The Long-Lasting Effects of Citizenship Education

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Abstract

Both policymakers and scholars disagree about the effects and suitability of citizenship education standalone courses. Extant evidence about their effects is mixed and inconclusive. In this paper we exploit the discontinuities generated by changes in Spanish education policies to identify the long-term effects of civic education on multiple outputs usually set as the policy goals of these courses: political engagement, institutional support, and political values. In 2007, a new standalone citizenship education subject was introduced in the Spanish school curricula. This subject was then progressively removed from the curricula until its disappearance in 2017. These changes gave rise to exogenous variation in exposure to civic education between young individuals born in different years. We exploit these policy changes to identify the effects of citizenship education through a regression discontinuity design that draws on a 12-wave panel survey. Our results point to the emergence of a generation of critical—yet passive—citizens as a result of the implementation of a standalone citizenship education subject in Spanish schools.

Keywords: Civic education; Citizenship education; Political engagement; Institutional Support; Political Values; Regression discontinuity design.

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INTRODUCTION

Since Langton and Jennings (1968) first addressed the effects of civic education courses, the literature has not settled for a definitive conclusion on their impact. For the last two decades, and in parallel with a growing concern about democratic backsliding and young people's disengagement, academics have renewed their interest on the potential of Citizenship Education (CE) to bridge the gap between politics and the young (Kerr, 2000; Niemi & Junn, 2005; Dassonneville, Quintelier, Hooghe and Claes, 2012; Torney-Purta J & Amadeo JA, 2013; Albacete, 2014; Whiteley, 2014; Nelsen, 2019). Citizenship Education, understood as the subject area that is taught in schools with the aim of fostering democratic and civic values and skills, has been shown to have some positive effects on political knowledge and civic engagement (Dassonneville et al. 2012; Neundorf, Niemi & Smets, 2016). Therefore, the current academic debate is not about whether CE works, but about the kind of CE that works.

Among the wide array of CE policies, one stands out for the scarce and mixed evidence with regards to its effects on political attitudes: CE standalone subjects. CE can be taught in schools as a set of transversal competences learned in different subjects (i.e. an "integrated approach") or as an independent subject (Keating et al., 2012). While some scholars do not see an advantage in the last strategy (Pike, 2007), others claim that, to maximize its potential effects, CE contents require a specific and independent subject in the school curriculum (Faulks, 2006; Hayward and Jerome, 2010). Moreover, studies on the effects of standalone CE subjects point in multiple and contradictory directions. Some evidence from the UK indicates that standalone CE courses have positive effects on political knowledge, efficacy and participation (Whiteley, 2014), yet no effects on political interest (Prior, 2018). At the same time, comparative research indicates that CE taught as a standalone subject has null, or even negative, effects on political engagement (Garcia-Albacete, 2013; Krzywosz-Rynkiewicz et al., 2017). Some of these contradictory findings could be the result of studies not differentiating between compulsory and elective CE subjects. Furthermore, the wide array of attitudes and behaviors on which CE is expected to have an impact leads to heterogeneity in the research objects examined, which hinders comparisons across studies.

Contradictory findings about the effects of CE are reflected in divergent policy decisions recently adopted by governments across Europe. France approved the reintroduction of compulsory standalone citizenship courses into the French educational system in 2015 in order to enforce republican values. Belgian authorities adopted a similar decision in 2017, including compulsory citizenship education courses in the French community's secondary schools. However, in Ireland, Cyprus and Norway this subject has been recently substituted by integrated approaches. Such a diverse landscape suggests that policymakers and practitioners will also benefit from empirical work analyzing the effects of CE standalone subjects (European Commission/EACEA/Eurydice 2017).

What are they beneficial for? Do they actually foster a democratic, civic, engaged and critical citizenship?

This paper contributes to the CE literature by examining the impact of a compulsory standalone CE subject (Educación para la Ciudadanía) taught in Spain between 2007 and 2017. The introduction, and later removal, of this subject from the Spanish school curriculum generated an ideal scenario to identify the causal effects of standalone CE courses. Our results, based on a regression discontinuity design and a unique 12-wave panel survey, reveal significant effects on a series of attitudes that point to the emergence of a generation of *critical citizens* as a result of the introduction of this subject. While this CE subject appears to slightly increase political efficacy and knowledge, it also makes citizens less trustful of representative institutions, less satisfied with the way democracy works and less attached to the Spanish nation. Indeed, the generation exposed to this CE subject does not seem enthusiastic about conventional politics, and the loss of allegiance towards traditional political institutions and the nation itself suggest that some of the subject's goals set by policymakers might have, unexpectedly, backfired. We elaborate further on the interpretation of these findings in the conclusion.

THEORETICAL FRAMEWORK: WHY IMPLEMENTING CITIZENSHIP EDUCATION AS A STANDALONE, COMPULSORY SUBJECT?

The literature on the effects of CE is divided in multiple strands that expect this type of education to have different "civic" effects. In the US, CE is mostly conceived as "the knowledge, skills, attitudes and experiences to prepare someone to be an active, informed participant in democratic life" (Campbell, 2012:1). In this context, CE had the initial purpose of reverting the decline in turnout among the young. As a result, American classic works have a broad understanding of CE. This encompasses in- and out-of-classroom educational civics experiences, which should have a positive effect on political participation, as well as "instructive" effects on political knowledge. These studies also have a recurrent interest on CE's equalizing effects, which should bridge the gap between children of different socio-economic backgrounds (Langton & Jennings, 1968; Nelsen, 2019). According to this perspective, we would expect CE to mostly have an effect on citizens' political engagement, and especially on how much citizens formally know about politics.

The contemporary European perspective is generally more limited in that it tends to focus on formal school curricula (i.e. in-classroom activities), but, at the same time it has a broader understanding of the potential "civic" effects of CE. For example, the 2017 Eurydice report on "Citizenship Education in Europe" defines CE as "the subject area that is promoted in schools with the aim of fostering the

harmonious co-existence and mutually beneficial development of individuals and of the communities they are part of." (Eurydice 2017: 9). This conception of CE is in line with the idea that these subjects might transcend the political realm and promote the acquisition of basic skills such as information collection, argumentation and reasoning, critical thinking, or being empathic when taking decisions (Heater, 2004).

Indeed, European studies on CE highlight its potential impact on the democratic and critical aspects of citizenship, aiming at preparing students to defend all citizens' rights and act in a socially responsible manner (Maurissen 2018). As a result, this literature considers that CE subjects can have an impact on a wider array of attitudes that can lead, in turn, to a critical and community-oriented citizenship (Whiteley, 2014). Hence, the impact of CE would not be limited to promoting individuals' political engagement. Based on this perspective, we would expect CE to lead to a better informed and empowered citizenry, fostering positive political values such as adherence to democratic ideals or tolerance towards minorities, as well as the capacity to independently exercise critical political judgment (see e.g. Eurydice 2017).

Given that contemporary studies on the effects of CE tend to consider a vast range of research outcomes, it is no wonder that most scholars agree that CE does have some effects. As a result, the main research concern on CE has shifted from analyzing whether it has any effects to assessing what type of CE "works" (Keating et al., 2012). Indeed, CE can be informally taught through extra-curricular activities -like service learning (Morgan & Streb, 2001)-, or it can be included in the school curriculum through: formal civic education, open classroom climates, and active learning strategies (Campbell, 2008; Neundorf et al. 2016). Moreover, formal civic education can be taught as a standalone independent subject or transversally in a cross-curricular or integrated mode that incorporates civic education to various courses and that, according to Dassonneville et al., (2012), increases political efficacy and political participation.[1]

Some evidence suggests that continuous and comprehensive education (i.e. an integrated approach) yields the best results when it comes to CE (Keating et al., 2010). According to this perspective, a transversal and integrated teaching of civics might help students see the relevance of citizenship to their personal lives, and will prevent that low-achiever students lose interest in a subject that is assessed just like any other (Pike 2007). Conversely, other researchers point out that the wide and complex contents of civic education require an established, specific, and independent subject in the

[1] The International Association for the Evaluation of Educational Achievement (IEA) has assessed the different approaches to CE. In its last study, "International Civic and Citizenship Education Study 2009" it distinguishes between a) CE taught as a separate subject, b) CE taught as a subject related to human and social sciences, c) as a competence or content integrated in all subjects taught at school, d) as an extra-curricular activity, e) as an outcome of school experience as a whole, and f) it is not part of the school curriculum.

school curriculum in order to convey importance and identity (Faulks, 2006; Hayward and Jerome, 2010). Claims for an independent CE subject rest on relatively scant empirical work, tough. As Geboers et al.'s (2013) meta-analysis indicates, the effects of CE standalone school subjects have been less frequently examined than other CE types such as classroom climates and extra- curricular activities. Moreover, some of these studies have returned different conclusions. A recent comparative analysis of fourteen European countries indicates that CE was most effective when delivered as a standalone subject at two levels of education—primary and secondary—, but only in regard to increasing patriotism (Krzywosz-Rynkiewicz et al., 2017). Using IEA-ICCS 2009 data to conduct a comprehensive comparative analysis of the effects of CE, García-Albacete (2013) finds that standalone subjects had a negative effect on political interest. The UK's standalone CE subject has been found, though, to have positive effects on political efficacy, political participation and political knowledge, but no effects on subscription to norms of civic conduct, political values or political interest (Prior, 2018; Whiteley, 2014). Yet, the British case poses some problems to identify the effects of CE, as schools were free to decide the content, mode and load of the teaching, causing substantial cross-school variation (Kerr et al., 2007).

In sum, the evidence about the effects of standalone CE is mixed. Moreover, the dialogue between the different literature strands is obscured by the multiplicity of research objects and expectations about CE outcomes. Our review of the literature also reveals some difficulties to isolate the specific feature of CE (whether or not it is offered as a standalone subject, whether or not it is uniformly implemented across schools, and whether it is mandatory or elective) that is responsible for such "civic" outcomes. Moreover, most empirical studies have analyzed whether CE has an effect on political attitudes and behavior only in the short-run. However, the scarce existing panel data suggest that formal civic education has positive lasting effects well after young citizens leave school (Neundorf et al. 2016). Our study contributes to this literature and addresses some of these shortcomings by assessing the long-term effects of a standalone compulsory CE subject, whose goals were explicitly defined in the laws and regulations that implemented the subject.

