

Université de Montréal

The Stability and Sources of Citizens' Sense of Civic Duty to Vote

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Abstract

Prior work provides large evidence that civic duty to vote represents a key predictor of electoral participation. Moreover, the analysis of civic duty questions from extant surveys reveals that many individuals feel the moral obligation to participate in elections. However, is civic duty the result of rationalization, meaning that those who voted in an election are likely to report a belief in the duty to vote, while those who abstained are likely to report that voting is not a duty, but rather a choice? Also, where does the sense of civic duty to vote come from?

In this doctoral dissertation, I provide an answer to those questions. More specifically, in Chapter 1, I investigate the extent to which duty is stable in search of evidence on the rationalization of duty. In this analysis, I rely on structural equation models, which I fit to unique, nine-wave panel data from Spain and the United Kingdom. I find that civic duty is a very stable attitude, implying that, while some rationalization is possible, duty is unlikely to be strongly driven by individuals' previous voting behavior.

In Chapter 2, I examine whether civic education in school contributes to the development of duty. I focus on civic education as a potential source of duty given its role in transmitting social norms to new generations, and its influence on duty-related attitudes like political interest, implying that civic education likely contributes to the development of duty. To examine the connection between civic education and duty, I use data from the 2016 International Civic and Citizenship Education Study, as it represents the largest survey research containing civic duty and civic education measures. My analyses reveal that three common forms of civic education (civics courses, active learning strategies, and open classroom environment) contribute to predicting duty, but that civics courses exert the largest effect on duty.

In Chapter 3, I investigate the role of compulsory voting in the development of duty. I examine the relationship between compulsory voting and duty given the possibility that compulsory voting signals that a “good” citizen must vote, and, in this way, that compulsory voting fosters duty. I analyze this relationship by leveraging the abolition of compulsory voting in Chile (in 2012), which I explore with synthetic control models and Latinobarometer data. With this empirical strategy, I find evidence that compulsory voting positively affects duty.

In Chapter 4, I investigate if corruption bears an effect on duty. I focus on the relationship between corruption and duty given the chances that corruption reduces individuals’ incentives to believe in the duty to vote, and, consequently, that corruption affects duty. I examine the link between corruption and duty through mediation tests, where corruption represents the independent variable, duty the mediating variable and turnout the dependent variable. Using data from the Making Electoral Democracy Work project, I find that, unlike civic education and compulsory voting, corruption exerts a marginal effect on duty.

Keywords: electoral participation, political behavior, political attitude, civic duty to vote, attitude stability, civic education, compulsory voting, corruption.

Résumé

Plusieurs travaux montrent que le devoir civique constitue un prédicteur clé de la participation électorale. De plus, les recherches antérieures indiquent que le sentiment d'obligation de voter est répandu parmi un grand nombre de citoyens. Cependant, le devoir civique est-il le résultat d'une rationalisation de la part des électeurs ? Autrement dit, ceux qui ont voté lors d'une élection sont-ils plus enclins à dire que le vote est un devoir plutôt qu'un choix ? En outre, quelles sont les origines du sens du devoir civique de voter ?

Dans cette thèse de doctorat, j'offre une réponse à ces questions. Plus précisément, dans le premier chapitre, j'examine dans quelle mesure le sens du devoir civique est stable afin de déterminer s'il y a, oui ou non, un phénomène de rationalisation parmi l'électorat. Je m'appuie sur des modèles d'équations structurelles et sur des données de panel espagnoles et britanniques comprenant neuf vagues. Je trouve que le devoir civique est très stable, ce qui indique qu'il y aurait peu de rationalisation en ce qui a trait à cette attitude politique.

Dans le second chapitre, j'examine si l'éducation civique contribue au développement du devoir civique. Je me concentre sur l'éducation civique en raison du rôle que joue celle-ci dans la transmission des normes sociales aux nouvelles générations et de son influence sur les attitudes liées au sens du devoir comme l'intérêt politique. Pour déterminer la nature du lien entre éducation civique et devoir civique, j'utilise les données de l'International Civic and Citizenship Education Study (ICCS) de 2016. Il s'agit de la plus grande enquête sur le devoir civique et l'éducation civique. Mes analyses montrent que trois formes courantes d'éducation civique (les cours d'éducation civique, les stratégies d'apprentissage actif et un environnement

de classe ouvert) contribuent à prédire le sentiment du devoir civique de voter chez les individus, mais que les cours d'éducation civique disposent du plus grand impact.

Dans le troisième chapitre, j'étudie l'effet du vote obligatoire sur le devoir civique. J'examine la corrélation entre le vote obligatoire et le devoir civique dans la mesure où cette institution politique peut signaler aux individus qu'un « bon » citoyen doit voter. Prenant l'abolition du vote obligatoire au Chili comme cas d'analyse, j'utilise des modèles de contrôle synthétiques et les données des Latinobarometers afin de mesurer le lien causal entre le vote obligatoire et le devoir civique. Je trouve que le vote obligatoire affecte positivement le devoir civique.

Dans le quatrième chapitre, j'étudie l'impact de la corruption sur le devoir civique. Je me concentre sur la corrélation entre la corruption et le devoir civique puisque les individus risquent d'avoir un sentiment de devoir moindre dans les contextes de corruption politique. J'examine la corrélation en question à travers des tests de médiation et les données du projet Making Electoral Democracy Work (MEDW). Je trouve qu'au contraire de l'éducation civique et du vote obligatoire, la corruption affecte marginalement le devoir civique.

Mots clés : participation électorale, comportement politique, attitude politique, devoir civique de voter, stabilité des attitudes politiques, éducation civique, vote obligatoire, corruption.

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Introduction

In many contemporary democracies the common wisdom is that voting is a civic duty. Official sources like the Kansas Secretary of State's Office argue that "U.S. citizens have a responsibility to participate in their government by registering to vote and voting in elections" (CyberCivics n.d., 2). Similarly the United States Citizenship and Immigration Services (USCIS) tells individuals who wish to become American citizens that "the law does not require citizens to vote, but voting is a very important part of any democracy" (Services n.d., 4). Media personalities also adhere to and spread the message that voting is a civic duty. For instance, Jennifer King, the assistant coach for the Washington Football Team, signs an op-ed in the National Football League's (NFL) website in which she urges football fans to exercise their civic duty and vote in the 2020 presidential election (King 2020).

In this sense there is a widespread view in contemporary democracies that citizens have a civic duty to vote in elections. But what does it really mean to feel a sense of civic duty to vote? And where does a sense of civic duty to vote come from? With this dissertation, I provide an answer to these two questions. More precisely, I advance knowledge on the nature of civic duty by studying the stability of individuals' sense of civic duty to vote. And I provide insights on the sources of civic duty by investigating whether and to what extent civic education, compulsory voting, and corruption contribute to the development of civic duty among individuals.

This introduction is organized as follows. First, I discuss citizenship norms and how the duty to vote relates to those norms. Subsequently, I debate the importance of duty as a motivation to vote and as a widespread political attitude, and I conduct a systematic review of prior work on the stability and sources of duty. Then, I indicate how I operationalize duty in this

dissertation, as well as the methods that I use to examine the stability and the relationship between civic education, compulsory voting, corruption and duty. I conclude by presenting the four empirical chapters that make up the body of this dissertation.

