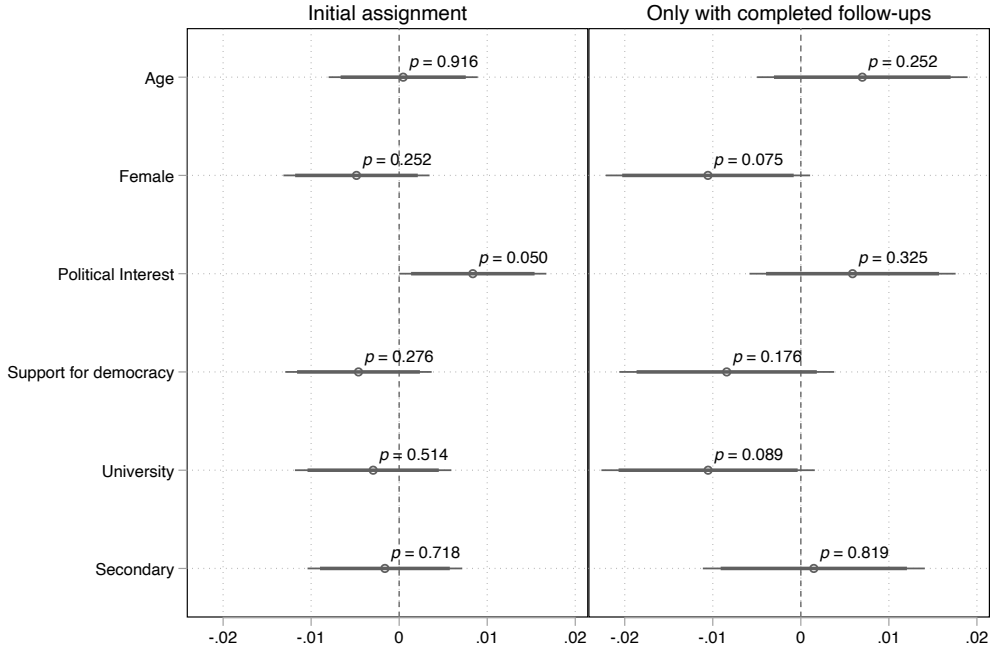


Note: This analysis is based on the difference of calendar days among observations; it does not take into account how many hours there were between observations.

L.2. Balance Tests

Did the randomization work as intended? Our analysis is presented in Figure A.7. We present the results separately for all the participants assigned to one of the treatment groups and for participants who completed the follow-up survey.

Figure A.7: Balance test: Determinants of assignment into the treatment



Note: OLS regression. All variables are standardized around the mean.

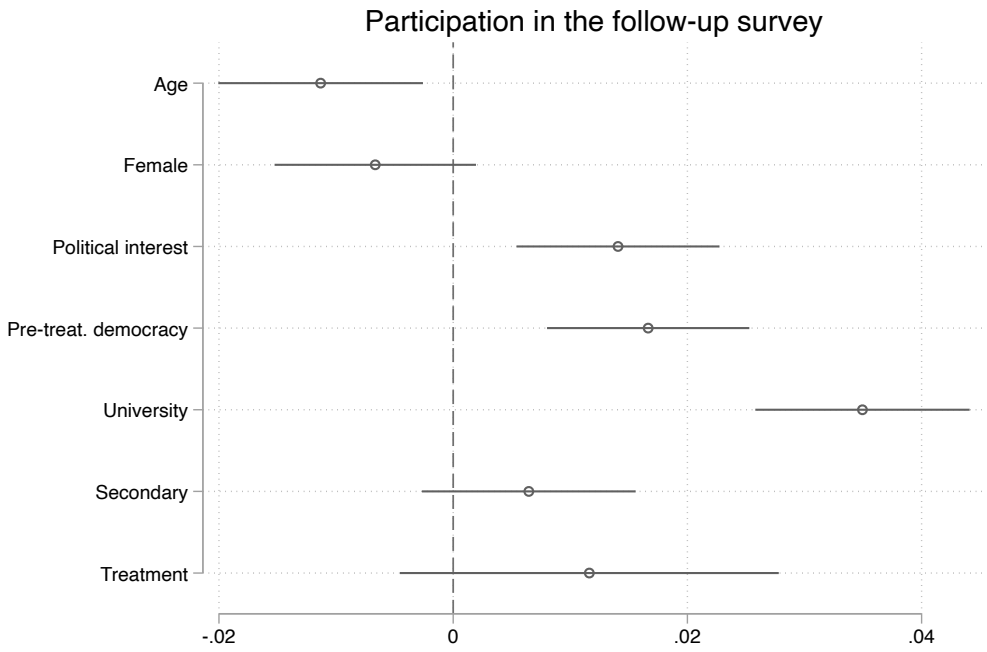
The results demonstrate that higher pre-treatment levels of political interest were associated with the initial assignment into the treatment group (p=0.05). Yet, this relationship no more holds once we limit our sample to the respondents who completed the follow-up survey. We do not have any other relationship between the pre-treatment variables and the treatment that is statistically significant at a 0.05 or lower significance level.

L.3. Attrition

Despite our best efforts, around 37 % of the respondents who completed the original survey did not participate in the follow-up survey. Do the respondents who participated in the follow-up survey differ from the respondents who did not? To test this, we ran a regression analysis, in which participation in the follow-up survey was the dependent variable and pre-treatment variables in the original survey were independent variables. We limited the sample to respondents who completed the original survey. Results are presented in Figure A.8.