RESEARCH DESIGN

The Spanish case

In 2006 the Spanish Government, led by the Socialist Party (PSOE), passed a law (*Ley Orgánica de Educación*, LOE) that, for the first time, introduced CE as a compulsory standalone subject in the school curricula. Following the recommendation of the Council of Europe on education for democratic citizenship, the subject addressed issues related to tolerance, discrimination, cultural diversity and sustainable development, as well as political engagement and political support related topics. Before the approval of this law, Spain had an integrated and transversal approach to CE. Like

Scotland, Finland or Walonia at the time, there were some "civic" contents scattered across different subjects, but not a standalone CE subject.

According to the new law, the standalone compulsory subject called "Education for Citizenship and Human Rights" (EdC) was taught in Primary Education and Compulsory Secondary Education. The unfolding of the new policy was meant to be progressive and staggered, yet it was filled with obstacles. Spain is a decentralized state formed by 17 regions that have ample powers on education policies. This meant that regions had some leeway in the implementation of this new subject. This decentralized scenario became even more complex due to the strong objection to the subject by the main opposition party (the Popular Party (PP)). The PP obstructed its implementation, filing complains and lawsuits at the Regional Courts of Justice and the European Court of Human Rights. All in all, this meant that in regions governed by the PSOE or other left-wing coalitions EdC was first taught in secondary schools during the 2007-2008 school year , while in regions governed by PP its implementation was delayed until the 2008-2009 school year.[2] Moreover, depending on the region, EdC was taught either in the second or the third grade of secondary schools. In Appendix A we provide further details about the implementation of this new CE subject in Spain.

In 2011 the PP won the Spanish general election. By the end of 2013, the PP government passed a new education law (LOMCE) that eliminated CE as a standalone subject. The elimination of EdC from the school curricula was, again, staggered and heterogeneous across the territory, with some regions teaching EdC until the 2015-2016 school year and others until 2016-2017 (see Table A1 in the Appendix).

Thanks to these policy shifts, the Spanish case provides an ideal scenario to isolate the causal effects of standalone CE subjects. Our expectation, based on the aims explicitly defined in the law that introduced CE in Spain and the previous literature discussed above, is that being exposed to that subject should have a long-lasting positive effect on attitudinal outcomes related to political engagement, institutional support, and civic values. That is, significant differences in these attitudes should be observable between cohorts that had to take CE courses and those that did not. The benefit of focusing on this single case is, precisely, that we can narrow the specific outcomes that might be affected by CE to those that policymakers intended to influence by implementing this policy (see below).

[2] The exception to this pattern was the region of Castilla la Mancha. Although governed by PSOE, in this region EdC was first taught during the 2008-2009 school year.

Identification Strategy

The identification of the attitudinal and behavioral effects of CE poses multiple challenges. First, there are a number of individual factors (e.g. SES, motivation, intelligence) that predict educational attainment, the scope of schooling effects (i.e. how beneficial schooling is for a particular individual) and civic outcomes. Some of these confounders are particularly difficult to measure and, if unmeasured, they are likely to bias any estimate of the effects of CE. Second, analyzing the effects of CE by comparing its effects across countries is subject to similar limitations related to differences between countries and school systems that cannot be controlled away effectively. An alternative is to capitalize on changes in education policies that generate a quasi-experimental scenario that can be exploited to identify the effects of CE.

In Spain, students are assigned to a school grade based on the calendar year when they are born. Combined with the changes in CE polices described above, this generates variation in exposure to CE across birth cohorts. For example, in most regions, those born in December 1992 did not take any CE course, while those born in January 1993 took one CE course (in secondary school). Crucially, being born in December 1992 or January 1993 is likely to occur as-if random. That is, these two groups will be similar on most accounts, except for the fact that the latter took a standalone CE course while the former did not. This is the logic underlying the method that we implement in this paper: the regression discontinuity design (RDD). Through the RDD we estimate the effects of CE by comparing the attitudes of those barely above and below a known cutoff or threshold: the birthdate corresponding to the school year when CE was introduced in the school curriculum in each region.[3]

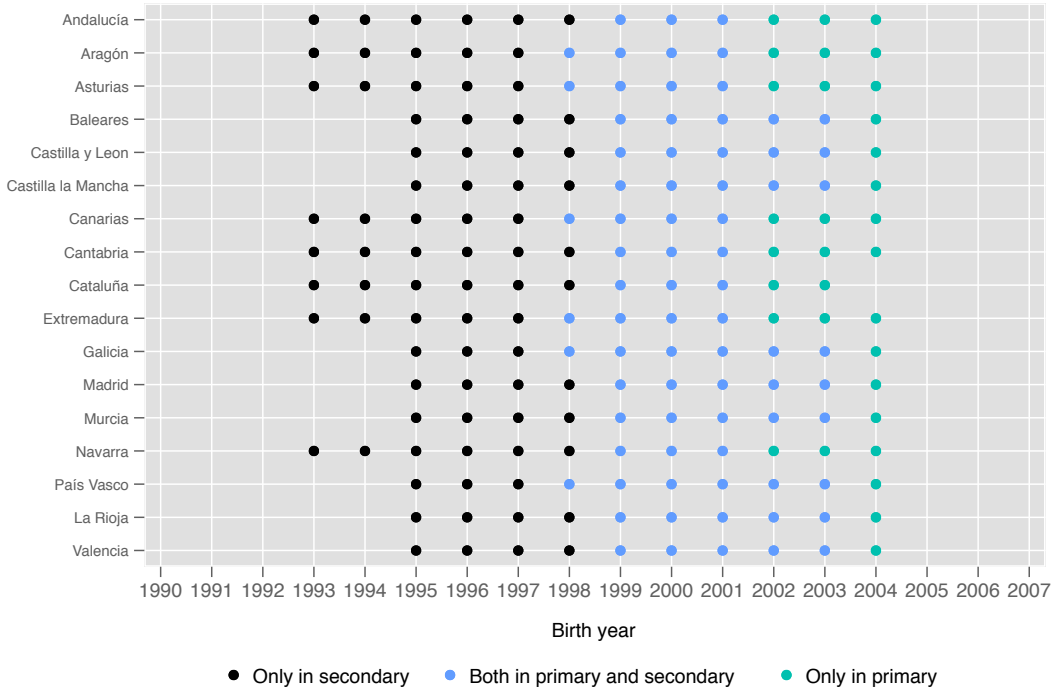
The key cutoff in our RDD is defined by the grade and year when CE was first implemented in each region. For example, according to the data summarized in Table A1 in the Appendix, Catalonia first introduced CE in secondary education in the 2007-2008 school year as a third-grade subject. This means that the cohort born in 1993 was the first one to study CE in Catalonia. We use this cutoff to operationalize our running variable x for each individual i, which, based on the birthdate of each respondent, determines whether or not she studied CE and how far from the threshold (in days) she is. Specifically, the running variable x for individual i is computed as follows:

$$x_i = birth_i - first_{ir} \qquad Eq.1$$

[3] This identification strategy has been used to assess the effects of education reforms on voting (Lindgren et al., 2019), as well as the effects of CE policies on political interest (Prior, 2018).

where $birth_i$ is respondent's i birthdate and $first_{ir}$ is the date that corresponds to the birthdate of the first cohort that studied CE in individual's i region r. Since in Spain students are assigned to a school grade based on the calendar year when they are born, January 1st marks the threshold to be assigned to one grade or another. Therefore, $first_r$ always takes the value of January 1st of the birth year that corresponds to the first cohort that studied CE in a given region. Take again as an example the case of Catalonia. To compute the running variable we calculate the difference in days between the birthdate of each Catalan respondent in our sample and January 1, 1993 (the birth year corresponding to the first cohort that studied CE in this region). Figure 1 summarizes the educational stages at which individuals born in different years studied EdC in each region. The first black dot in each row provides us with the date of $first_r$ that we subtract to the birthdate of respondents in each region in order to generate our running variable.[4]

Figure 1: Educational stages at which individuals studied CE by birthdate and region



Note: Birth years with no markers indicate that individuals born on those years did not follow any CE course

The resulting running variable x takes positive values for those who studied CE and negative values for those who did not. The value of 0 is assigned to the individuals born on January 1st, who studied CE during the first year when that subject was implemented in their region. Since our sample does not include individuals born after 2002, all those respondents at the right of the cutoff (x_i values equal or higher than 0) studied CE (i.e. there are no respondents at the right of the cutoff who did not

[4] To calculate the running variable respondents are assigned to the region where they currently live. A limitation of this operationalization is that respondents might not be currently living in the same region where they attended school. However, we consider this to be unlikely given the low rates of interregional mobility in Spain (Maza and Villaverde, 2004).

study CE, because those born in 2004 were the last cohort exposed to this subject). Specifically, 432 respondents in our sample took at least one EdC course. Figure B1 in the Appendix summarizes the distribution of the running variable.

Treatment status D_i (having studied CE or not) for each individual is, therefore, a deterministic and discontinuous function of the running variable x_i.[5] Hence, we use a sharp RD specification and we adopt a continuity based RD framework (Cattaneo et al., 2019). This implies that our estimates are based on analyzing whether or not there is a discontinuity (jump) in the outcome variables at the threshold of the running variable ($x = 0$). Specifically, all our results are based on a local polynomial regression discontinuity estimation with bias corrected confidence intervals (Cattaneo et al., 2019).[6] Hence, we estimate the regression functions on each side of the threshold using only observations close to the threshold and a low-order (linear) polynomial approximation, since this has been shown to be less sensitive to overfitting problems (Cattaneo et al., 2019). In other words, our estimation does not include all those respondents above and below the threshold. It is based on a local linear estimation around a pre-selected optimal bandwidth on each side of the threshold, which "delivers a good tradeoff between simplicity, precision and stability in RD settings" (Cattaneo et al., 2019, p. 38).

A key decision is, therefore, how to select the bandwidth that determines which units below and above the threshold ($x = 0$) are included in the estimation of the regression functions. We follow Cattaneo et al., (2019) and rely on the data-driven mean square error (MSE) optimal bandwidth selector (for a common bandwidth on each side of the threshold).

All our RD estimates account for the panel structure of our data (see below) through the use of standard errors clustered at the respondent level. Moreover, in the RD estimations of the effects of CE we include as additional covariates the region where the respondent lives and an indicator variable capturing whether she studied CE in primary school.[7] While Table B2 in the Appendix reveals that there are no regional imbalances at the threshold, there might be some differences in how CE was implemented in each region. These covariates account for these potential differences.

[5] That is, $D_i = 1$ if $x_i \geq 0$ and $D_i = 0$ if $x_i < 0$.

[6] The estimation is based on the *rdrobust* Stata package developed by Cattaeno et al., (2019).