1. Citizenship norms and the importance of civic duty to vote

Citizenship norms represent a group of social norms defining what it means to be a “good” citizen (Denters, Gabriel, and Torcal 2007; Dahl 1998; Pateman 1970; Verba, Schlozman, and Brady 1995). Within the vast literature on the topic, we can find at least three different typologies of citizenship norms. First, some researchers like Van Deth (2009) suggest citizenship norms divide in those referring to (1) political participation, (2) political autonomy, (3) law obedience and respect of the state authority, and (4) social justice (Van Deth 2009). Other researchers like Bolzendahl and Coffé (2013) propose instead a typology of citizenship norms that includes a political, a civil and a social dimension. The political dimension “combines beliefs in the importance of voting, being active in social and political associations and keeping a watch on government” (Bolzendahl and Coffé 2013, 47). The civil dimension includes “the importance of paying taxes and obeying the law” (Bolzendahl and Coffé 2013, 47). And the social dimension refers to “the importance of understanding the opinions of others, shopping for political reasons and helping those worse off” (Bolzendahl and Coffé 2013, 47). Still another group of researchers, including Dalton (2008), propose a typology of citizenship norms comprising two dimensions: the duty-based and the engaged-based dimensions. To begin with, a duty-based dimension “involves norms of social order” like political participation and law obedience (Dalton 2008, 80). By contrast, an engaged-based dimension spans “elements that are typically described as liberal or communitarian norms of citizenship” like

solidarity and participation (Dalton 2008, 81). The engaged citizen is also “willing to act on his or her principles, be politically independent and address social needs” (Dalton 2008, 81).

Yet a common point across the three different typologies of citizenship norms is the view that a “good” citizen must vote in elections, which is at the basis of civic duty to vote. In fact, Gallego and Oberski (2012) suggest that civic duty to vote follows a three-stage development process. The first stage entails individuals’ recognition of the citizenship norm to vote, as “people need to be aware that it exists” in order to believe that voting is a civic duty (Gallego and Oberski 2012, 428). The second stage relates to the internalization of the citizenship norm to vote, yielding the development of the belief in the civic duty to vote. The third and final stage concerns voting based on the internalized sense of civic duty to vote in elections.

Defined in this way, there are many reasons for studying the duty to vote as a stand-alone research object. The first reason is that duty represents a key motivation for voting. Classical studies in the field like Campbell et al. (1960) and Riker and Ordeshook (1968) offer compelling evidence of the strong relationship between duty and the vote. More precisely, Campbell et al. (1960, 156) show in the *American Voter* that “the strength of a person’s sense of citizen duty has a very great influence on the likelihood of his voting”. In the same vein, Riker and Ordeshook (1968) reveal that duty has a large impact on voter turnout, and more so than any of the other variables from a rational model of voting – i.e. the benefit from voting, the probability of casting a decisive vote, and the cost from voting.

More recent research provides evidence of the importance of duty as a turnout predictor too. Blais and Achen (2019, 474) show, for example, that “preference and duty are each powerful determinants of turnout”. Furthermore, Collins and Block (2020) reveal that duty is one of the

main reasons why African-Americans vote in elections. Finally, Smets and van Ham (2013) report that of the 17 tests with duty as one of the main independent variables, only one test fails to observe a statistically significant effect of duty on individuals' electoral participation. In addition, of the six studies that include tests of the link between duty and turnout, none reports a failure to find a significant correlation between those two factors. Comparatively, political interest, which researchers like Prior (2010) consider as a very important turnout predictor, fails to reach statistical significance in 18 of 73 tests (i.e. 25%).

Through its impact on turnout, civic duty contributes to the quality of representative democracy in two important ways. First, the number of voters in elections is directly correlated with a key aspect of representative democracy; equal participation (Lijphart 1997). In fact, Dassonneville and Hooghe (2017, 195) investigate the sharp turnout decline in the Netherlands following the abolition of compulsory voting and longitudinal data from six European countries (Denmark, Germany, Great Britain, the Netherlands, Norway, and Sweden), and find that educational gap in turnout grows "as fewer and fewer citizens turn out to vote". Persson et al. (2013) report similar results in an analysis of a 36.5 p.p. turnout decline between the 2010 and the 2011 Swedish county elections. They reveal that as a result of the sharp decline in turnout, the "level of inequality in participation substantially increased between young and old, rich and poor, low and high educated and politically interested and uninterested" between those two elections (Persson et al., 2013, 172). Hajnal and Trounstine (2005) further reveal with American data that "lower turnout leads to substantial reductions in the representation of Latinos and Asian Americans on city councils and in the mayor's office" (Hajnal and Trounstine 2005, 515).

In addition to equal participation, the number of voters correlates with another key aspect of representative democracy: responsiveness (Esaiasson and Wlezien 2017). Martin and

Claibourn (2013) examine whether legislative behavior in the United States is a function of district electoral participation and they report that “the collective preferences of congressional districts are more influential on the voting behavior of members of Congress when those districts participate at higher rates” (75). Peters and Ensink (2015, 596) similarly find in an analysis of responsiveness in 25 European countries that “when participation is relatively high, the preferences of more people are communicated to the representatives” (596). And Hooghe et al. (2019) show with an original data set including 21 OECD countries and the 1980-2017 period that “in high-turnout conditions, electoral results are more representative of public opinion as a whole, thus allowing for an effective transfer of public opinion preferences to the policy-making process” (1005). In summary, as the number of voters increases, two crucial aspects of representative (equal participation and responsiveness) tend to increase too.

The second reason I focus on duty is that it represents a widespread political attitude. The fact that most individuals believe in the duty to vote can be illustrated with the analysis of responses to a civic duty question in the Making Electoral Democracy Work (MEDW) dataset (Stephenson et al. 2017). I use the data from the MEDW to illustrate this point because it includes duty information from 27 election studies that were administered in sub-national, national, or supra-national elections, across five countries (Canada, Germany, France, Spain, and Switzerland). Moreover, the MEDW is unique as it interviews individuals about their sense of duty to vote in national, regional, European, and municipal elections, thus allowing a more detailed exploration of how widespread is duty. In the analysis of the MEDW data, I recode responses to either 0 or 1, 0 indicating no duty belief, and 1, a sense of duty. Such analysis reveals that 52% of respondents in the MEDW believe that voting in national elections is a duty, denoting a widespread sentiment of duty to vote in national elections. While a variation is observed when it comes to second-order elections (49% report a duty to vote in regional

elections, 38% in European elections, and 47% in municipal elections), it stands out that a quite large number of individuals believe that voting is a duty in second-order elections.

Given the two reasons for studying duty, in this dissertation I address two important questions about individuals' sense of duty to vote: 1) does duty reflect a rationalization of citizens' voting behavior? And 2) where does duty come from? I focus on the first question (does duty reflect a rationalization of citizens' voting behavior?) in this dissertation because if individuals rationalize how dutiful they feel, duty might end up being less influential on the vote. And I focus on the second question (where does duty come from?) because, by learning about why some individuals feel a duty to vote, we also learn about why individuals vote (Gallego and Oberski 2012). In the following section, I elaborate on these two questions, mobilizing the relevant literature on the stability and the sources of sense of duty to vote.