These results demonstrate that respondents who have higher levels of education, political interest, and support for democracy and were younger were more likely to participate in the follow-up survey. The relationship is most significant for university graduates. The university graduates form 22% if the original sample and 25% of the followup sample. The question, then, is how the attrition impacted our results.

Figure A.8: Attrition: Determinants of participating in the follow-up



Note: All variables are standardized around the mean.

Fortunately, we measured the effect sizes in the original survey, right after our respondents were exposed to the treatment, for both of the outcome variables used in the follow-up study. Exploring how these immediate treatment effects were dependent on levels of education, political interest, age, and support for democracy can help us to analyse how the biases in follow-up sample might have affected the treatment effects measured in those samples. We present the analysis of heterogeneous effects in the original sample in the following two pages. Figure A.9 presents the analysis based on a categorical interaction of the variables of interest; Figure A.10 presents the results based on continuous interactions.

Figure A.9: Heterogeneous effects in the original sample: categorical interaction

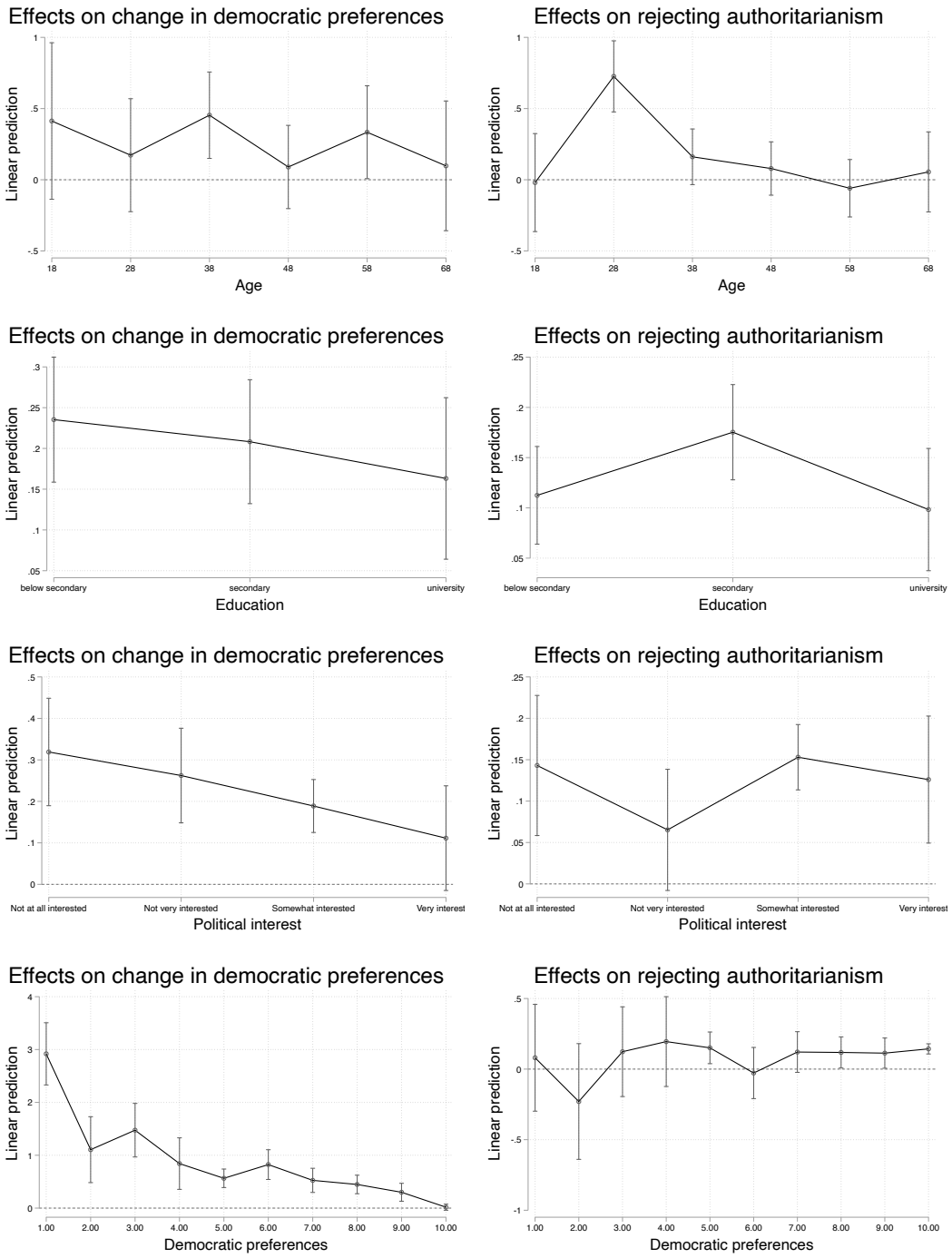
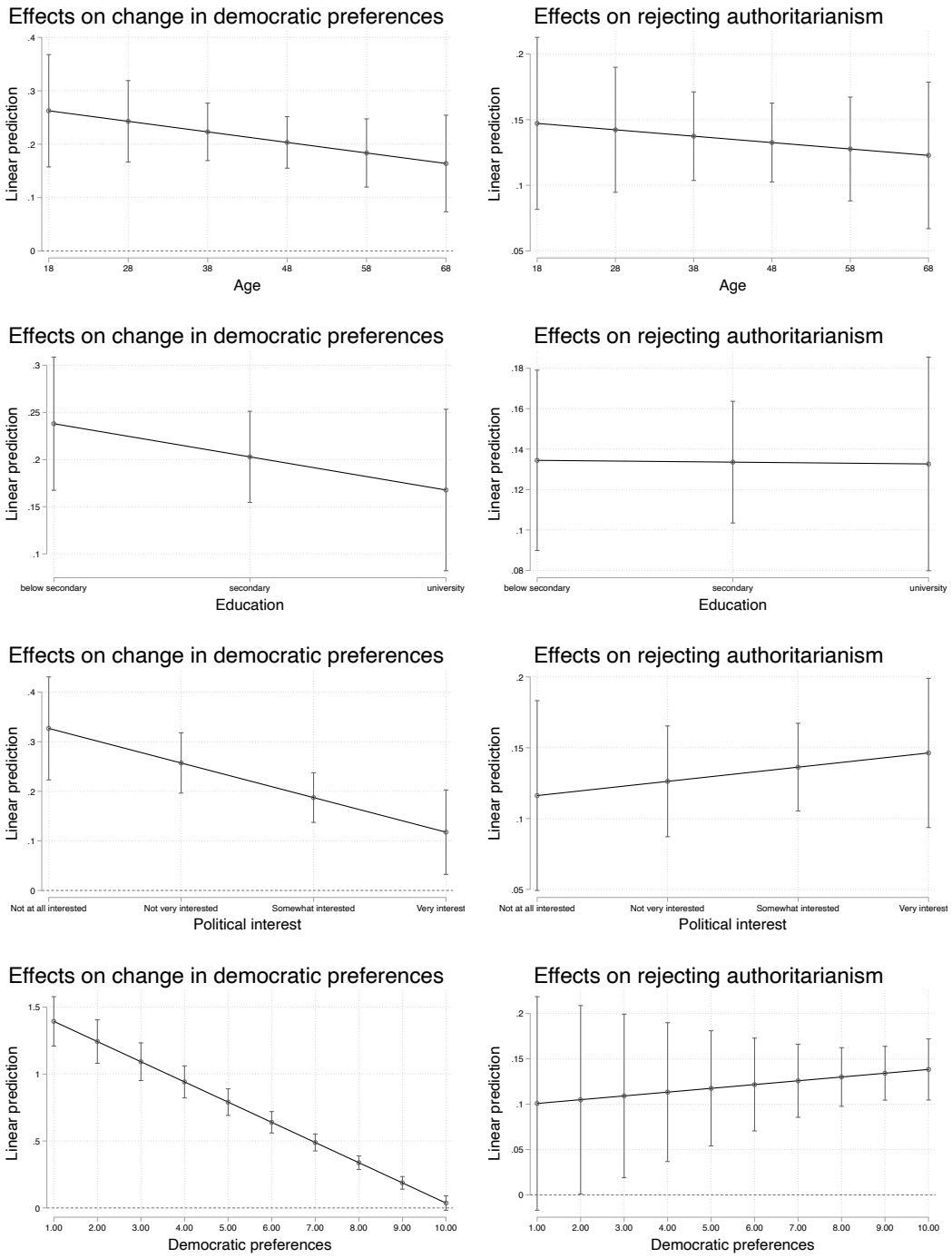