[7] In Spain CE was implemented in both primary and secondary school. However, our sample only includes individuals born before 2003. As a consequence, our sample only includes 74 respondents who studied EdC in primary school (and 73 of those also studied EdC in secondary school).

Data and variables

Our analyses draw on data from twelve waves of the "Spanish Political Attitudes Panel Dataset" (POLAT), an online panel survey fielded in Spain between 2010 and 2020 through the commercial firm Netquest. The survey sample is restricted to Spanish citizens born between 1951 and 2002 who have internet access. Our sample includes 20,963 observations from 4628 different respondents.[8] Unlike most surveys, the POLAT panel records the exact birthdate of all respondents, which is a key requirement of our identification strategy. Moreover, the POLAT survey includes numerous questions about respondents' political attitudes that tap on CE's multiple goals and outcomes

In this respect, the 2009 International Civic and Citizenship Education Study (ICCS) from the IEA considers seven processes and 12 topics on which CE can have an impact. Table 1 depicts the match between the goals of the Spanish CE subject, structured according to the ICCS processes and topics distinction, and the outcome variables analyzed in this paper. Through the POLAT panel we cover six out of the seven process-related goals and eight of the 12 topic-related aims. The selected variables tap the most important aims of CE in Spain according to the ICCS 2009 report, and hence should capture the effects of the subject, if there are any. Note that we do not address behavioral outcomes, because these are contingent on many other factors such as resources, mobilization, or context (Verba et al., 1995).

These dependent variables are grouped into three categories: subjective political engagement (political efficacy, political interest, political discussion, political knowledge); institutional support (sense of duty to vote, political trust, satisfaction with democracy); and values (Spanish nationalism, attitudes towards freedom, attitudes towards gays, and attitudes towards migrants). Note that some questions (political discussion, attitudes towards freedom, or satisfaction with democracy) have only been asked in a limited number of waves of the panel dataset, which restricts the number of observations available when analyzing these variables.[9]

[8] Further details about the survey and its sampling procedures can be found in Appendix B.

[9] Further details on the wording of survey questions and the distribution of all the variables can be found in Appendix C.

Table 1. Dependent variables and correspondence with CE official aims

IEA/ICCS REPORT: CE AIMS	POLAT panel survey indicator	Measurement and operationalization
Processes		
Knowledge and understanding of civics and citizenship; which includes knowing basic facts, understanding key concepts, values and attitudes	• Political knowledge • Sense of duty to vote	• 0-1 political knowledge index based on the proportion of correctly answered knowledge questions asked in each panel wave (higher values indicate higher knowledge) • 0=Voting is a choice, 3=Voting is a duty, very strongly
Communicating through, which considers discussion and debate; projects and written work.	• Political discussion	• Additive scale (0-6) resulting from adding the frequency of political discussion (0=never, 1= sometimes, 2=often) with your best friend, family and coworkers
Creating opportunities for students' involvement in decision-making in school & community-based activities		
Analyzing and observing change process in both the school & the community	• Satisfaction with democracy	• How satisfied are you with the way democracy works? 0=completely unsatisfied ,10= completely satisfied
Reflecting on and analyzing participation and engagement opportunities	• Sense of duty to vote	• See above
Developing a sense of national identity and allegiance	• National identity • Political trust • Satisfaction with democracy	• 0= does not feel attached to the nation at all, 10=feels attached to the nation, very much. • Additive scale (0-10) of trust in parliament, government, parties and politicians • See above
Developing positive attitudes toward participation & engagement in civic & civil society.	• Political interest • Political efficacy • Satisfaction with Democracy	• 0=not interested at all, 3=very much interested • 0-10 index resulting from adding two questions: (i) I (don't=0) understand (very well=10) what's going on in politics; (ii) I (never=0) (always = 10) have opinions about political issues. • See above
Topics		
Human rights	• Attitudes towards homosexuality • Attitude towards freedom	• Child adoption by homosexual couples should be 0=forbidden, 10=allowed • 0=freedom should be prioritized over order, 10= order should be prioritized over freedom
Legal system and courts	• Political knowledge • Attitude towards freedom • Satisfaction with Democracy	• See above • See above • See Above
Understanding different cultural and ethnic groups	• Attitudes towards immigrants	• Immigrants having the same culture than us is 0=not important at all, 10=very important
Parliament and government systems	• Political know. • Political trust	• See above
Voting and elections	• Duty to vote	• See above
The economy and economics	• Political knowledge	• See above
Voluntary groups		
Resolving conflict	• Political discussion	• See above
Communication studies (e.g. the media)		
The global community and international organizations	• Political knowledge	• See above
Regional institutions and organizations		
The environment		

RESULTS

Before analyzing the effects of CE we conduct a key test to validate our identification strategy: whether, around the threshold, treated and untreated units are similar on characteristics that should not be affected by CE (Cattaneo et al., 2019). For this purpose, we test whether there is any discontinuity in 34 outcomes: a set of predetermined covariates (respondents' gender and age, whether the respondent attended public or private school, and multiple parents' characteristics and behaviors), and placebo outcomes (size of municipality where the respondent lives, her SES, income, level of education, religiosity, ideology, and identification with Spain's main parties). The results (Appendix Table B1) reveal that all RD estimates are small and not statistically significant (average of p-values = 0.56). Additional tests also indicate that there are no noticeable discontinuities in respondents' region of residence (Appendix Table B2). This indicates that, as expected, individuals around the threshold are similar.

Tables 2-4 and Figures 2-4 summarize the results of the RD models that test the effects of CE on each of the 11 outcomes variables.[10] We report the conventional RD estimator and p-value along with the robust bias-corrected p-value. The tables also report the size of the selected bandwidths (in days), as well as the number of observations that fall within that bandwidth.

Table 2 summarizes the effects of EdC on political engagement. In the case of political interest, the RD estimate is substantively small and clearly not distinguishable from 0. In the cases of political efficacy, political discussion, and political knowledge, Figure 2 reveals that, at the threshold, there appears to be a positive impact of EdC. For political discussion, the effect is substantively strong: the positive increase caused by EdC (1.41) is equivalent to a change of 0.93 units in the standard deviation of this variable. However, the reduced number of observations available for this variable, which is only included in one wave of the POLAT dataset, makes this estimate quite imprecise and not distinguishable from zero. In the case of political interest, we observe a weaker positive effect (0.65), equivalent to an increase of 0.47 units in the standard deviation of this variable. However, this estimate is also quite imprecise, since both types of p-values are very close or even higher than 0.10. In the case of political knowledge, the estimate of the positive effect of EdC is slightly more precise, with both conventional and robust bias-corrected p-values consistently below 0.10 (but above the 0.05 threshold). The positive increase of 0.09 units in the political knowledge index caused by EdC is of moderate magnitude, since it only amounts to 0.27 units in the standard deviation of this variable. Overall, it seems that EdC has a slight positive effect on some variables related to

[10] In all figures solid lines are first order (linear) polynomials fitted separately on the common bandwidths at each side of the threshold. Dots represent bin averages of the outcome variable. The number and size of bins are selected through the mimicking variance evenly-spaced method. Note that the polynomials are fitted using the original observations (raw data), not the binned ones.

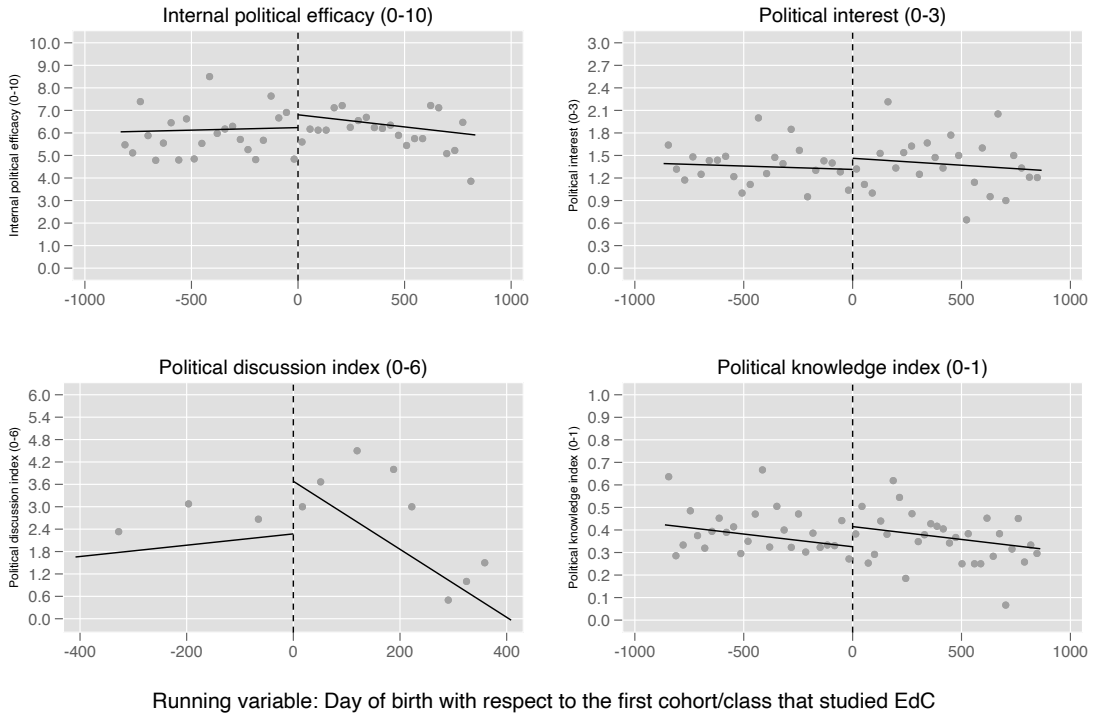
political engagement, although in most cases our inferences are limited due to the high uncertainty surrounding these estimates of the effects of CE.

Table 2: Subjective political engagement variables

	Political efficacy	Political interest	Political discussion	Political knowledge
RD estimate	0.65	0.15	1.41	0.09
	(0.39)	(0.16)	(0.80)	(0.05)
p-value (conventional)	0.09	0.34	0.08	0.07
p-value (robust bias-corrected)	0.10	0.39	0.11	0.08
Bandwidth (days)	989.33	867.84	408.36	861.71
Effective number of observations	1361	1213	47	1203

Note: Standard errors adjusted for clustering at the respondent level in parentheses. Covariate-adjusted estimates.