2. The stability of civic duty to vote

Within the political behavior literature, there is an ongoing debate about the nature of duty. More precisely, some researchers argue that individuals who voted in an election will be more likely to report a sense of duty to vote than those who abstained in that election. Following the same logic, individuals who abstained in an election will be more likely to reject the duty to vote. Matsusaka and Palda (1999) are some of the scholars who see duty in this way. Specifically, the authors say that duty is likely to be the result of rationalization because “if there is such a thing as a person’s sense of citizen duty, and it is being accurately measured, the value should not depend on something as irrelevant as question order” (434). The same view of duty can be found in Dowding (2005). For this scholar, “habitual voters [may] justify their voting in terms of civic duty since they cannot rationalize it any other way” (456).

To the best of my knowledge, only three studies search for evidence of rationalization in duty. First, Galais and Blais (2016a) conduct cross-lagged structural equations with the purpose of measuring the extent to which duty is conditional on voter turnout. Relying on panel data from Canada and Spain, Blais and Galais reveal that there is “evidence of some rationalization or learning process in Canada but not in Spain” (224). Taking a different approach, Blais and Achen (2019) test the validity of the rationalization argument by assessing the stability of duty. Using panel data from the 2008 Cooperative Campaign Analysis Project (CCAP), they find that 70% of respondents in the CCAP provide the same responses to duty questions in two adjacent panel waves. Moreover, the authors reveal over-time correlations for duty between 0.58 to 0.73, values that are substantially higher than the 0.37 average degree of stability registered by Converse (1964) regarding individuals’ policy preferences. Feitosa et al. (2020) further test the effect of turnout on duty by means of a quasi-experiment. Specifically, the authors explore discontinuity in voting at 18 years old with an original dataset collected in the Belgian city of Ghent and they reveal no evidence of a turnout effect on duty.

Yet not all researchers are convinced that duty is mainly exogenous to turnout. Some scholars still argue that duty might be contingent on individuals’ voting behavior. Fieldhouse (2019) argues, for example, that “the concept of voting as a duty is bedeviled by the possibility of reverse causation: people who vote, when asked, reason that voting is a duty whilst nonvoters quite reasonably prefer to deny this claim” (322). My dissertation addresses this scholarly debate. Following the work of Blais and Achen (2020), I explore the stability of duty in search of evidence of the rationalization in duty. Evidence of a high stability – especially among those who voted in one election, but abstained in a subsequent election, or vice-versa – should

strengthen the view that duty is not the result of rationalization. By contrast, evidence of high instability would suggest that duty includes a relevant degree of rationalization.

3. The sources of civic duty

Within the civic duty literature, we can observe many studies on the origins of this political attitude. These studies provide compelling evidence that genes, personality traits, and political attitudes contribute to the development of the duty to vote. Beginning with genes, Weinschenk and Dawes (2018) reveal with data from the Midlife in the United States (MIDUS) Study that “the heritability estimate for the measure of civic duty is 0.39 and is significantly different from zero at the 5% level” (60). Dawes et al. (2014) offer evidence in support of these findings. Specifically, the authors examine data from the Swedish Screening Across the Life-Span Study (SALTY), and they find that heritability estimates are “significant for political interest, internal and external political efficacy, confidence in politicians, and the view that voting is a civic duty” (899) (see Loewen and Dawes 2012, as well, for similar results). In this sense, only one study (that of Klemmensen et al. 2012) provides evidence in the contrary direction; that is, that there may be little or “no heritability in citizens’ sense of civic duty” (420).

Moving to personality traits, Dinesen et al. (2014) examine data from an original survey in Denmark, and they find that “conscientiousness is mainly positively associated with adherence to citizenship norms and specifically those that invoke a strong sense of duty” (148). Similarly, Gallego and Oberski (2012) use data from a Spanish survey conducted before and after the 2009 European Parliament, and they report that conscientiousness strongly affects individuals’ sense of duty, which, in turn, affects their likelihood of voting in elections. Schoen and Steinbrecher (2013), as well, examine survey data from the 2009 German federal election, and

they reveal that agreeableness and conscientiousness are positively correlated with duty (see Weinschenk 2014 and Blais and St-Vincent 2011 for additional evidence of the relationship between personality traits and individuals' sense of duty to vote).

Finally, when it comes to political attitudes, Bowler and Donovan (2013) explore panel data from the British 2011 Alternative Voting Referendum Campaign Survey, and they offer evidence that dutiful citizens are generally more interested in politics and more trustful of politicians than those for whom voting is a choice. Carreras (2018) also finds a relationship between political interest and duty. Examining the data from the 2014 Citizenship Module of the International Social Survey Programme (ISSP), Carreras shows that political interest affects three different citizen duties: voting, being politically active, and watching for governmental actions (see Blais and Daoust 2020 for similar results about voting). Besides political interest and political trust, there is evidence that party identification also affects individual' sense of duty (Santana and Aguilar 2019), and that support for political community and for democracy exert an influence on the development of sense of duty too (Galais and Blais 2017).

In this way, the review of prior work on the sources of duty indicates that duty is partly inherited, and partly the result of personality traits and of political attitudes.¹ Most importantly, this review suggests some avenues for research, especially regarding the role of the social/political context in how dutiful a person feels. To the best of my knowledge, few studies

¹ Besides research on the relationship between genes, personality traits, political attitudes and duty, work explores if duty is correlated with socio-demographic characteristics too. For instance, Blais and Rubenson (2013) examine data from 1952–1992 American National Election Studies and offer “evidence of substantial generational change in how people construe the act of voting” (110). In addition, Jackson (1995) shows that “highly educated individuals are more likely to believe that a person should vote in an election, regardless of concern over the outcome” (288). Furthermore, Carreras (2018) uses the 2014 ISSP and shows that “female citizens are more likely to express a civic duty to vote” This finding that is questioned, however, by Galais and Blais (2019). They show with data from the Centre d'Études Européennes of SciencePo (CEESP) and the MEDW that “in all the countries analyzed, men were more dutiful than women, or there were no significant differences present” (11).

focus on contextual factors when studying the origins of duty.² For example, Galais and Blais (2017) find that “the stronger a democracy is, the more dutiful citizens are” (16), but that ethnic fractionalization is weakly correlated with duty. Moreover, Bowler and Donovan (2013) reveal that duty is impervious to the number of elections that citizens are exposed to on the same day. Finally, Galais (2018) show that parents’ economic status, their engagement in the school, and individuals’ exposure to democratic government in the school affect the development of duty.³

Considering the limited understanding of the role of contextual factors in the development of duty, in this dissertation I also explore the effect of civic education, compulsory voting, and corruption on duty. I focus on these three factors for two main reasons. First, there are good reasons to expect a connection between civic education, compulsory voting, corruption, and duty. Beginning with civic education, we know from prior research that the school serves as an incubator of social norms to new generations of citizens (Bourdieu 1977; Dennis 1968). Given that voting constitutes a widespread social norm in many contemporary democracies (Bolzendahl and Coffé 2013), it is quite likely that civic education transmits this norm and, in this way, fosters duty. As for compulsory voting, research suggests that laws have an expressive function: they signal to citizens how they should behave (Van Der Burg 2001). Employing this logic to the law that mandates voting, it means that this law conveys the message that citizens must vote in elections, and, consequently, it is likely to lead to the development of duty (Quintelier, Hooghe, and Marien 2011; Chapman 2019). Finally, regarding corruption, research suggests that duty entails, in part, a sense of moral obligation

² Two experts on duty to vote agree with this view. They claim that while there are good reasons to expect a large influence of the context on the development of sense of duty, “we know little about [...] the institutional and contextual factors that sustain or weaken it” (Galais and Blais 2017, 19).