Figure A.10: Heterogeneous effects in the original sample: continuous interaction



To interpret these results, we focus on two questions. First, is there a statistically and substantially significant heterogeneity in terms of the effects? Second, given the attrition in the follow-up sample, would this heterogeneity result in the overestimation or underestimation of the effect sizes in the follow-up sample? Building on Figure A.9, we summarize our responses to these two questions in Table A.15.

Table A.15: How attrition during recontact may have affected the effect sizes

	Change in Preferences		**Rej. Authoritarianism**	
Variables	**Heterogeneity**	**Effect**	**Heterogeneity**	**Effect**
Age	No clear heteregoneity	N/A	No clear heteregoneity	N/A
Education	No clear heteregoneity	N/A	No clear heterogeneity	N/A
Interest	Lower more responsive	Underestimate	No clear heterogeneity	N/A
Dem. Pref.	Lower more responsive	Underestimate	No clear heterogeneity	N/A

Our results do not suggest a clear case of attrition bias that could result to an overestimation of effect sizes. To the extent that there is a heterogeneity in the effects, results point to an underestimation of the effect sizes. For example, the follow-up sample includes a slightly higher proportion of respondents with higher political interest. In average, these respondents were less responsive to the treatment in the original survey.

L.4. Results

Table A.16 documents the average effect of the exposure to the treatment on the democratic attitudes of responses reported in the follow-up surveys.

Table A.16: Main effects in the follow-up survey

	Change in Dem. Sup.		Auth. Support	
	(1) Base	(2) W. Controls	(3) Base	(4) W. Controls
Treatment	0.140**	0.144**	0.076**	0.076***
	(0.051)	(0.052)	(0.026)	(0.023)
Age		0.006**		0.015***
		(0.002)		(0.001)
Female		−0.029		−0.172***
		(0.057)		(0.025)
Education		−0.091**		−0.343***
		(0.032)		(0.015)
Political Interest		0.011		−0.077***
		(0.037)		(0.015)
Support for democracy				−0.044***
				(0.007)
Observations	8,728	8,262	8,341	7,833

Significance levels: *** p< 0.001, ** p< 0.01, * p< 0.05. Results are based on linear regressions with standard errors clustered at the individual level.

M. Appendix: Heterogeneous treatment effects - continuous level macro context

All results presented here are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level control: Liberal democracy score (H4) or Human development index (H2).