Figure 2: Subjective political engagement variables



When it comes to institutional support, Table 3 reveals that following an EdC course erodes this type of support, but, at the same time, it has no significant effect on the sense of the duty to vote. The RD estimate indicates that taking a CE course does not make citizens more or less likely to think that voting in elections is either a duty or a choice. However, those who took an EdC course are clearly less trustful of representative institutions (such as the parliament or political parties), as well as less satisfied with how democracy works in Spain. For these two estimates, the identified negative effects of EdC are statistically significant at conventional levels, both when considering conventional and robust bias-corrected p-values. Figure 3 illustrates the substantial negative jump in political trust and

satisfaction with democracy observable at the threshold. The negative RD estimate for political trust is equivalent to a change of -0.68 standard deviations in this variable, while the negative effect on satisfaction with democracy caused by studying EdC corresponds to a reduction of 0.56 standard deviations in this variable. These results indicate that EdC clearly makes citizens more critical by reducing their trust in political institutions and their satisfaction with the way democracy works in Spain.

Table 3: Institutional support variables

	Duty to vote	Political trust	Satisfaction with democracy
RD estimate	-0.07	-1.37	-1.56
	(0.26)	(0.41)	(0.53)
p-value (conventional)	0.80	0.00	0.00
p-value (robust bias-corrected)	0.85	0.00	0.00
Bandwidth (days)	839.67	673.79	826.41
Effective number of observations	998	748	625

Note: Standard errors adjusted for clustering at the respondent level in parentheses. Covariate-adjusted estimates.

Figure 3: Institutional support variables

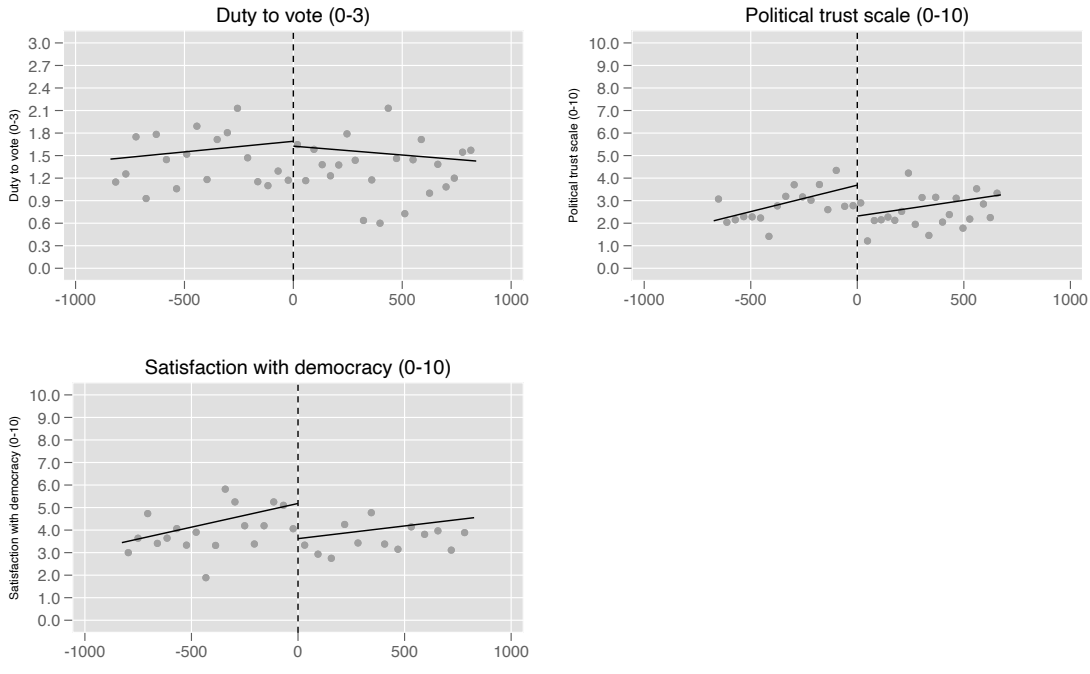


Table 4 and Figure 4 summarize our findings about the effects of CE on citizens' values. The results indicate that, at the threshold, those who took a compulsory EdC course became less attached to the Spanish nation and more likely to think that freedom should be prioritized over order. For these two outcome variables the RD estimates are statistically significant both when considering the

conventional and the robust bias-corrected p-values. Moreover, in both cases the magnitude of the effects of EdC are substantial. In the case of Spanish nationalism, the estimated effect of EdC equals -2.64 in a 0-10 scale, which is equivalent to a change of -0.66 standard deviations in this variable. Similarly, when it comes to respondents' preferences about the freedom vs. order tradeoff, which is also measured from 0 to 10 with 0 indicating a strong preference for freedom, the RD estimate equals -2.47. This estimate is equivalent to a change of -0.73 standard deviations in this variable.

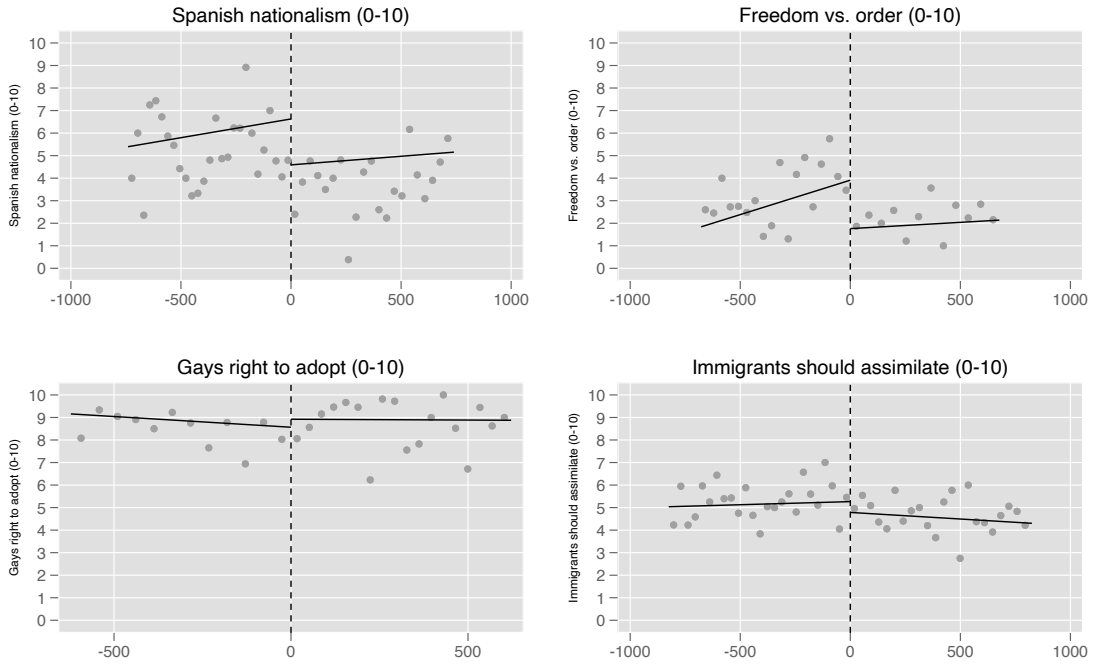
EdC, therefore, has a strong negative effect on individuals' attachment to the Spanish nation and clearly increases the likelihood of prioritizing freedom over order. However, EdC does not appear to affect other values such as individuals' attitudes towards gays and immigrants. The RD estimate indicates that those who studied EdC are slightly more likely to think that gay couples should be allowed to adopt children. However, this RD estimate is of reduced magnitude and not statistically significant. Similarly, the RD estimate about the effects of EdC on the belief that immigrants should adapt to the culture of the county of destination is of reduced magnitude and not statistically significant neither when using a conventional nor a robust bias-corrected p-value.

Table 4: Values

	Spanish nationalism	Freedom vs order	Gay adoption	Immigrants should assimilate
RD estimate	-2.04	-2.15	0.36	-0.48
	(0.59)	(0.66)	(0.56)	(0.39)
p-value (conventional)	0.00	0.00	0.53	0.21
p-value (robust bias-corrected)	0.00	0.00	0.37	0.34
Bandwidth (days)	740.09	677.34	621.89	824.95
Effective number of observations	996	521	744	1142

Note: Standard errors adjusted for clustering at the respondent level in parentheses. Covariate-adjusted estimates.

Figure 4: Values



Regional heterogeneity and robustness checks

We have conducted multiple additional analyses to asses the robustness of our findings. First, as we point out above, regions had some leeway on the implementation of the new CE subject. This led to differences on the exact grade in which EdC was taught in primary and secondary education, as well as to differences in the school year when the new subject was implemented. Generally, the conservative opposition party, the PP, attempted to obstruct the implementation of the new subject in the regions it was governing by, for example, delaying for one year its implementation (see Appendix A). This might generate differences in the effects of EdC across regions. In order to explore these potential differences, we have re-estimated all our models distinguishing between *early adopting* regions, mostly ruled by left-wing parties and where EdC was first taught in the 2007-2008 school year, and *late adopting* ones, generally ruled by conservative parties and where EdC was first taught in 2008-2009. The results, summarized in Figure B2 in the Appendix, do not reveal any relevant differences in the effects of EdC between these two groups of regions.

Second, there are multiple types of RD estimates that can be computed (Cattaneo et al., 2019). In Figure B3 in the Appendix we summarize the results of all of our RD specifications reporting three types of estimates for each of them: conventional, bias-corrected, and robust bias-corrected. All these different estimates lead us to the same conclusions.

Third, a key falsification test of the RDD is testing for treatment effects on the outcome variables at alternative (placebo) cutoffs of the running variable, where such an effect should not exist (Cattaneo et al., 2019). We follow Imbens and Lemieux (2008) who recommend testing for discontinuities at the empirical median of the control and treatment groups, since this increases the statistical power of these robustness checks. Therefore, using only observations on either side of the cutoff, we test for discontinuities at values $x = -6136$ and $x = 852$, Figures B4-B6 in the appendix summarize the results of these placebo tests. If we focus on those outcome variables for which we identified an effect of EdC (political efficacy, knowledge, trust, satisfaction with democracy, Spanish nationalism, freedom vs. order), none of the RD point estimates of the placebo cutoffs equals or is larger than the original estimate obtained with the true cutoff ($x = 0$), and none of these estimates is statistically significant either at the five or ten percent levels.

DISCUSSION

Scholars do not agree on the effects of CE standalone subjects. As a result, some policymakers keep on deploying these subjects as a way to foster a harmonious co-existence, along with a critical, engaged, tolerant, informed and responsible citizenry. At the same time, other nations are reverting to integrated approaches as a more effective path to achieve these goals. In order to shed some light on this matter, this paper exploits a quasi-experiment generated by the implementation of a new CE standalone subject in Spain.