³ See Campbell 2006 for evidence that the school affects duty, but in another way: specifically, by the degree to which students share the same party identification.

toward the state, who, in turn, must comply with obligations like acting ethically. From this perspective, when the state fails to comply acts unethically, citizens are likely to feel disobliged to believe in the duty to vote (Bowler and Donovan 2013; Jones and Hudson 2000).

My focus on these three factors (civic education, compulsory voting, and corruption) is also motivated by the fact that they vary in numerous aspects. For example, compulsory voting represents an institutional factor, while civic education and corruption do not. In addition, corruption is more likely to change overtime (to be less stable) than civic education and compulsory voting. Furthermore, civic education initiatives generally focus on individuals in their formative years. In turn, corruption and compulsory voting should affect particularly individuals who passed those years. In short, with a focus on civic education, compulsory voting, and corruption, there is interesting variation to explore where duty comes from.

4. Operationalizations of duty

A review of the civic duty literature reveals at least three different operationalizations of this political attitude. The first measure of duty includes a statement, with which individuals can either (strongly) agree, (strongly) disagree, or neither agree nor disagree. Weinschenk and Panagopoulos (2020) use this measure of duty. More specifically, the authors ask individuals how much they agree (or disagree) with “It is a civic duty to vote in elections”. Hur (2017) and Fieldhouse and Cutts (2018) measure duty in the same way, but with similar statements: “In a democracy, every citizen has the duty to vote regularly at elections”, or “It is every citizen’s duty to vote in an election”. The second measure of duty pertains to the “good” citizenship question. For instance, Carreras (2018) assesses individuals’ sense of duty by asking, on a scale of one to seven, where one means not important at all, and seven means very important, how

important is voting for them. Klemmensen et al. (2012) use the same strategy. The authors ask individuals the question: “As far as you are concerned personally, how important is it to always vote in elections” (418). The third measure of duty concerns the Blais and Achen's (2019) duty/choice question. It reads “People have different views about voting. For some, voting is a DUTY. They feel that they should vote in every election. For others, voting is a CHOICE. They only vote when they feel strongly about that election. For you personally, is voting FIRST AND FOREMOST a Duty or a Choice?”. Galais and Blais (2019) use this measure to study the gender gap in duty, whereas Galais and Blais (2016b) use it to examine variations in duty across different election levels. Importantly, the duty/choice question has the advantage of providing a worthy alternative to duty (“choice”). In addition, the duty/choice question asks directly the degree to which voting is a duty for an individual, and it does not rely on agree/disagree response options. For these reasons, Blais and Galais (2016) defend the operationalization of duty mainly by means of a duty/choice question.

Agreeing with Blais and Galais (2016), I use the duty/choice question when examining the stability of duty, and in the study of the relationship between duty and corruption. The inclusion of the duty/choice question in an original nine-wave panel study in Spain, and in the 27 election studies from the MEDW project allows the use of the duty/choice question to study the stability of duty and the effect of corruption on duty. However, given data availability, in the analysis of the stability of duty I operationalize duty also by asking individuals how much they agree (or disagree) with “I would be seriously neglecting my duty as a citizen if I didn't vote”. As Weinschenk and Panagopoulos (2020), I acknowledge that this duty measure is limited in two important respects. First, survey respondents might be more likely to report a sense of duty because of an acquiescence bias, meaning individuals' tendency toward agreeing with statements in survey research (Holbrook, Green, and Krosnick 2003). Second, given a

widespread voting norm in many countries, individuals with no attitude about duty to vote may pay lip service to that norm and falsely report a duty to vote (Blais and Galais 2016). As a result, I might overestimate the real number of dutiful citizens in the stability tests.

Furthermore, I use a “good” citizenship question in the investigation of the effect of civic education and compulsory voting on duty. One important limitation of the “good” citizenship question is that it “asks about people's perceptions of the public norm of citizenship rather than about whether they personally support that norm” (Blais and Galais 2016, 61). Consequently, by relying on this measure, I might also overestimate the real number of dutiful individuals in the analysis of the relationship between duty, civic education, and compulsory voting. I return to the discussion of the potential consequences of measuring duty by means of an agree/disagree question and by means of the “good” citizenship question in the conclusion, when I discuss the main limitation of this doctoral dissertation.

5. Research methods

The study of the stability of civic duty, and its correlation with civic education, compulsory voting, and corruption call for different statistical methods. More precisely, in the analysis of the stability of duty, I use structural equations models. I do so because of the need to account for errors that are inherent in survey research, like question vagueness (Achen 1975). In line with this view, Prior (2010) uses structural equation models to study the stability of political interest. Prior believes that “if a respondent has different aspects of politics in mind in different panel waves”, it should yield wrong estimations of the stability of political interest (754). Green and Palmquist (1994) agree with Prior about the need to account for imperfect survey measures of partisanship. They claim that “coding errors, interviewer biases, misread or misunderstood

questions, incomplete translation of the partisanship continuum into a discrete 7-point categorization, or an inability on the part of respondents to gauge their own partisan sentiment all represent threats to accurate measurement”, and, consequently, to the accurate estimation of partisan stability (440). From this perspective, it makes sense to study the stability of duty by means of structural equation models. With these models, I “clean” duty from measurement error, and, subsequently, I estimate the stability of “latent” duty through a lag-1 approach. That is, I regress duty at time t on duty at time $t-1$, and so forth. I fit the structural equation models to a nine-wave panel dataset from the 2005-2010 British Election Panel Study, and a nine-wave panel dataset from an original study in Spain. With this modeling strategy, I believe I can properly estimate if and the extent to which individuals’ sense of duty is stable.⁴

While I rely on structural equation models to assess the stability of duty, I use different models to study the relationship between civic education, compulsory voting, corruption and duty. Beginning with civic education, I test its relationship with duty by means of multilevel models, which I fit to data from the 2016 International Civic and Citizenship Education Study (ICCS). I use these models because they allow estimating the effect of variables that are at different levels (Snijders and Roel 2012). In my case, civic education measures are aggregated at the school level. This aggregation reduces the risk of reverse causation, and it likely helps to reduce measurement error in the civic education variables. Besides allowing the estimation of the effect of variables from different levels, multilevel models also account for the fact that observations are not independent from each other (Snijders and Roel 2012). Some respondents in the ICCS dataset belong to the same school or country. Hence, given the advantages of using

⁴ Some authors disagree, however, about the use of structural equation models in stability tests (e.g. Zaller and Feldman 1992). They argue that random change could be due to ambivalent considerations, and, in this sense, random change would not necessarily mean measurement error. In what follows, I do not discuss this alternative interpretation of random change as it is beyond this study’s scope.

multilevel models, I follow prior civic education work (Claes, Hooghe, and Stolle 2009; Dassonneville et al. 2012), and I test the education-duty nexus with multilevel models.

In the analysis of the relationship between compulsory voting and duty, I use yet another estimation strategy. More specifically, I leverage the abolition of compulsory voting rules in Chile, in 2012, and I estimate the effect of compulsory voting on duty. This quasi-experimental setting is examined by means of synthetic control models, which represent a variation of the difference-in-differences approach in which the control unit is created from donor countries based on variables that generally predict the outcome variable. Synthetic control models are recommended when there is a potential violation of the parallel trend assumption (Abadie, Diamond, and Hainmueller 2015). Exploring the duty trends in all Latin-American countries in the 2006, 2007, 2008, 2009, 2010, 2011 and 2015 LatinoBarometers, I find no country that could serve as a good counterfactual for Chile, as no country in those datasets have similar duty trends before Chile's abolition of the compulsory voting rules.