Figure A.11: H2: Marginal effects (and their 95% confidence intervals) of treatments by level of liberal democracy

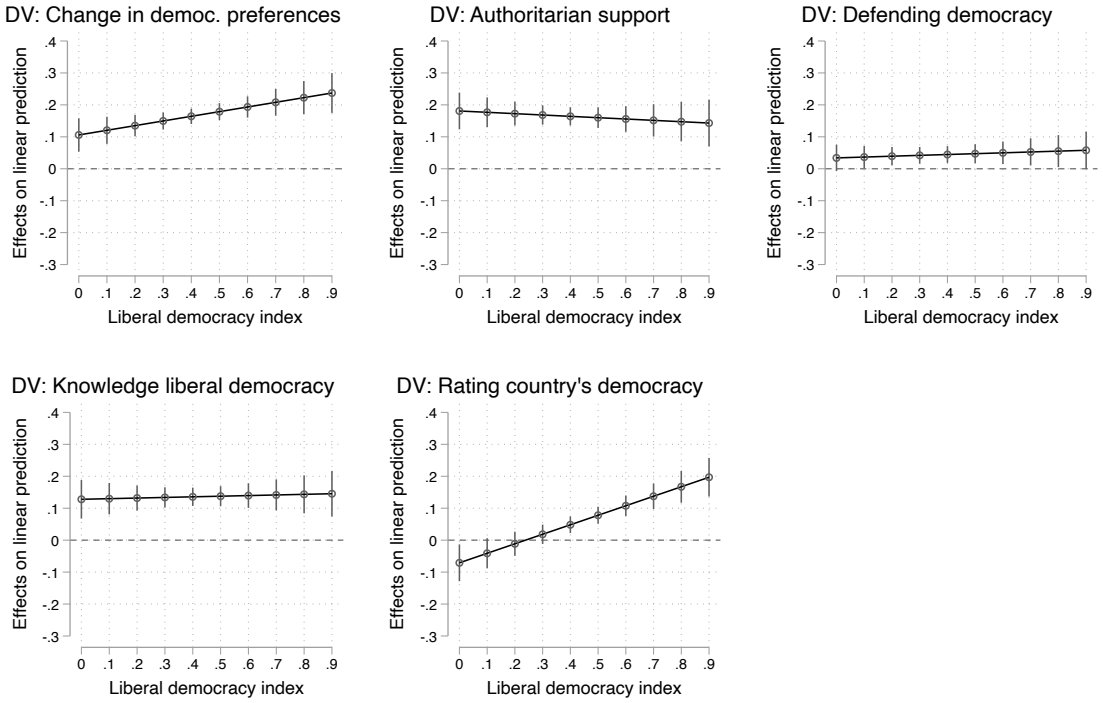


Figure A.12: H3: Marginal effects (and their 95% confidence intervals) of treatments by change in liberal democracy

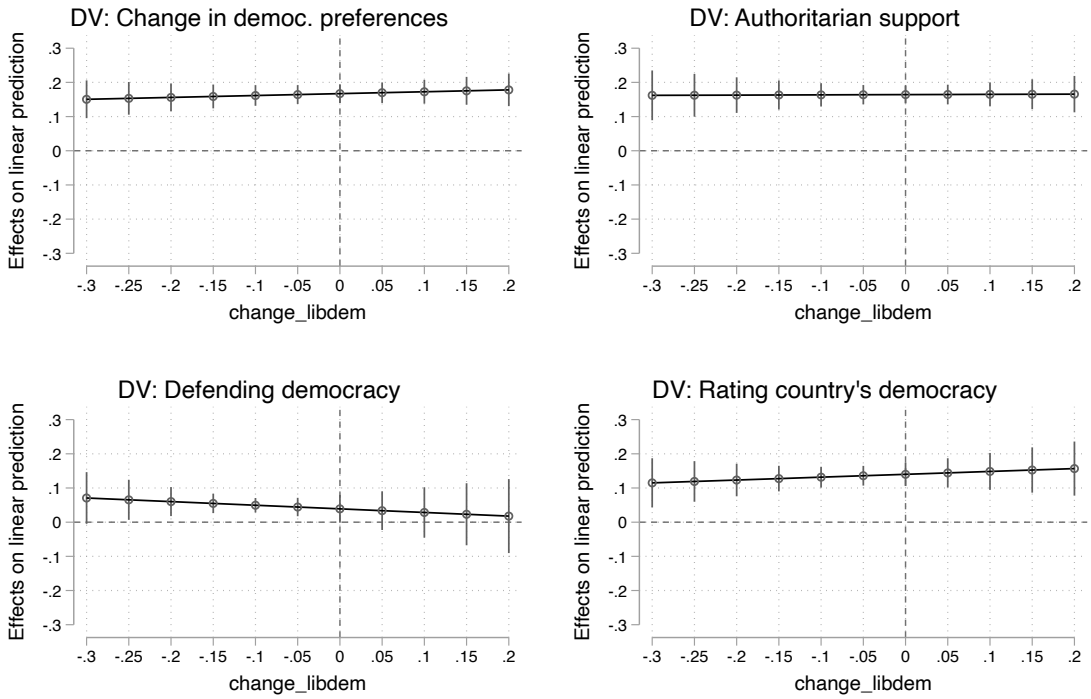
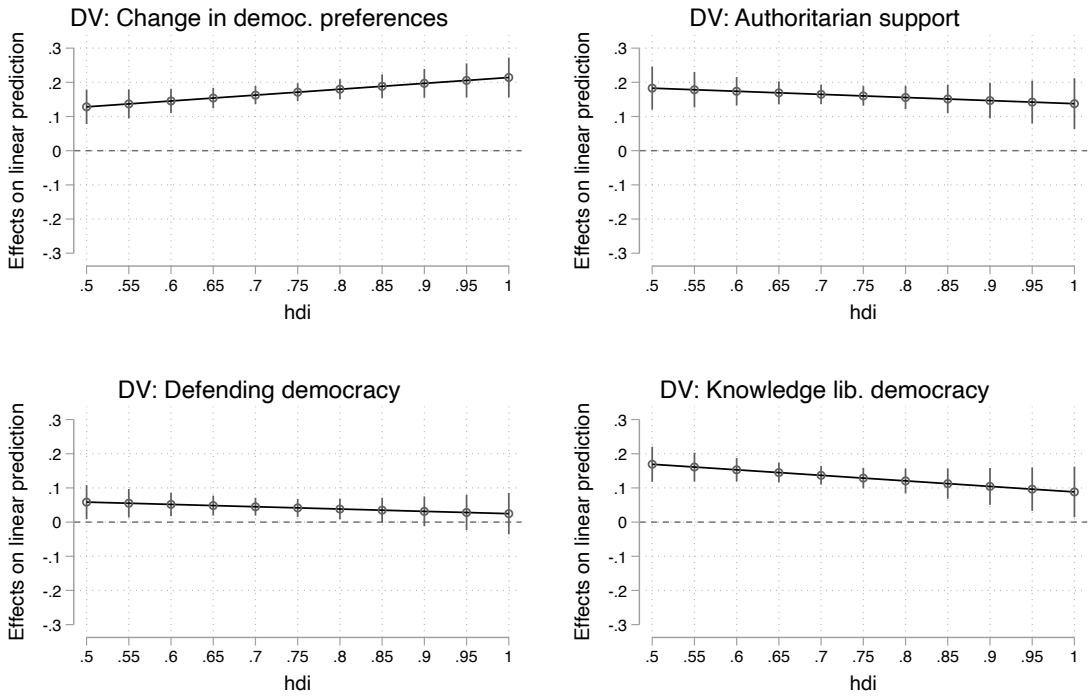