Following previous studies on CE and the ICCS reports, we have selected a series of outcome variables that tap into a variety of the goals of the Spanish CE subject. We have grouped them in three categories: political engagement, institutional support, and political values. As for the first category, it seems that the Spanish CE subject had a slight positive effect on some variables related to political engagement. We identify a small increase in individuals' political efficacy and political knowledge as a result of this CE subject, but these inferences are limited by the high uncertainty surrounding our estimates of these effects. In contrast, most of the institutional support variables seem to be clearly affected by CE. Those exposed to this subject are significantly less trustful and less satisfied with the way democracy works. All in all, this paints a critical portrait of Spanish CE students, which seem alienated from the political system, and particularly from representative institutions. Finally, CE had an effect on two of the four values analyzed. For a start, individuals have about the same attitudes towards minorities (migrants and gay couples), no matter whether they took a standalone CE course or not. Yet those exposed to the new CE subject turn out to be more "libertarian" and also less attached to the Spanish nation.

Some of these results are at odds with Whiteley's (2014) study of CE in the UK, since we do not find an effect on political interest. We do, however, identify some effects on political values: individuals

become more "civic" in the sense that they are fonder of freedom and unwilling to trade it for more order. When it comes to political interest, though, our findings are in line with those of the most recent study by Prior (2018), who did not identify any effects of CE on political interest in the UK. Moreover, conversely to what Krzywosz-Rynkiewicz, et al. (2017) found, the Spanish CE subject not only does not foster nationalism, but undermines it. Indeed, youngsters having taken the course feel less attached to their nation than the rest.[11]

These results clearly clash with some of the goals of CE that practitioners considered a priority: citizens may have become more critical as a result of the subject, but in a seemingly unproductive way, as they seem alienated from the political process and not particularly interested in politics. The only remarkable outcome in this respect is the increase in their political knowledge and in their subjective feeling that they are able to understand what is going on in politics. However, our evidence on the positive effects on these particular variables is less conclusive. Moreover, the course failed to foster national allegiance and any sort of political support. Given the number of non-significant results and findings that go against some of the expected goals of the policy, we can claim that the policy has, to some extent, backfired, at least in the long-run.

From a more optimistic standpoint, though, one could think that this CE subject led to the emergence of a generation of *critical citizens* (in the optimistic sense that Norris (1999) defined these citizens). One of the aims of CE is to enhance individuals' capacity to independently exercise critical political judgment. Hence, the reduction in political trust or satisfaction with democracy could also be a reflection of this enhanced capacity. This interpretation is rendered more credible if we take into consideration the Spanish context during this particular period, which was characterized by low confidence in political institutions due to, among others, salient corruption scandals (Ares & Hernández, 2017), and a profound and long-lasting economic crisis. The negative impact of CE on these political support variables might be, therefore, taken as an indication that the new CE subject worked. In this rather negative political context, it is fair to assume that an enhancement of one's capacity to process and understand political information and exercise political judgment could lead to a reduction in institutional support. However, a more robust effect of CE on variables related to political engagement would make us more confident about this rather optimistic conclusion.

In any case, more data is required to determine whether the quantity of courses taken (one, two or three in the Spanish case) has a robust impact on the dependent variables considered in this research,

[11] Tony Venables for Bundeszenttrale für politische Bildung (12/12/ 2018) states: "From reading the academic literature about European citizenship education […], the way citizenship education is articulated appears less nationalistic and gives more scope to universal values and critical thinking and to therefore" According to this approach, what we find in Spain is actually consistent with a general European trend in CE.

and whether such effect varies if individuals are exposed to civic contents early (primary education) or later (secondary education). Once data on the attitudes of the youngest generations, who only studied CE in primary school, becomes widely available it will be possible to analyze these differences in greater detail.

Future research should also tackle an intrinsic limitation of our study: the local nature of our results. We have estimated the effects of CE among a particular subset of the Spanish population, who were born in the period comprised by the bandwidths of our RD estimations. These effects might be different in other cohorts socialized in a different period. Moreover, the implementation of the CE subject in 2007 in Spain was characterized by a high politicization around the subject and a particular context, marked by the Great Recession and a succession of salient corruption scandals. It is possible that the same civic education contents implemented in a different (more positive) context might yield different results. This is especially so, when it comes to the effects of CE on attitudes related to institutional support. If CE enhances individuals' capacity to exercise critical political judgments, its impact on attitudes such as trust in political institutions, might depend on the actual performance of these institutions. In countries with good/bad institutional performance the effects of CE on these variables might be positive/negative. In fact, if CE standalone subjects amplify the effects of the political context, the Spanish case may serve as a cautionary tale for practitioners and politicians, who might want to avoid implementing a CE subject when the political system is underperforming.

Further research should also consider whether CE might have heterogeneous effects depending on individuals' upbringing and primary socialization. Recent studies conducted in other contexts indicate that CE has stronger effects among young people less exposed to political information at home (Campbell & Niemi, 2016; Neundorf et al., 2016). This is a relevant and fruitful avenue for further research since these heterogeneous effects might imply that CE can reduce political inequalities.

It would also be advisable to explore behavioral outcomes to see if any effects go in the same direction as the attitudinal effects identified in this paper. Yet, all the signs indicate that standalone CE subjects, at least the one deployed in Spain between 2007 and 2013, has achieved its goal of fostering some critical attitudes among citizens. However, the subject might have also led to the development of some passivity among citizens, turning them into dissatisfied, disaffected and suspicious spectators of the political process.

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Supporting information for:

The Long-Lasting Effects of Citizenship Education

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APPENDIX A: IMPLEMENTATION OF CIVIC EDUCATION IN SPAIN

The LOE (2006) law[1] established the main objectives of the new standalone CE subject: "to promote the development of free and upright people through nurturing self-esteem, personal dignity, freedom and responsibility and the formation of future citizens with discretion, respect, equity and solidarity (…) they know their rights, assume their duties and develop civic habits so that they can effectively and responsibly exercise citizenship".[2] Indeed, according to the ICCS 2009 report, the most important aim of this subject was "promoting students' critical and independent thinking" (67% of the interviewed teachers agreed), followed by "promoting knowledge of citizens' rights and responsibilities" (61%) and "developing students' skills and competencies in conflict resolution" (57%).

According to the LOE, the standalone compulsory subject called "Education for Citizenship and Human Rights" (EdC) was taught in some grades in Primary Education and Compulsory Secondary Education. As for Primary Education (6 to 12 years), children were taught CE in one course (either at 11 or at 12 years old), under the name of "Education for Citizenship and Human Rights", one and a half hours per week, up to 50 hours a year. In Compulsory Secondary Education (12 to 16 years), two subjects on CE were included in the curricula. For a start, kids had to take one CE course during one of the three first courses of secondary school (13- 15 years), under the name "Education for Citizenship and Human Rights", one hour per week up to 35 hours a year. Moreover, in the fourth and last year of Compulsory Secondary Education (16 years) the subject changed its name to Ethical-Civic Education, and was taught for one hour per week, which makes a total of 35 hours a year.

The unfolding of the new policy was meant to be progressive and staggered, yet it was filled with obstacles. Spain is a decentralized system where regional authorities get to decide a great share of the school contents and curricula. Even if the minimum common contents of the new CE subject were fixed by the Spanish government, regions could decide when to start teaching CE (within a limited time frame) and also whether they wanted to go beyond the minimum contents established

[1] The new Education Act was published as a Royal Decree (RD 1631/2006, December 29th).

[2] In the past, Spain had several experiences of moral values teaching. Different regulations between 1837 and 1850 instructed the teachers to teach their pupils "urbanity" (for which there were manuals that persisted even in the time of the second Republic, 1931-1939), tolerance and mutual benevolence, docility, morals, religion and obedience. The educational reform of 1926 included an oral examination of urbanity for elementary students who wanted to enter the baccalaureate. Francoism reinstated the Urbanity subject in the Primary Education Act (1940). Francoism also included a compulsory subject in the baccalaureate called "Formation of the National Spirit", which was taught with specific and differentiated handbooks. Some of the most common school encyclopedias of the time included books such as "Political-Social Training (boys)" and "Political, Family and Social Formation (girls)". The subject persisted until the enactment of the General Education Law of 1970 (González, 2014). For a short period of time, during the transition to democracy (1978-1982), the subject called "Living together in democracy" included civic and political content in the last year of primary education, at the age of 13 and 14. Finally, a new education law (LOGSE, 1990) introduced the concept of transversal education of values in all subjects of the school curriculum (García, 2011).

by the government. This decentralized scenario became even more complex due to the strong opposition regarding moral issues between the two main parties, the social democrats PSOE -the driving agents of the CE subject- and the conservative, center-right Popular Party (PP). The PP obstructed its implementation, filing complains and lawsuits. As a result, regions ruled by the PP delayed the implementation of EdC as much as possible (Burchianti and Zapata-Barrero, 2016). [3]

Following the controversy, only half of the Spanish Autonomous Communities had implemented the CE subject by the start of the 2007-2008 course. The pioneering regions, ruled by left-wing parties or coalitions, were Andalusia, Aragon, Asturias, Cantabria, Catalonia, Extremadura and Navarre. Conservative-ruled regions (Madrid, Castile-Leon, la Rioja, Murcia and Valencia, among others) started to implement it in the 2008-2009 school year. Moreover, there were also some minor differences across regions when it comes to the specific school grade in which EdC was taught.

In the following table we summarize all the details about the implementation of EdC in each Spanish region. Specifically, the table summarizes the grades in which EdC was taught in primary and secondary school, the years when EdC was first taught at each educational stage, the years when EdC was last taught at each educational stage, as well as the regional law that regulates the implementation of EdC in primary and secondary schools in each region. This is the information we used in order to define whether individuals were exposed to EdC or not, and how far from the cutoff they are.

The information summarized in the table is based on the analyses of the relevant laws in each region. In those cases in which the laws did not provide all the details we required (e.g. the exact grade at which EdC was taught in secondary school) we directly contacted the relevant authorities to gather that information.

[3] The content that was deemed inappropriate by these opposing agents was mostly related to gender and families (see Burchianti and Zapata-Barrero, 2016). In this research we analyze attitudes towards homosexuals but do not consider feminism precisely because we fear that the content taught on this respect might not be homogenous across “abiding” and “rebel” regions.