Finally, to study the connection between corruption and duty, I use a fourth modeling strategy. More specifically, I examine the effect of corruption on duty by means of Baron and Kenny's (1986) and Imai, Keele and Tingley's (2010) mediation models.⁵ Baron and Kenny's mediation approach entails a sequence of three regression models, first testing the connection between an independent variable and a mediating variable, then the connection between an independent variable and dependent variable, with and without the mediating variable in the model. If the effect of an independent variable weakens with the introduction of a mediating variable, it means that there is actually a mediation effect by that variable. Otherwise, it means that there

⁵ I use these models because they not only provide information about how much corruption affects duty, but also how much corruption affects turnout through duty. In these models, corruption represents the independent variable, duty the mediating variable, and electoral participation the dependent variable.

is no mediation effect. Given the risk of making false inferences by comparing effects across different models, I also use an alternative mediation approach, outlined in Imai, Keele and Tingley (2010). This approach allows estimating a mediation effect within a single model (see Becher and Brouard, 2020, and Bormann et al., 2019, for recent uses of it).

In conclusion, with these different estimation strategies, I am confident that I can provide valid estimations of the stability of duty, as well as the effect of civic education, compulsory voting, and corruption on duty. I examine the stability of duty through structural equation models, which allow accounting for error in the duty measures. I assess the effect of civic education on duty through multi-level models, given that the variables of interest are not at the same level of analysis, and that some observations are not independent between themselves. I explore the link between compulsory voting and duty in a quasi-experiment by means of synthetic control models because of the potential violation of the parallel trend assumption. And I investigate the influence of corruption on duty by means of a more conventional mediation approach, and a more recent approach that addresses the issue of comparing estimates across models. Having discussed the methods that I use in the following empirical analyses, in the next section I outline the four chapters representing the body of this dissertation.

6. Dissertation chapters

As indicated, this dissertation has two main goals. The first goal is testing the stability of civic duty. The second goal is testing how much duty is affected by civic education, compulsory voting, and corruption. In what follows, I indicate how these goals are addressed in this dissertation, and I outline the main findings from each empirical chapter.

In Chapter 1 I examine how stable is civic duty. I estimate the stability of duty with structural equation models that I fit to nine-wave panel data from two studies: the 2010-2015 British Panel Election Study, and an original panel study conducted between 2011 and 2016 in Spain. I find that civic duty is a very stable attitude, much like partisanship and political interest (Green et al. 2004; Prior, 2010). With these findings, I conclude that there is little rationalization in duty and, consequently, civic duty likely represents a meaningful attitude.

In Chapter 2 I examine the influence of civic education on the development of duty. I test this relationship by means of multilevel models that I fit to data from the 2016 International Civic and Citizenship Education Study (ICCS). My analyses show that the three most common forms of civic education (civics courses, active learning strategies, and open classroom environment) all contribute to predicting duty, but that civics courses exert the most consistent effect. In this way, Chapter 2 provides evidence that exposure to civic education provides one explanation for why some individuals feel a duty to vote while others do not.

In Chapter 3 I move to investigating the influence of compulsory voting on duty. I assess the link between compulsory voting and civic duty through a quasi-experiment. Specifically, I examine if individuals' sense of duty to vote weakened after the abolition of compulsory voting rules in Chile, in 2012. For this analysis, I use a synthetic control model and cross-sectional data from the 2007, 2008, 2009, 2010, 2011, and 2015 Latinobarometers. I find that compulsory voting bolsters duty. In this way, Chapter 3 suggests that institutions – more precisely, compulsory voting – also account for the individual-level variance in civic duty.

In the fourth and final chapter of this dissertation, I examine whether individuals' sense of duty is sensitive to corruption. I investigate the connection between corruption and civic duty by

means of Baron and Kenny's (1986) and Imai, Keele and Tingley's (2010) mediation tests (where corruption is the independent variable, civic duty the mediating variable, and electoral turnout the dependent variable). In these tests I use election-specific duty measures from the MEDW project. The mediation tests suggest that individuals are not much more likely to consider voting a duty in corrupt than non-corrupt contexts and, consequently, that duty exerts a weak mediation effect. As such, Chapter 4 suggests that, unlike civic education and compulsory voting, corruption plays a marginal role in the development of duty.

With these findings, this dissertation advances important knowledge on the stability of duty, and the impact of three contextual-level factors on individuals' sense of duty to vote. More precisely, I show that duty is very stable, implying little rationalization. In addition, I show that civic education and compulsory voting contribute to the development of duty, but that corruption plays a marginal role. In the conclusion, I discuss the implications of these findings, the limitations of this research, and suggest potential avenues for future research.

Chapter 1

How stable is sense of civic duty to vote?

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1. Introduction

Since Riker and Ordeshook (1968) first modified the voter utility function to include “duty” as one of the terms, civic duty has been characterized as “the psychological benefit derived from following the injunctive norm of voting” (Gerber and Rogers 2009: 181). Research has provided strong evidence that civic duty represents a key turnout predictor (Harder and Krosnick 2008; Blais 2000; Blais and Achen, 2019; Campbell et al. 1960; Clarke et al. 2004).

Work that includes duty among the predictors of electoral participation has generally assumed that civic duty is antecedent to turnout, meaning it affects voting behavior rather than being affected by it. Some studies challenge that view, arguing that when asked in the context of a survey, individuals rationalize their perception of the duty to vote based on their previous participation, or not, in elections (Dowding 2005; Matsusaka and Palda 1999). In this study, we assess the temporal stability of the sense of civic duty and, by doing so, evaluate whether the public’s perception of civic duty does constitute a plausible antecedent to voting or not.⁶

In studying the stability of the sense of civic duty to vote, we also make an important contribution to the characterization of this political attitude. By knowing whether and to what extent individuals change their beliefs in the duty to vote over time, we should have a better sense of whether short-term factors (beyond voting) have any influence on it. The implications of our research should be of particular interest for scholars studying the effects of civic campaigns and the possibility that adherence to the voting norm among new generations – which has been found to be very low – might improve in the short term (Dalton 2007, 2008).

⁶ For a test with a turnout model, see Galais and Blais (2016a). See also Kosmidis (2014) for a test on the stability of the duty-turnout link; specifically, whether and the extent to which the effect of civic duty on turnout increases as the election day looms or not.

Drawing on past studies on attitudinal stability, we test the stability of the sense of civic duty empirically by means of a structural equation model that accounts for measurement error. We fit that model to data from the 2005–2010 British Election Panel Study and the 2010–2017 Spanish Democracy, Elections and Citizenship Panel Study. To our best knowledge, these are the studies with the largest number of waves (nine) that include a civic duty measure, thus allowing a stringent measurement of the stability of civic duty to vote.

Our results suggest that civic duty attitudes are very stable over time, a finding that remain valid in the case of different survey samples, civic duty measures, elections, inconsistent voting behavior, model specifications, and between-wave intervals.