Figure A.13: H4: Marginal effects (and their 95% confidence intervals) of treatments by HDI



N. Appendix: Heterogeneous treatment effects - additional results

In this section, we present two additional sets of hypotheses and analyses that were pre-registered, but for space reasons are not presented in the main manuscript.

N.1. Autocratization and democratization

It is plausible that civic education impacts will be conditioned not only on the level of democracy but also on the extent of recent changes in democratic development. The early evaluations in the field which showed generally positive civic education impacts, for example, were mainly conducted in contexts where there had been recent democratic breakthroughs (e.g., South Africa, Poland, and Kenya, see **??**). Based on this evidence, we hypothesize that civic education may serve to facilitate or amplify positive changes that are otherwise underway in a given context. On the other hand, it may also be the case that civic education may be effective in countries experiencing democratic backsliding, as the "deficit" hypothesis above would predict, given that issues related to democracy in autocratizing contexts may become more salient. If both of these processes are at work, we would expect that civic education impacts will be greater when there is substantial recent *change* in democratic levels compared to relative stasis:

Hypothesis – Democratic trajectory (H4): CE treatments (H1) will be more effective when countries are experiencing substantial changes in the democratic environment.

We measure the democratic trajectory of a country by using the 10-year **change in a country's liberal democracy index** (2012 to 2022) and dividing the sample into three categories. Countries that are backsliding have a change value of -0.1 or lower, while countries that are democratizing have a value of +0.1 or higher. All other countries were classified as stable, as their liberal democracy score did not change much in the 10 years.

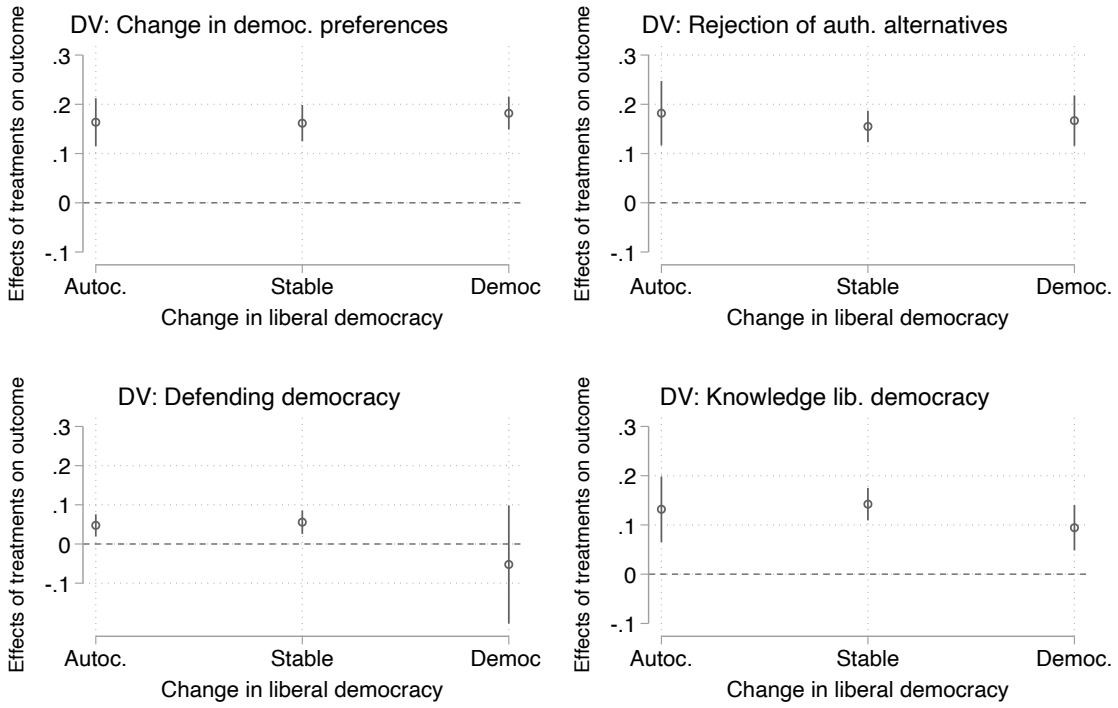
Figure A.14 presents the results testing the trajectory hypothesis (H4), which focuses on the conditioning impact of political change. The democratic change indicator is interacted with our pooled treatment effect.