Table A1: Details about the implementation of EdC in primary and secondary schools in each Spanish region

Region	Grade in which EdC was taught in primary school	Grade in which EdC was taught in secondary school	First school year EdC was taught in primary	First school year EdC was taught in secondary	Last school year taught in primary	Last school year taught in secondary	Regional law regulating EdC in primary school	Regional law regulating EdC in secondary school
Andalucia	5	3	2009-2010	2007-2008	2014-2015	2015-2016	Decree 230/2007	Decree 231/2007
Aragón	6	3	2009-2010	2007-2008	2015-2016	2015-2016	Order from may 9 2007, from the Education, Culture and Sports department.	Order from may 9 2007, from the Education, Culture and Sports department.
Asturias	6	3	2009-2010	2007-2008	2015-2016	2015-2016	Decree 56/2007	Decree 74/2007
Islas Baleares	5	2	2009-2010	2008-2009	2015-2016	2016-2017	Decree 72/2008	Decree 73/2008
Canarias	6	3	2009-2010	2007-2008	2015-2016	2016-2017	Decree 126/2007	Decree 127/2007
Cantabria	5	3	2009-2010	2007-2008	2014-2015	2015-2016	Decree 56/2007	Decree 57/2007
Castilla la Mancha	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 68/2009	Decree 69/2007
Castilla y Leon	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 40/2007	Decree 52/2007
Cataluña	6	3	2009-2010	2007-2008	2014-2015	2015-2016	Decree 142/2007	Decree 143/2007
Extremadura	6	3	2009-2010	2007-2008	2015-2016	2015-2016	Decree 82/2007	Decree 83/2007
Galicia	6	2	2009-2010	2008-2009	2015-2016	2016-2017	Decree 130/2007	Decree 133/2007
La Rioja	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 26/2007	Decree 23/2007
Madrid	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 22/2007	Decree 23/2007
Murcia	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 286/2007	Decree 291/2007
Navarra	5	3	2009-2010*	2007-2008	2015-2016	2015-2016	Foral decree 24/2007	Foral decree 25/2007
País Vasco	6	2	2009-2010	2008-2009	2015-2016	2016-2017	Decree 175/2007	Decree 175/2007
Valencia	5	2	2009-2010	2008-2009	2014-2015	2016-2017	Decree 111/2007	Decree 112/2007

APPENDIX B: SAMPLE DETAILS, ADDITIONAL RESULTS, ROBUSTNESS CHECKS AND FALSIFICATION OF THE RDD.

FURTHER DETAILS ABOUT THE POLAT PANEL SURVEY

Respondents of the "Spanish Political Attitudes Panel Dataset" (POLAT) were recruited by the commercial firm Netquest from a pool of users who were active in mainstream commercial websites in Spain. The survey is conducted online using Netquest's dedicated platform. The four first panel waves were conducted six months apart (starting in November 2010 and ending in May 2012). The next 8 waves (2013-2020) have been conducted yearly, every May.

The initial sample was not representative of the Spanish population, but it was representative of the young (16-45) Spanish internet users with one exception: our initial sample was more educated than actual Spanish internet users of that age. To compensate for panel attrition, new respondents were included in the study in waves 2 (N=620), 5 (N=845), 9 (N=996),10 (N=504) and 12 (N=417). The new respondents in waves 9, 10 and 12 were incorporated using sampling quotas for education, gender, age, region ("*Comunidad Autónoma")* and size of the municipality in order to make the final sample of each of these waves representative of these characteristics in the Spanish population aged between 18 and 54 (18 and 55 in the case of wave 12). The number of participants in each wave ranges from a maximum of 2433 respondents in wave 2 to a minimum of 1014 respondents in wave 7.

Figure B1: Distribution of observations along the values of the running variable

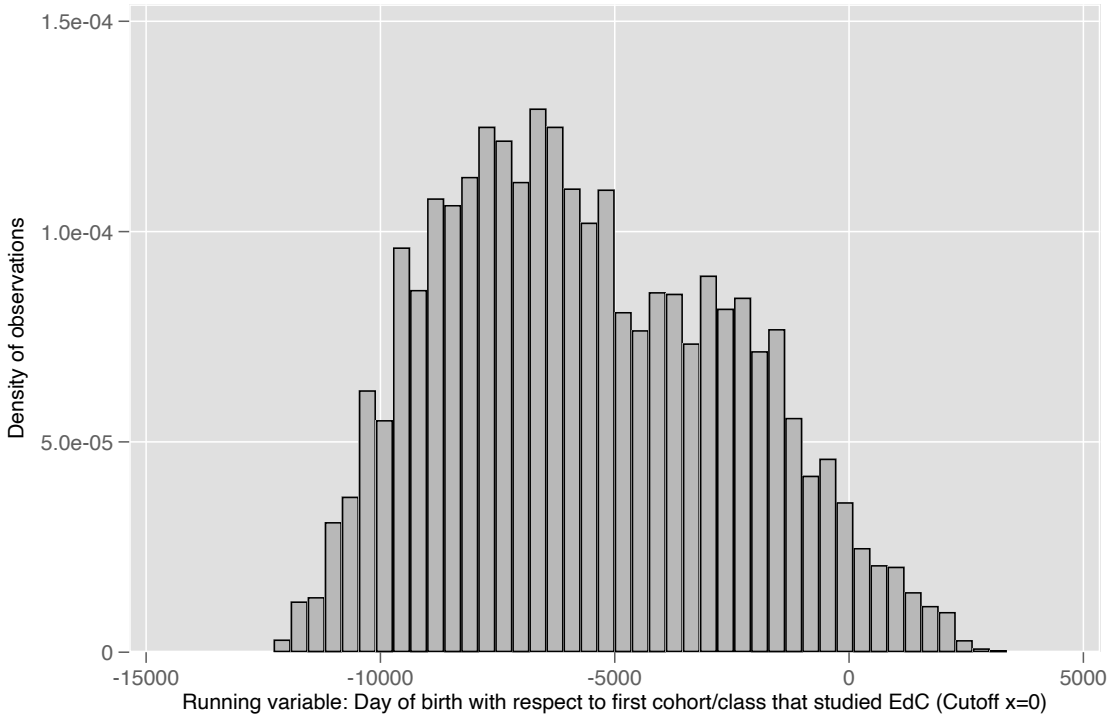


Table B1: Balance tests on predetermined covariates and placebo outcomes (RD estimates)

	RD estimate	p-value	Bandwidth	Effective number of observations
Socioeconomic characteristics:				
Town size: Less than 50.000	-0.07	0.55	806.22	1133
Town size: 50.000-500.000	-0.07	0.55	580.95	811
Town size: More than 500.000	0.11	0.26	577.08	808
Age	0.04	0.96	844.57	1167
Women	-0.06	0.69	652.51	901
Socio economic status	-0.94	0.31	399.67	136
Household income	0.04	0.93	825.05	1023
Religion: Catholic	0.04	0.83	421.92	55
Education level	0.42	0.41	775.58	1073
Attended public school	0.16	0.43	358.74	43
Political characteristics:				
PID: PSOE	-0.00	0.98	903.58	1086
PID: PP	-0.03	0.50	590.46	701
PID: Podemos	-0.13	0.22	699.71	514
PID: Ciudadanos	-0.04	0.43	1056.62	794
PID: IU	0.05	0.39	1014.43	1217
PID: PNV	0.00	0.48	1675.78	2197
PID: ERC	-0.02	0.63	1076.61	1297
PID: CIU	0.03	0.23	1057.75	1280
PID: BNG	0.00	0.75	690.59	813
PID: UPyD	0.01	0.72	753.97	878
PID: Compromís	0.03	0.48	1096.01	818
PID: VOX	-0.00	0.97	1099.90	820
PID: No party id	-0.02	0.86	624.43	743
Left-right respondent	-0.29	0.51	681.16	943
Parents' characteristics:				
Education father	-0.20	0.62	354.00	42
Education mother	0.49	0.37	393.76	52
Left-right father	0.04	0.97	379.35	49
Left-right mother	-0.58	0.65	369.80	46
Parents demonstrated during youth	0.23	0.55	395.74	52
Parents strike during youth	0.61	0.10	407.59	52
Parents attended meetings during youth	0.43	0.32	383.15	50
Parents attended church during youth	-0.39	0.34	408.39	53
Parents discussed politics during youth	-0.07	0.85	428.82	53
Parents read newspaper during youth	0.68	0.13	387.01	50

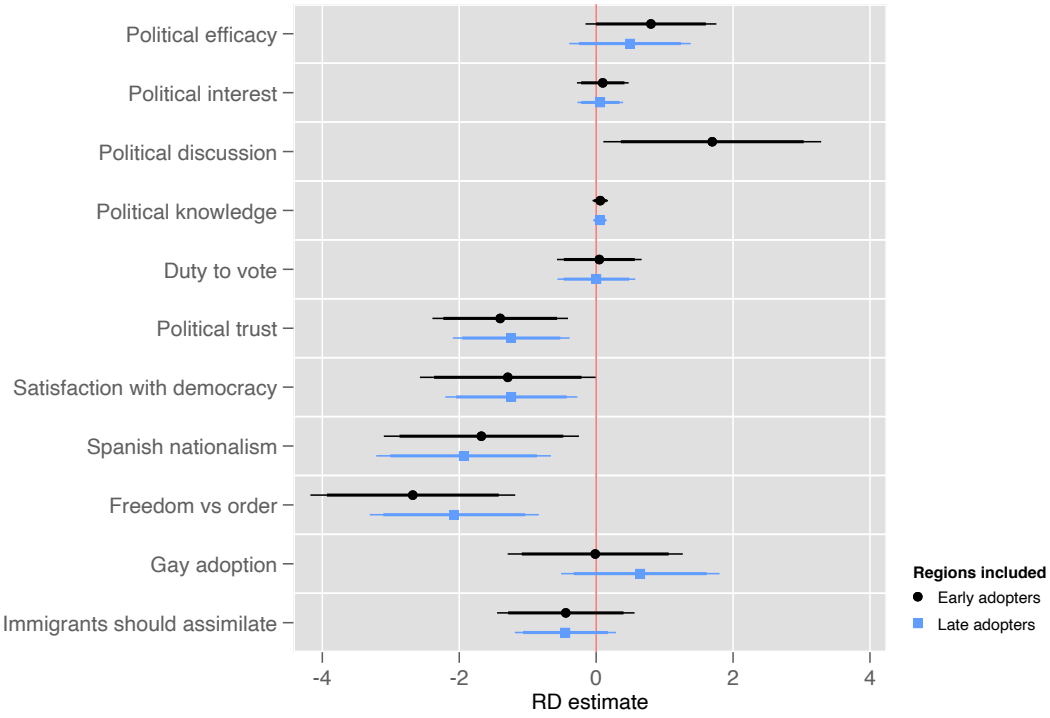
Note: Conventional RD estimate and p-values reported. Standard errors adjusted for clustering at the respondent level. Common bandwidth on each side of the threshold selected through the MSE-optimal bandwidth selector. PID = Party identification.