2. The stability of the sense of civic duty to vote

Attitude stability gained attention as a research subject with Converse's work (1964), in which he found relatively high temporal stability for partisanship but low stability for ideological stances. Stability – understood as a resistance to change-inducing stimuli (Erber et al. 1995) – has since become a core attitudinal trait that indicates how well an attitude is able to predict a subsequent behavior (Doll and Ajzen 1992; Glasman and Albarracin 2006; Prislin 1996; Schwartz 1978). As such, several studies from the political behavior literature have tested and found evidence of high stability among key attitudinal political behavior predictors, from partisanship to political interest (Green and Palmquist 1994; Green, Palmquist and Schickler 2004; Markus 1979; Miller 1991; Prior 2010, 2018; Schickler and Green 1997).

There is, however, no compelling evidence about the stability of the sense of civic duty to vote. Prior work, such as Blais and Achen (2019), Campbell (2006), and Blais and Young (1999), does not rely on large cross-national panel data, or account for measurement error when assessing the stability of sense of civic duty. As a result, it remains unclear how civic duty is connected with turnout. More specifically, while most researchers assume that civic duty antecedes turnout (Blais 2000; Clarke et al. 2004; Galais and Blais 2016b; Riker and Ordeshook 1968; Verba, Schlozman and Brady 1995), others argue that, in fact, individuals rationalize their sense of duty to be consistent with their electoral behavior, which seriously challenges the role of duty as a turnout predictor (Dowding 2005; Matsusaka and Palda 1999). Shedding light on the stability of the civic duty to vote will not only solve this discrepancy, but also clarify the potential effects of civic campaigns, and whether civic duty can increase, in the short term, from its current low levels for new generations.

There are good reasons to expect high stability. First, affective attitudes are known for being stable (Almond and Verba 1963; Krosnick 1991). Given that civic duty derives from an affective attachment toward one's community, it qualifies as an affective attitude and, thus, it is likely stable (Blais and Galais 2016; Galais and Blais 2016b; Hur 2020). Second, due to its moral nature (Blais and Galais 2016), civic duty is likely to predispose individuals against divergent views (Skitka, Washburn and Carsel 2015), thus hampering attitudinal change. Third, because dutiful subjects constitute the majority of the population (Bolzendahl and Coffé 2013; Dalton 2008; Rolfe 2012; Sinclair 2012), they might have low chances of encountering change-inducing stimuli, resulting in a high stability of duty. Fourth, twin studies report a genetic influence on duty to vote (Loewen and Dawes 2012), and personality traits have also been

found related to duty (Blais and St. Vincent 2011; Weinschenk 2014), all of which suggests that sense of duty to vote is an early formed, change-resistant attitude.⁷

On the other hand, some scholars suggest duty might not be very stable. For example, Bowler and Donovan (2013) posit that, given that civic duty entails a social contract between the state and citizens, “any perceived misconduct in the political system should erode a sense of duty among voters” (Bowler and Donovan 2013: 269), therefore undermining its stability. In the same vein, Galais and Blais (2014) detect a small decrease in the level of young Spaniards who felt a duty to vote during the Great Recession. Putting those two different perceptions regarding the stability of civic duty to the first rigorous empirical test, our study adjudicates between them, providing evidence of the stability, or instability, of that political attitude over time.

3. Estimation approach

Ever since Achen (1975) reported that Converse (1964, 1970) had overestimated the degree of attitude change by ignoring measurement errors, studies on attitude stability have made use of structural equation modeling (Green et al. 2004; Krosnick 1991; Prior 2010, 2018).⁸ Following Achen’s and Wiley and Wiley’s (1970) lead, we employ a two-stage model in which we first regress observed duties on a group intercept,⁹ as well as their corresponding latent duties and

⁷ Additionally, research has identified links between civic duty and stable political attitudes like political interest (Carreras 2018; Prior 2010).

⁸ Importantly, as these models account for measurement errors at the individual level, our study concentrates on civic duty stability at the individual level.

⁹ Given the increase in civic duty mean across waves (from 3.93 to 4.44 in the BEPS dataset, and from 2.12 to 2.58 in the DEC dataset, on a 1–5 and a 1–4 scale, respectively), we follow Kline (2016) and add a group intercept in our measurement equation. Adding a group intercept also at the lag-1 equations stage yields identical stability coefficients, while it augments the fit values (for the worse).

a (measurement) error term, and, subsequently, each latent duty from the first stage on its antecedent latent duty and a disturbance term. Our model can be expressed mathematically as:

$$y_t = v_t + \lambda_t * \mu_t + \epsilon_t \text{ (First stage: Measurement stage)}$$

$$\mu_t = \delta_t, \text{ when } t=1 \text{ (Second stage: Lag-1 equations stage)}$$

$$\mu_{t+1} = \beta_t * \mu_t + \delta_{t+1}, \text{ when } t=2, 3, \dots, T \text{ (Second stage: Lag-1 equations stage)}$$

where y_t refers to observed duty, v_t , to group intercept, λ_t , to factor loading, μ_t , to latent duty, ϵ_t , to measurement error, β_t , to stability coefficient, and δ_t , to disturbance term, all at time t . The degree of civic duty's stability is measured in our model by the stability coefficients, which have a minimum value of zero, and maximum value of one. If close to the minimum value (i.e. zero), duty is very unstable; if close to the maximum value (i.e. one), duty is very stable. As such, our model provides a stability test of duty that is sensitive to measurement error.

Scholars often struggle with model identification when using the same or a similar structural equation model. With only a few degrees of freedom, they force the error terms to be equal, and do not allow them to covary. With many degrees of freedom due to a large number of panel waves (nine), we estimate stability coefficients through a baseline model with only one of those constraints (error terms do not covary). Still, we also estimate stability coefficients using an alternative model that does not impose any of those constraints on the error terms.¹⁰

¹⁰ Less contested by the literature, we follow influential studies like Achen (1975) and Wiley and Wiley (1970) and assume that observed duties and latent duties are measured on the same scale, that disturbance terms do not covary among themselves or with latent duties from other waves, and that error terms do not covary with latent variables or with disturbance terms.

To assess the stability of sense of civic duty, we make use of data from the 2005–2010 British Election Panel Study (BEPS), and original data from the 2010–2017 Spanish Democracy, Elections and Citizenship (DEC) Panel Study. We chose these two studies as they have measured duty nine times, thus allowing the estimation of stability coefficients over multiple time-points, with few model constraints, and over relatively long intervals (5 and 7 years).

In addition, these studies include civic duty measures from before, during, and after two British and two Spanish general elections, when post-hoc rationalization of the act of voting – and, thus, change – is most likely to occur (see Appendix A.1 for fieldwork date of each BEPS and DEC wave). Moreover, the Spanish sample includes young people – specifically, between 15 and 45 years old –, who are likely more open to attitudinal change (Krosnick and Alwin 1989).

The BEPS study and the DEC study further differ with respect to the way in which they assess respondents' civic duty to vote, allowing an even a stringent test of civic duty stability. The BEPS question states: “I would be seriously neglecting my duty as a citizen if I didn't vote”, which is followed by an agree-disagree scale. In contrast, the DEC question reads: “For some people voting is mostly a duty. They think they should vote regardless of their views on the parties. For others, voting is a choice. They decide whether to vote or not according to their opinions about the parties. For you, is voting first and foremost a duty or a choice?”. Those who answered “a duty” were subsequently asked how strongly they felt that voting is “a duty”: “not very strongly,” “somewhat strongly,” or “very strongly.”¹¹ Both of these civic duty

¹¹ To give an overview of the distribution, most respondents in the BEPS dataset either “strongly agree” or “agree” with the statement (in W1 the distribution is 38% and 35%; in W9, it is 68% and 18%). By contrast, most respondents in the DEC dataset answer “a choice” to the civic duty question (in W1, 53% do so, whereas 3% answer “not very strongly”, 22% “somewhat strongly”, and 22% “very strongly” a duty; in W9, 37% report that voting is a choice, whereas 1% answer that voting is “not very strongly” a duty, 29% “somewhat strongly” a duty, and 33% “very strongly” a duty). Following past work (Prior

measures have been equally used by the civic duty literature (e.g. Blais and Achen 2019; Kosmidis 2014; Sanders et al. 2007; Schuck and de Vreese 2015).