As Figure A.14 demonstrates, the results testing H4 are mostly rejected. The political trajectory of a country does not condition the impact of our treatments on respondents' preference for democracy, their rejection of authoritarian alternatives, or their knowledge of liberal democracy. The estimated marginal effects are the same across the values of democratic change for these two outcomes. The picture looks more nuanced for respondents' willingness to support pro-democratic candidates. The treatments only have an impact in countries that are either backsliding or that are stable. The videos however are not effective in democratizing contexts.

N.2. Matching civic education content and context

Our final set of hypotheses extends the logic of H2 and H4, i.e., the expectation that the effects of the civic education treatments will be greatest in countries in the mid-range of democratic development, to the expected effects of exposure to each of the individual treatments. That is, we expect that the levels of *specific dimensions* of democratic and economic development will interact with the treatment corresponding to that particular dimension. For example, the effects of the treatment frame emphasizing democratic rights and liberties should depend on the level of rights protection that exists in a particular context, while the effects of the treatment frame emphasizing the beneficial economic

Figure A.14: H4: Marginal effects (and their 95% confidence intervals) of treatments by level of democratic development



Note: Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level control: Liberal democracy index and human development index.

and other outputs of democracy should depend on the country's level of economic development (extending H3).

We follow the expectations of H2 in predicting curvilinear conditional effects of the rights and liberties, and institutional checks/rule of law treatments depending on macro-contextual levels of civil rights protection and executive constraints, respectively. That is, we expect the effects of these treatments to be greatest among countries at the middle level of these democratic dimensions. For the effects of the economic output treatment, however, we posit that this treatment may be more effective in "selling democracy" in poorly performing economic contexts. As discussed above, the economic and security concerns that predominate in these settings may lead to an increased resonance of the CE frame's emphasis on the beneficial outcomes that democratic regimes tend to produce. These considerations lead to the following hypotheses regarding the *match* between the expected effects of the treatment frames and the macro-level characteristics of the country related specifically to that treatment's content:

Hypothesis 5a (H5a) – political rights match: CE treatments that focus on civil rights have the strongest impact in countries in the middle of the civil rights index.

Hypothesis 5b (H5b) – political institutions match: CE treatments that focus on separation of power have the strongest impact in countries in the middle of the executive constraint index.

Hypothesis 5c (H5c) – economic match: CE treatments that focus on the economic performance of democracy have the strongest impact in low-development contexts.[3]

We use terciles of V-Dem's "**Civil Liberties Index**" (CLI) (`v2x_civlib`), which closely matches the content of our rights video (H5a) as well as terciles of V-Dem's "**Legislative Constraints on the Executive Index**" (LCEI) (`v2xlg_legcon`), which closely matches the content of our institution video (H5b). Lastly, we use tercile split of HDI to match the output treatment (H5c).

For H5a, we ran the same specification as in the manuscript (testing H2+H3) but added a dummy where we pooled the institutions/output treatments in one dummy (=1 if institutions=1 or output=1; =0 otherwise) and another dummy for the rights treatment (rights=1; 0 otherwise). These dummies were interacted with the CLI terciles to capture the expected non-linear relationship based on deficiency theory. Again, our coefficients are interpreted relative to the placebo. For H5b, in a new specification, we followed a similar logic. We pooled rights/output (=1 if rights=1 or output=1; 0 otherwise) and added another dummy for the institution treatment (institutions=1; 0 otherwise). These dummies were interacted with the LCEI terciles. For H5c, in a new specification, we pooled rights/institutions (=1 if rights=1 or institutions=1; 0 otherwise) and another dummy for the output treatment (=1 if output==1; 0 otherwise). These dummies were interacted with the human development terciles. All regression coefficients of the main variables and interactions are reported in the Appendix (Tables A.17, A.18 and A.19).

The bottom two rows of Tables A.17, A.18 and A.19 present the p-values of F-tests comparing the interaction coefficients with terciles (low and high), whereby one pair represents a matching treatment and contextual variable, while the other does not. For instance, in the penultimate row of Table A.17, we compare the interaction coefficient between the rights treatment with the low tercile partition of CLI (matched treatment/context) and the interaction coefficient of the institutions/output (pooled) with the low tercile of CLI (non-matched treatments/context). In other words, we test whether the coefficient of `Rights*CLI Low` equals the coefficient of `Institutions/Output*CLI Low` for each dependent variable. If H5a is correct, we would expect the former coefficient to be significantly stronger than the latter.

For H5a, Table A.17 shows that the rights treatment is not stronger than the Institutions/Output (pooled) treatments when conditioning the effect on the civil liberties enjoyed in each of our cases. Only for defending democracy, we observe a slightly stronger impact of the rights video than the pooled treatment, see Table A.17. Similarly, for H5b, we do not find evidence supporting stronger effects when matching the LCEI terciles to the institution treatment compared to matching the specific context to the other two treatments. For H5c, we find some evidence that the output video presented larger effects on the rejection of authoritarianism for low-development contexts. However, overall we do not find stronger impacts of the output treatment in low-development contexts, as hypothesized in H5c.