Table B2: Regional balance tests (RD estimates)

	RD estimate	p-value	Bandwidth	Effective number of observations
Andalucía	0.05	0.66	707.58	961
Aragon	0.04	0.21	704.52	959
Asturias	-0.01	0.85	973.49	1339
Baleares	0.03	0.12	1007.13	1373
Canary Islands	-0.00	1.00	777.22	1085
Cantabria	-0.02	0.18	1369.11	1938
Castilla and Leon	0.02	0.67	726.68	975
Castilla la Mancha	-0.05	0.09	723.21	975
Catalonia	0.01	0.94	689.81	951
Valencia	0.01	0.86	809.08	1140
Extremadura	-0.05	0.11	906.12	1258
Galicia	-0.02	0.68	639.99	880
Madrid	0.04	0.39	1016.93	1401
Murcia	0.03	0.30	880.00	1228
Navarra	-0.01	0.46	446.41	582
Basque Country	-0.07	0.23	962.36	1314
La Rioja	0.00	0.30	982.34	1359

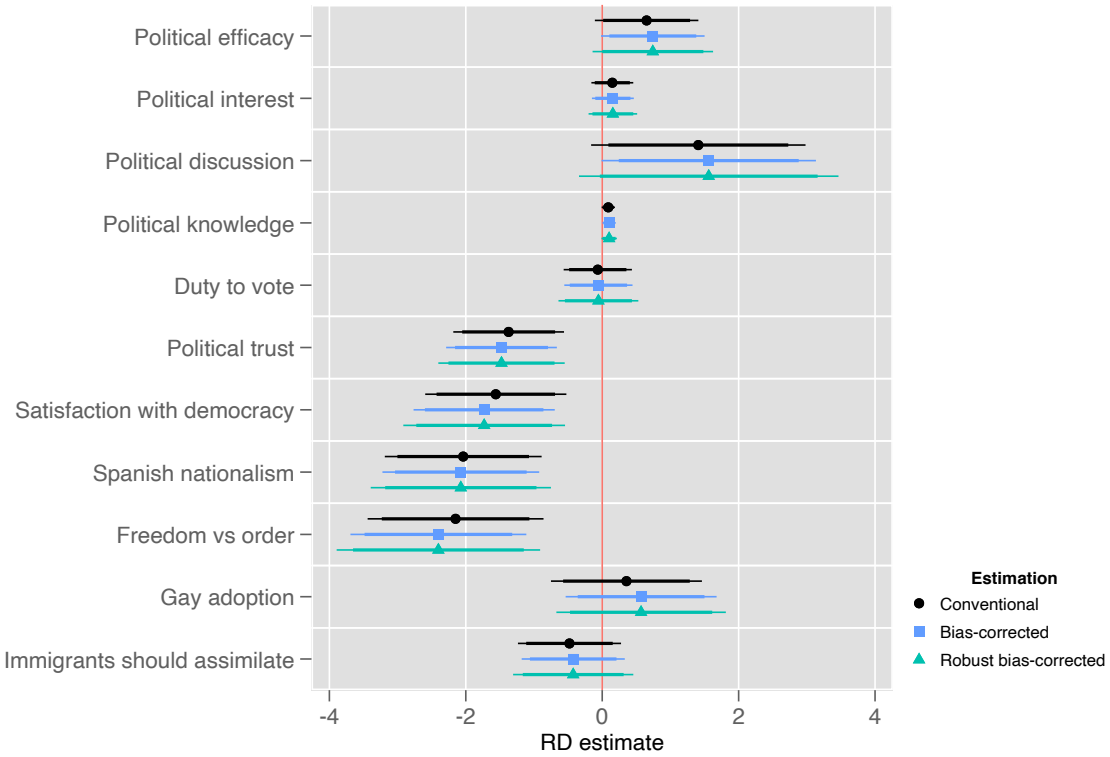
Note: Conventional RD estimate and p-values reported. Standard errors adjusted for clustering at the respondent level. Common bandwidth on each side of the threshold selected through the MSE-optimal bandwidth selector.

Figure B2: Regional heterogeneity in the effects of EdC



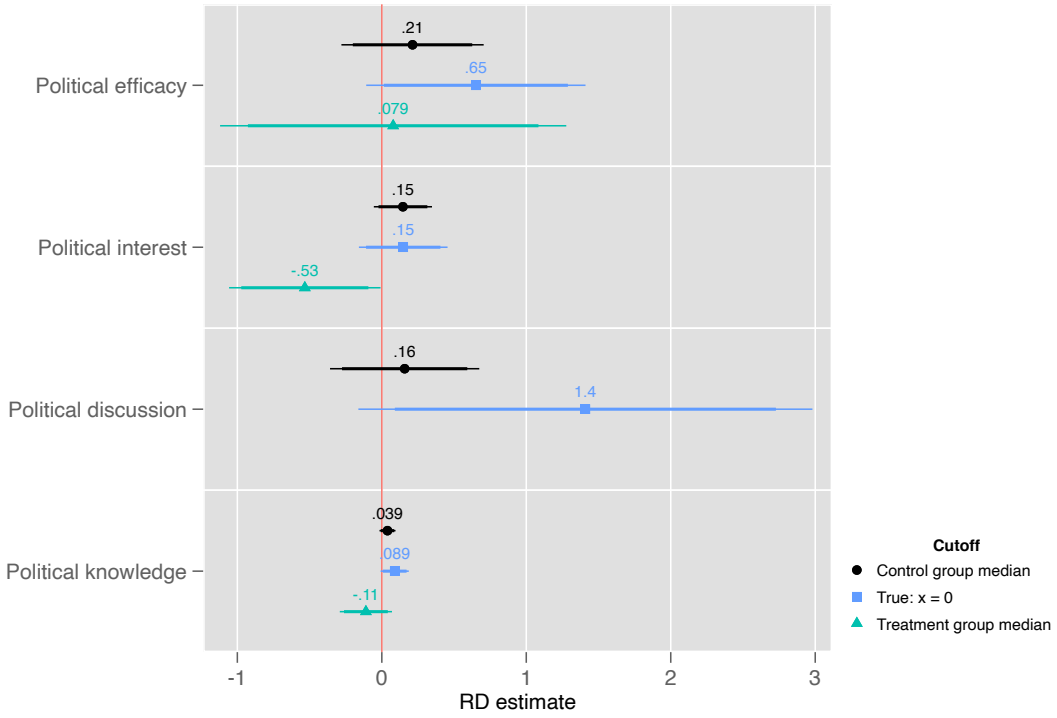
Note: RD estimate cannot be computed for the "Political discussion" variable in the late adopting regions due to the small number of observations at the threshold. Standard errors adjusted for clustering at the respondent level. Covariate-adjusted estimates. Common bandwidth on each side of the threshold selected through the MSE-optimal bandwidth selector. Thin and thick lines are 95 and 90 percent conventional confidence intervals, respectively.

Figure B3: Different types of RD estimate



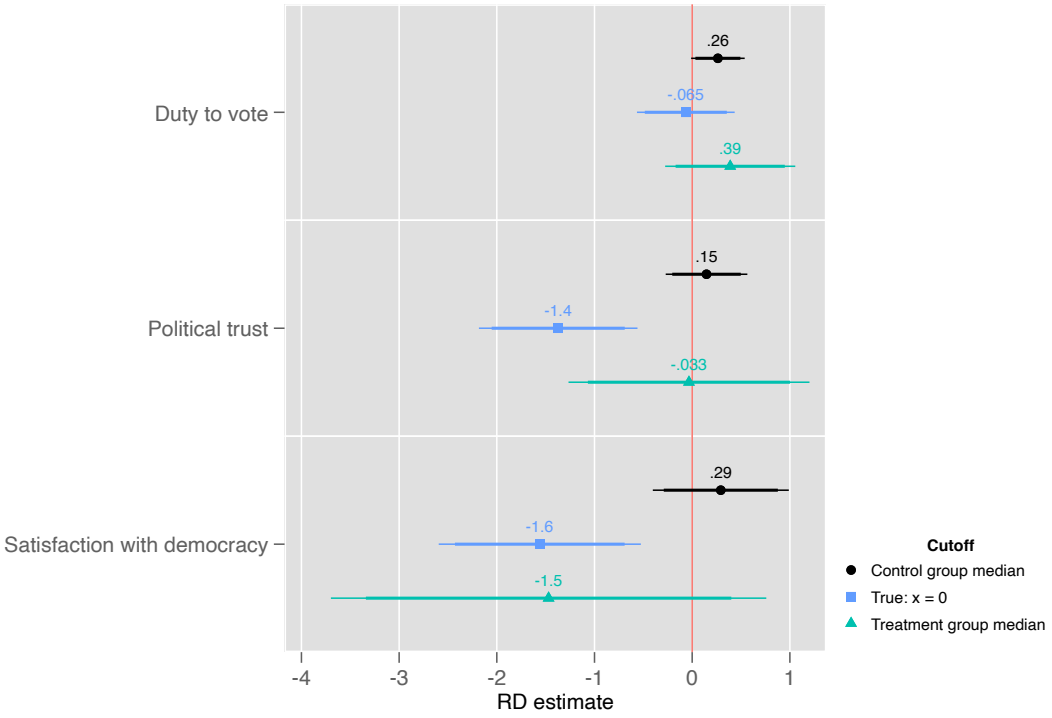
Note: Standard errors adjusted for clustering at the respondent level. Covariate-adjusted estimates. Common bandwidth on each side of the threshold selected through the MSE-optimal bandwidth selector. Thin and thick lines are 95 and 90 percent confidence intervals, respectively.

Figure B4: RD estimates for true and placebo cutoffs. Political engagement variables.