It is important to note that the BEPS dataset considers 1,524 individuals. In contrast, the number of Spaniards that were retained after nine panel waves in the DEC dataset was only 204. While the response rate in the DEC study (26%) fits the Spanish pattern – the Spanish Labour Force Survey retention rate is, for example, 19.8 –, and most Spanish respondents do not consider voting a duty, but rather a matter of personal choice, our results from the DEC data should be construed as additional evidence regarding civic duty stability, or instability.

4. Results

Table 1.1 presents the coefficients from our individual stability tests. Supporting the “stability” argument, we find that civic duty attitudes are very stable. With a good model fit,¹² stability coefficients are often above .94, that is, close to the maximum possible value of 1.00. In a few instances (e.g. column 1, W5–W6 and W6–W7; column 3, W7–W8 and W8–W9), the stability coefficients are in fact 1.00. Importantly, those coefficients differ only marginally across our British and Spanish datasets, thus, across different civic duty measures and survey samples.¹³

2010), we transformed these civic duty measures to a 0–100 scale in order to avoid negative variances. Also, because these measure include a five-point or a four-point response options, stability coefficients are estimated by means of a robust maximum-likelihood estimator (Prior 2010).

¹² The literature usually considers a good model fit when the Santorra–Bentler adjusted chi-squared is associated with an insignificant (and high) p -value, and when the comparative fit index (CFI) passes the .95 threshold. It is also important that the root mean square error of approximation (RMSEA) is lower than the .06 cutoff point, and that the upper limit of its confidence intervals does not surpass the .08 threshold (Hu and Bentler 1999).

¹³ A marginal difference is also observed between the results from structural equation models and those from Goodman–Kruskal gamma-type rank correlations, in which errors in the duty measures are not taken into account, and without listwise deletion of observations (see Appendix A.2).

Table 1.1. Civic duty stability as indicated by stability coefficients from baseline and alternative structural equation models (without and with covariance between error terms)

	2005-2010 BEPS (Waves 1-9)		2010-2017 DEC (Waves 1-9)	
	Baseline model (no covariance between error terms)	Alternative model (covariance between error terms)	Baseline model (no covariance between error terms)	Alternative model (covariance between error terms)
W1-W2	.99	.98	.99	1.00
W2-W3	.98	.99	.97	.95
W3-W4	.96	1.00	.94	.98
W4-W5	.92	.91	.97	1.00
W5-W6	1.00	1.00	.95	.97
W6-W7	1.00	1.00	.96	.95
W7-W8	.92	.92	1.00	1.00
W8-W9	.94	.95	1.00	1.00
Var ϵ_1	190.84	196.33	634.17	667.61
Var ϵ_2	128.56	163.25	493.44	547.10
Var ϵ_3	130.25	146.83	403.16	536.53
Var ϵ_4	178.66	168.14	424.72	497.79
Var ϵ_5	184.89	189.38	513.27	505.02
Var ϵ_6	175.59	178.57	366.65	370.35
Var ϵ_7	151.86	154.69	480.77	531.21
Var ϵ_8	84.83	114.74	490.50	594.02
Var ϵ_9	91.84	111.26	348.29	390.20
Santorra-Bentler adjusted χ^2	40.38	15.06	16.07	8.02
df	19	11	19	11
p-value	.003	.18	.65	.71
CFI	1.00	1.00	1.00	1.00
RMSEA	.04	.02	.00	.00
[90% c.i.]	.02;.06	.00;.05	.00;.07	.00;.08
N	1,524		204	

Notes: Stability coefficients are unstandardized. Values close to zero indicates instability; values close to one, stability. Different intervals separate two adjacent panel waves. Stability coefficients obtained with both baseline and alternative models suggest a high stability of sense of civic duty. Fit statistics indicate a good model fit particularly with the alternative model specification.

We also observe that the stability coefficients are close to their maximum value (1.00) in electoral periods. Considering pre-election/campaign intervals, the stability coefficients are .99 (BEPS W1–W2), .92 (BEPS W7–W8), .97 (DEC W2–W3), and 1.00 (DEC W7–W8). Regarding campaign/post-election intervals, stability coefficients are .98 (BEPS W2–W3), .94 (BEPS W8–W9), .94 (DEC W3–W4), and 1.00 (DEC W8–W9). Importantly, while the alternative model specification (with covariance between the error terms) yields a slightly

better fit, it yields practically identical conclusions: the stability coefficients are often high and close to 1.00 (see Table 1.1, columns 2 and 4).

While these results weaken the argument that turnout causes civic duty, we went further and tested if civic duty is also stable among individuals who are most likely to change their sense of duty based on their voting behavior, namely, those who changed from voting to abstaining between elections. The results are consistent with our previous findings. While a lower stability coefficient follows the 2010 election, suggesting some rationalization, it is still high: .83 – or .77 with the alternative model specification (see Appendix A.3).

We also tested the stability of the sense of civic duty with a slightly larger Spanish sample ($n = 376$) resulting from considering only 8 DEC panel waves. Our results were essentially the same. That is, although the stability coefficient from the W2–W3 (pre-election/campaign) interval dropped .09 (from .97 to .88), it remained close to 1.00 (see Appendix A.4).

Last, we note that, consistent with past work (Erikson, 1979), the stability coefficients are not affected by the time elapsed between panel waves. Indeed, the stability coefficient obtained in the largest interval of the BEPS (i.e. 25 months) is .92, only .04 lower than the coefficient obtained in a 12-month interval (column 1, W4–W5 and W3–W4). Similarly, the DEC surveys conducted 12 months apart (DEC waves 5–9) yield approximately the same stability coefficients as the DEC surveys separated by only 6 months (DEC waves 1–4).

5. Discussion and conclusion

Our research estimated civic duty stability by means of a structural equation model. With data from two different datasets, the 2010–2015 BEPS and the 2010–2017 DEC, containing different survey samples, and civic duty measures, we found evidence that civic duty is likely to be a very stable attitude. That finding was robust also for elections, inconsistent voting behavior, different model specifications, and between-wave intervals.

Our study is limited in some respects. While we have shown that civic duty attitudes are very stable up to a 25-month interval, they may be less so over longer periods. Furthermore, our results might be only indicative of stable civic duty responses.

Still our findings have important implications for the literature on electoral behavior and political attitudes. First, while some studies argue that duty reflects a post hoc rationalization of the act of voting, which makes feelings of duty a poor predictor of turnout, our findings suggest that civic duty is a very stable attitude – as are partisanship (Green et al. 2004) and political interest (Prior 2010, 2018). Hence, civic duty likely represents a plausible antecedent of turnout, implying civic duty would affect turnout, not the reverse.