Looking at the coefficients of the interactions between the treatments and the macro-level terciles in Tables A.17 and A.18 we also do not find curvilinear effects as hypothesized in H5a and H5b. This confirms the findings already presented in the previous section. We therefore reject the deficiency theory.

In sum, we do not find support for our hypothesis that matching the content of civic education interventions to the specific country contexts improves their impact. The results presented here rather

[3] This hypothesis is slightly reformulated from the original registration.

suggest that the different frames on promoting democracy worked very similarly across different contexts.

Table A.17: H5a: Video Content by Civil Liberties Index (CLI)

Dependent Variable (DV)	Change dem pref	Authoritarian support	Defending democracy	Knowledge lib. democracy
Rights	0.211**	0.194**	0.060**	0.179**
	[0.020]	[0.026]	[0.017]	[0.022]
Institutions/Output	0.164**	0.190**	0.052*	0.128**
	[0.016]	[0.020]	[0.021]	[0.019]
Level of democracy (CLI Low)	-0.010	-0.054	0.003	0.012
	[0.054]	[0.036]	[0.018]	[0.045]
Level of democracy (CLI High)	0.019	0.079	-0.024	0.012
	[0.041]	[0.039]	[0.023]	[0.029]
Rights*CLI Low	-0.092*	-0.070	0.032	-0.052
	[0.036]	[0.035]	[0.026]	[0.037]
Rights*CLI High	0.042	-0.044	-0.006	-0.025
	[0.033]	[0.040]	[0.041]	[0.043]
Inst/Out*CLI Low	-0.053	-0.048	-0.040	-0.015
	[0.029]	[0.032]	[0.026]	[0.034]
Inst/Out*CLI High	-0.002	-0.044	-0.026	0.003
	[0.024]	[0.036]	[0.040]	[0.035]
Observations	41,395	41,001	42,003	39,518
R-squared	0.301	0.087	0.010	0.153
Control Mean	0	0	0	0
Rights Low vs Inst/Out Low	0.177	0.518	0.009	0.231
Rights High vs Inst/Out High	0.123	0.998	0.444	0.297

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level controls: Level of liberal democracy and human development index.

Table A.18: H5b: Video Content by Legislative Constraints on the Executive Index (LCEI)

Dependent Variable (DV)	Change dem pref	Authoritarian support	Defending democracy	Knowledge lib. democracy
Institutions	0.119**	0.231**	0.030	0.150**
	[0.020]	[0.032]	[0.026]	[0.022]
Rights/Output	0.183**	0.128**	0.000	0.118**
	[0.022]	[0.026]	[0.022]	[0.021]
Legislative constraints (LCEI Low)	0.125*	0.104	0.026	-0.002
	[0.048]	[0.052]	[0.020]	[0.047]
Legislative constraints (LCEI High)	0.044	-0.094*	-0.011	-0.059
	[0.039]	[0.035]	[0.014]	[0.034]
Institutions*LCEI Low	0.005	0.004	0.011	-0.024
	[0.029]	[0.042]	[0.034]	[0.030]
Institutions*LCEI High	0.046	-0.043	0.103*	0.046
	[0.029]	[0.044]	[0.039]	[0.033]
Rights/Output*LCEI Low	-0.034	0.033	0.054	-0.004
	[0.033]	[0.034]	[0.030]	[0.042]
Rights/Output*LCEI High	0.020	-0.009	0.053	0.014
	[0.031]	[0.037]	[0.030]	[0.032]
Observations	41,395	41,001	42,003	39,518
R-squared	0.302	0.089	0.010	0.154
Control Mean	0	0	0	0
Institutions Low vs Rights/Out Low	0.181	0.429	0.120	0.599
Institutions High vs Rights/Out High	0.250	0.438	0.082	0.186

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level controls: Level of liberal democracy and human development index.

Table A.19: H5c: Video Content by Human Development Index (HDI)

Dependent Variable (DV)	Change dem pref	Authoritarian support	Defending democracy	Knowledge lib. democracy
Output	0.202**	0.088**	-0.015	0.058**
	[0.020]	[0.020]	[0.024]	[0.019]
Rights/Institutions	0.186**	0.201**	0.051*	0.135**
	[0.023]	[0.019]	[0.025]	[0.017]
Human Development (HDI Low)	0.013	-0.057	0.019	-0.013
	[0.059]	[0.073]	[0.025]	[0.056]
Human Development (HDI High)	-0.025	0.009	-0.054	-0.022
	[0.079]	[0.072]	[0.027]	[0.063]
Output*HDI Low	-0.064	0.064	0.018	0.052
	[0.032]	[0.033]	[0.035]	[0.032]
Output*HDI High	-0.058	-0.011	0.023	0.039
	[0.033]	[0.042]	[0.031]	[0.050]
Rights/Institutions*HDI Low	-0.054	-0.017	0.043	0.061*
	[0.032]	[0.036]	[0.031]	[0.026]
Rights/Institutions*HDI High	-0.002	-0.011	-0.002	0.002
	[0.034]	[0.031]	[0.032]	[0.034]
Observations	41,395	41,001	42,003	39,518
R-squared	0.300	0.087	0.010	0.154
Control Mean	0	0	0	0
Output Low vs Rights/Inst Low	0.668	0.006	0.233	0.715
Output High vs Rights/Inst High	0.118	0.996	0.206	0.263

Significance levels: ** p< 0.01, * p< 0.05. Results are based on linear regression with clustered standard errors on the country level. Pre-treatment individual-level controls: gender, age, education, urban, language proficiency, levels of democratic preferences, political interest, and turnout. Country-level controls: Level of liberal democracy and human development index.