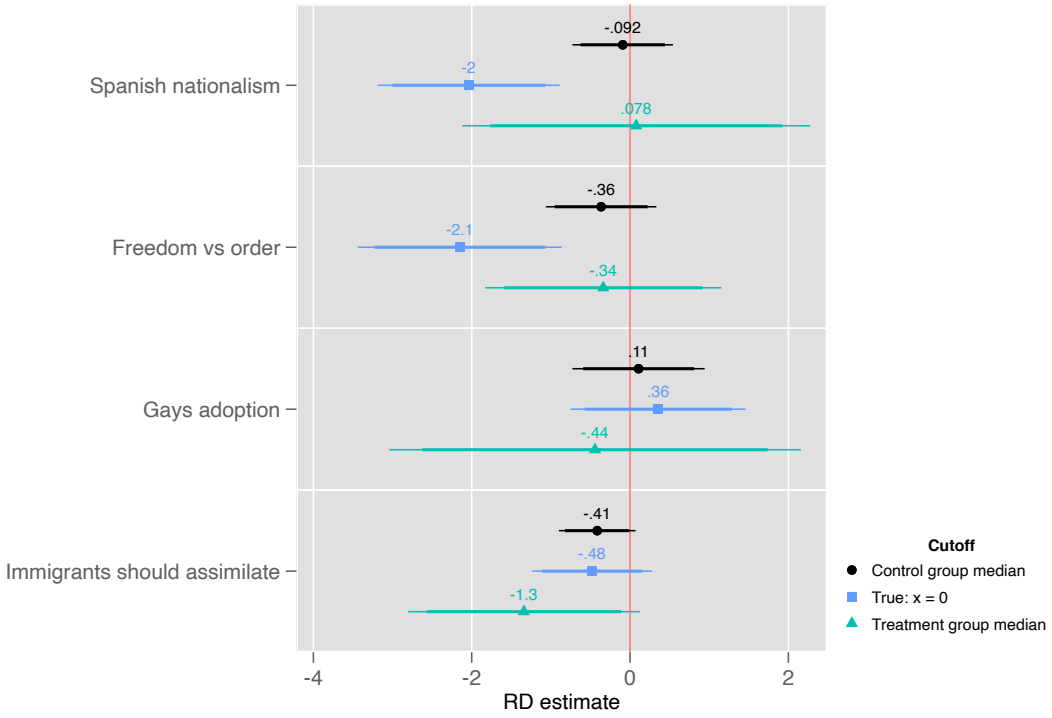
Note: RD estimate cannot be computed for the "Political discussion" variable at the treatment group median due to the small number of observations at the threshold. Conventional RD estimation. Standard errors adjusted for clustering at the respondent level. Region covariate-adjusted estimates. Common bandwidth on each side of the true and placebo thresholds selected through the MSE-optimal bandwidth selector. Thin and thick lines are 95 and 90 percent conventional confidence intervals, respectively. Control group median $x = -6136$. *Treatment group median* $x = 852$. The RD model using the control/treatment group median as a placebo cutoff only includes observations from the control/treatment group.

Figure B5: RD estimates for true and placebo cutoffs. Institutional support variables



Note: Conventional RD estimation. Standard errors adjusted for clustering at the respondent level. Region covariate-adjusted estimates. Common bandwidth on each side of the true and placebo thresholds selected through the MSE-optimal bandwidth selector. Thin and thick lines are 95 and 90 percent conventional confidence intervals, respectively. Control group median $x = -6136$. *Treatment group median* $x = 852$. The RD model using the control/treatment group median as a placebo cutoff only includes observations from the control/treatment group.

Figure B6: RD estimates for true and placebo cutoffs. Institutional support variables



Note: Conventional RD estimation. Standard errors adjusted for clustering at the respondent level. Region covariate-adjusted estimates. Common bandwidth on each side of the true and placebo thresholds selected through the MSE-optimal bandwidth selector. Thin and thick lines are 95 and 90 percent conventional confidence intervals, respectively. Control group median $x = -6136$. *Treatment group median* $x = 852$. The RD model using the control/treatment group median as a placebo cutoff only includes observations from the control/treatment group.

APPENDIX C: QUESTION WORDING, CODING AND DISTRIBUTION OF VARIABLES

Table C1: Summary of variables used in the paper

Variable	Coding	N	Mean	SD	Min	Max	# Panel waves
Internal political efficacy	I (don't=0) understand (very well=10) what's going on in politics.	20961	6.07	2.4	0	10	12
Political interest	Politics interests you: 0"Not at all"; 1"A bit"; 2"Very much"; 3"A lot"	20962	1.38	0.85	0	3	12
Political discussion	Additive scale (0-6) resulting from adding the frequency of political discussion (0=never, 1= sometimes, 2=often) with your best friend, family and coworkers	1931	2.92	1.51	0	6	1
Political knowledge	Political knowledge index based on the proportion of correctly answered knowledge questions asked in each panel wave (higher values indicate higher knowledge)	20907	0.45	0.33	0	1	12
Duty to vote	0=Voting is a choice, 3=Voting is a duty, very strongly	18949	1.41	1.29	0	3	11
Political trust	Additive scale (0-10) of trust in parliament, government, parties and politicians (alpha 0.89)	16440	2.26	2	0	10	10
Satisfaction with democracy	How satisfied are you with the way democracy works? 0=completely unsatisfied ,10= completely satisfied	7876	4.05	2.74	0	10	4
Spanish nationalism	The following scale measures your feeling of "Spanisheness". Where would you locate yourself? 0= does not feel attached to the nation at all, 10=feels attached to the nation, very much.	20960	5.79	3.09	0	10	12
Freedom vs. Order	0=freedom should be prioritized over order, 10= order should be prioritized over freedom	7876	3.11	2.94	0	10	4
Gay adoption	Child adoption by homosexual couples should be 0=forbidden, 10=allowed	18948	7.47	3.22	0	10	11
Immigrants should assimilate	Immigrants having the same culture than us is 0=not important at all, 10=very important	20961	5.97	2.72	0	10	12
Age	Age of the respondent	20962	35.83	9.11	16	58	12
Gender	Gender of the respondent (0=men; 1=women)	20962	0.49	0.5	0	1	12
Education	Education of the respondent measured in 11 categories. Higher values indicate higher education	20962	6.9	2.88	1	11	12
Socio Econonic Status (SES)	Socio economic status of the respondent measured in 5 categories, with higher values indicating higher income	6154	2.08	1.26	1	5	4
Income	Net income of respondent measured in 9 categories, with higher values indicating higher income	19351	3.95	1.83	1	10	12
Town size: Less than 50.000	Town where respondent lives has less than 50,000 inhabitants (=1), otherwise (=0)	20872	0.44	0.5	0	1	12
Town size: 50.000-500.000	Town where respondent lives has between 50,000 and 500,000 inhabitants (=1), otherwise (=0)	20872	0.35	0.48	0	1	12
Town size: More than 500.000	Town where respondent lives has less more than 500,000 inhabitants (=1), otherwise (=0)	20872	0.2	0.4	0	1	12
Catholic	Respondent identifies as catholic when thinking about religion (=1) or as an atheist, agnostic or "another religion" (=0)	2094	0.44	0.5	0	1	1

Left-right respondent	When talking about politics people commonly refer to right-wing and left-wing. Where would you locate yourself (0= extreme left; 10=extreme right)	20939	4.36	1.86	0	10	12
Attended public school	Takes value 1 if respondent attended public school. 0 otherwise.	2428	0.67	0.47	0	1	1
PID: PSOE	Takes value 1 if respondent feels close or identifies with PSOE, takes value 0 otherwise	19292	0.18	0.38	0	1	12
PID: PP	Takes value 1 if respondent feels close or identifies with PP, takes value 0 otherwise	19292	0.14	0.35	0	1	12
PID: Podemos	Takes value 1 if respondent feels close or identifies with Podemos, takes value 0 otherwise	10143	0.13	0.33	0	1	7
PID: Ciudadanos	Takes value 1 if respondent feels close or identifies with Ciudadanos, takes value 0 otherwise	10143	0.13	0.33	0	1	7
PID: IU	Takes value 1 if respondent feels close or identifies with IU, takes value 0 otherwise	19292	0.09	0.29	0	1	12
PID: PNV	Takes value 1 if respondent feels close or identifies with PNV, takes value 0 otherwise	19292	0.01	0.09	0	1	12
PID: ERC	Takes value 1 if respondent feels close or identifies with ERC, takes value 0 otherwise	19292	0.03	0.16	0	1	12
PID: CiU	Takes value 1 if respondent feels close or identifies with CIU, takes value 0 otherwise	19292	0.01	0.12	0	1	12
PID: BNG	Takes value 1 if respondent feels close or identifies with BNG, takes value 0 otherwise	19292	0.01	0.09	0	1	12
PID: UPyD	Takes value 1 if respondent feels close or identifies with UPyD, takes value 0 otherwise	19292	0.04	0.2	0	1	12
PID: Compromis	Takes value 1 if respondent feels close or identifies with Compromis, takes value 0 otherwise	10143	0.01	0.11	0	1	7
PID: Vox	Takes value 1 if respondent feels close or identifies with Vox, takes value 0 otherwise	10143	0.03	0.18	0	1	7
PID: No party id	Takes value 1 if respondent does not feel close or identifies with any party, takes value 0 otherwise	19292	0.33	0.47	0	1	12
Left-right father	Left-right ideology of respondent's father. 0=extreme left; 10=extreme right	2407	4.97	2.25	0	10	1
Left-right mother	Left-right ideology of respondent's mother. 0=extreme left; 10=extreme right	2411	4.82	2	0	10	1
Parents demonstrated during youth	When respondent was a child or teenager parents went to demonstrations =1. Otherwise = 0	2427	2.63	0.59	1	3	1
Parents strike during youth	When respondent was a child or teenager parents participated in strikes =1. Otherwise = 0	2427	2.51	0.62	1	3	1
Parents attended meetings during youth	When respondent was a child or teenager parents attended meeting or assemblies =1. Otherwise = 0	2427	1.75	0.73	1	3	1
Parents attended church during youth	When respondent was a child or teenager parents went to church on Sundays =1. Otherwise = 0	2427	2.22	0.78	1	3	1
Parents discussed politics during youth	When respondent was a child or teenager parents talked about politics =1. Otherwise = 0	2427	1.96	0.64	1	3	1
Parents read newspaper during youth	When respondent was a child or teenager parents read newspapers (excluding sports newspapers) =1. Otherwise = 0	2427	1.69	0.73	1	3	1
Education father	Education of respondent's father in 4 categories, with higher values indicating higher education	2428	2.32	1.02	1	4	1
Education mother	Education of respondent's mother in 4 categories, with higher values indicating higher education	2428	2.19	0.95	1	4	1

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