O. Appendix: Heterogeneous treatment effects by individual traits

Figure A.15: Marginal effects (and 95% c.i.) of treatments by gender

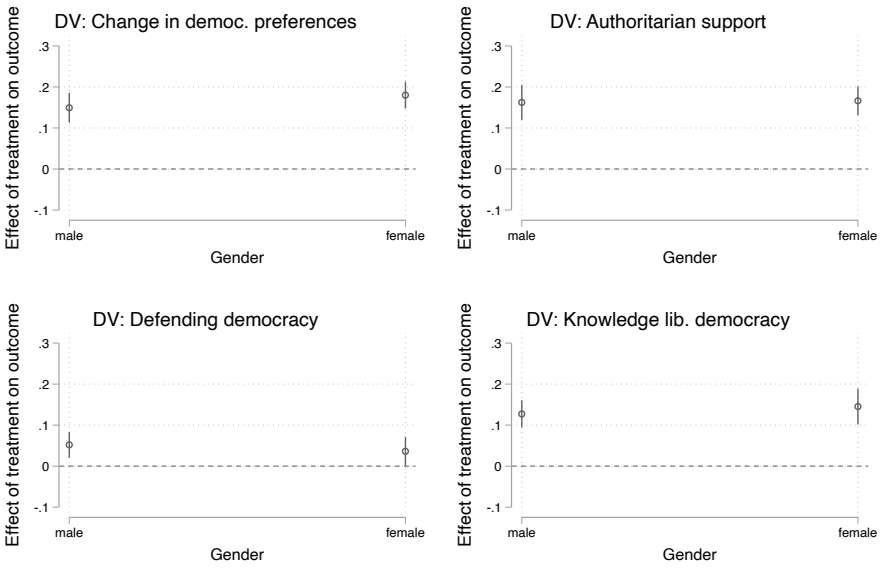


Figure A.16: Marginal effects (and 95% c.i.) of treatments by highest education

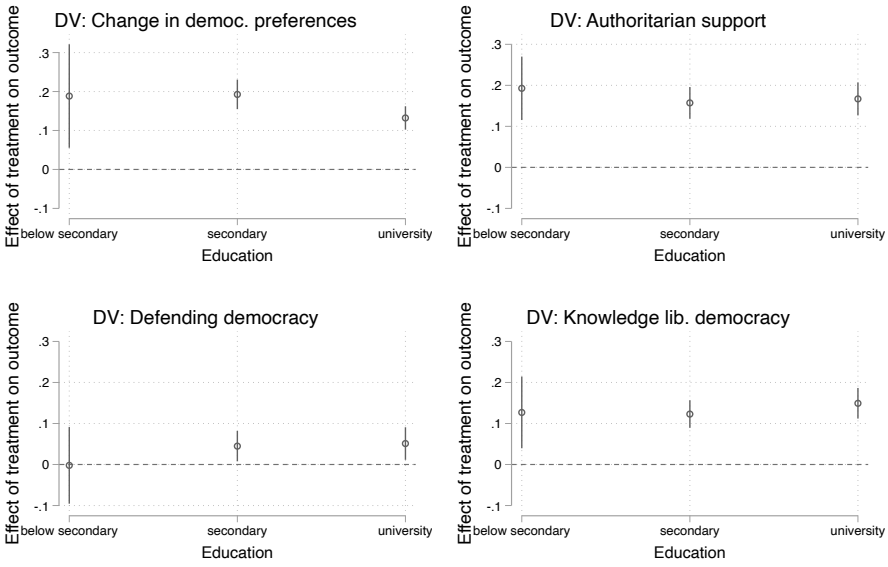


Figure A.17: Marginal effects (and 95% c.i.) of treatments by residence

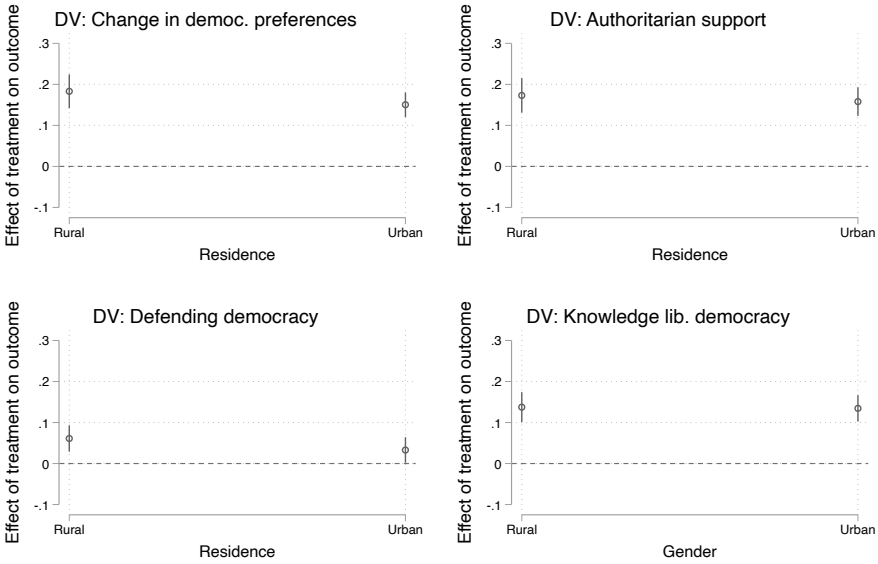


Figure A.18: Marginal effects (and 95% c.i.) of treatments by pre-treatment democratic pref.