Furthermore, our findings give support to the view that individuals are marginally affected by appeals to their civic duty, insofar as their aim is to change civic duty attitudes (Geys 2006). At best, these attitudes might be activated (but not “boosted”) by these appeals. Moreover, our findings suggest that the lower adherence to the voting norm that is observed among new generations of citizens might be permanent and is not sensitive to life cycle or contextual

effects. As such, maybe the educational efforts of the authorities that are concerned about civic duty and turnout should concentrate on childhood and early adolescence.

Chapter 2

Does civic education foster civic duty to vote?

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1. Introduction

Voter turnout has declined in many countries. A lower turnout is problematic as it may lead to an unequal representation of citizens' preferences in politics (Dassonneville and Hooghe 2017). Many scholars have, for this reason, investigated what explains citizens' electoral participation. They have found that the decision to vote in elections is strongly determined by a sense of duty (Smets and van Ham 2013; Blais 2000; Blais, Young and Lapp 2000; Clarke et al. 2004); that is, individuals who feel a sense of duty are much more likely to vote than their counterparts.

Given the importance of duty for voting, many scholars have investigated where sense of duty comes. It has been shown that duty is in part developed in the school. More specifically, Galais (2018) offers empirical evidence that exposure to civic education in the school can contribute to the development of civic duty. Still her study misses some important mechanisms through which school socialization may occur (e.g. civics courses).¹⁴ As a consequence, we not only have a limited understanding of what drives duty to vote but also do not know the relative impact of different forms of civic education on sense of duty.¹⁵

¹⁴ In this article, I follow Dassonneville et al. (2012) in the way that I define civics courses, active learning strategies and open classroom environment. Civics courses represent a method of civic education that involves classroom-based instruction about issues related to politics and community affairs, such as how citizens can vote in local or national elections and how one can contribute to solving community problems. Civic education through active learning strategies relates to students' involvement in democratic procedures of schools, such as by voting for a class representative or by participating in a debate about how the school is run. And open classroom environment concerns the climate in which students are "encouraged to develop and express their own views" in school, such as whether teachers encourage students to make up their own minds and whether teachers present several sides in a debate (Dassonneville et al. 2012: 141).

¹⁵ Stadelmann-Steffen and Sulzer (2018) demonstrate empirically the importance of looking at different socialization mechanisms in schools. In their work, a focus on the knowledge dimension (which resembles civics courses) of civic education reduces individuals' political interest. By contrast, a focus on the skill dimension (which resembles active learning strategies) augments their interest. As a result,

Following prior socialization studies (e.g. Milner 2008), in this article I conduct a systematic cross-country analysis of the link between three forms of civic education (civics courses, active learning strategies and open classroom environment) and civic duty. To this end, I use the pooled data from the most comprehensive study of adolescents' experience with civic education: the 2016 International Civic and Citizenship Education Study.

My analyses suggest that those three forms of civic education are all correlated with civic duty but that civics courses are by far the most influential civic duty determinant. These results are confirmed in subsequent country-specific analyses: while civics courses are significantly correlated with civic duty in a majority (61%) of the countries in the data, active learning strategies are so in less than a tenth (9%), and open classroom environment are so in slightly more than a third (39%) of the countries. With these findings, this study advances knowledge on the means through which schools can spur a sense of duty and also suggests the relative effect of each mechanism of school socialization on civic duty.

The article is organized as follows: First, I discuss the theoretical relationship between civic education and civic duty and identify the hypothesized mechanisms connecting the two. Then I present the 2016 ICCS data, as well as the measures of my variables. In the subsequent sections I present the methodology, the main results and the results from supplementary tests. I conclude with a discussion of the main findings and what we can learn from them.

if we focus on a specific type of civic education, we may miss an overall understanding of the link between civic education and duty, and the relative effect of each form of civic education on duty.

2. Civic education and sense of civic duty to vote

Within the civic education literature, we can find many indications that civic education can foster sense of duty. For example, several studies have demonstrated that hands-on experiences with politics – whether through a legislative advocacy day (Beimers 2016), an engagement with local civil-society organizations (Turner, 2014) or extracurricular political activities in college (Simmons and Lilly 2010) – can foster civic skills, which in turn may lead to a sense of duty to vote. In addition to these studies, Huerta and Jozwiak (2008) have shown that the introduction of the New York Times as class material can yield an improvement in students' attitudes toward community involvement, which may affect sense of duty. In short, a review of the literature suggests a link between active learning strategies and civic duty.

Other studies suggest, as well, that open classroom environment and civics courses may instill a sense of duty to vote. For example, using data from the Belgian Political Panel Survey (BPPS), Dassonneville et al. (2012) have shown that experience with an open classroom environment can lead to a sense of political trust. The authors have also shown that learning about politics in a formal setting can foster an interest in political affairs. “Dutiful” citizens (those for whom voting in elections is a duty) are likely to trust the political system and politicians and to be very interested in political affairs (Galais and Blais 2017; Carreras 2018). Consequently, we may conclude that those two forms of civic education can foster duty too.

Galais (2018) represents the first exploration of the link between civic education and civic duty. In her work, Galais gives many reasons for an effect of civic education on duty. First, referring to the social reproduction theory, Galais defends the idea that the school serves as an incubator of social norms to new generations of citizens; that is, through the school, individuals learn

how to behave socially (Bourdieu 1977; Dennis 1968). Given that voting constitutes a social norm in most democracies (Bolzendahl and Coffé 2013), it is quite likely that civic education will transmit this norm and consequently foster an attitude of civic duty among adolescents.

Second, Galais argues that schools can instill duty in more indirect ways too. She suggests that schools may engender a sense of civic duty by conveying that it is important to take an active role in the democratic process and that people in a given community must fulfil their social obligations for the sake of a greater good (see Flanagan et al. 1998; Torney-Purta et al. 2001; Mosher, Kenny and Garrod 1994; Pasek et al. 2008). Furthermore, schools may foster a sense of civic duty by cultivating a language of duties and a sense of community attachment (see Macaluso and Wanat 1979; Burtonwood 2003; MacMullen 2004).

To prove a relationship between civic education and civic duty empirically, Galais explores the data from the 1994–2008 Canadian National Longitudinal Survey of Children and Youth (NLSCY). Her multivariate logistic regressions focus on the effect of democratic governance on youngsters' sense of duty. Her findings are encouraging, as they suggest a significant correlation between civic education (democratic governance) and civic duty.¹⁶ However, whether these findings are present for different forms of civic education remains an important question that no study has since explored with a more comprehensive dataset. This article performs this role by conducting a systematic cross-country analysis of the effect of civics courses, active learning strategies and open classroom environment on civic duty.

¹⁶ Galais (2018) defines (and operationalizes) democratic governance as the involvement of the staff and teachers in school policies and in how the resources are allocated.

3. Data and measures

In this article, I rely on the pooled data from the 2016 International Civic and Citizenship Education Study (ICCS) to test whether the connection between civic education and civic duty is present for different forms of civic education. To my knowledge, the 2016 ICCS represents the most comprehensive study of adolescents' exposure to civic education in the school, with information from 86,914 eighth-graders who are enrolled in 3,671 schools from 23 countries (see Appendix B.1 for a list of countries and for the number of respondents and schools in each of them).¹⁷ In addition, I make use of the comparative nature of the 2016 ICCS to investigate the number of countries in which a relationship between each form of civic education and civic duty is present, and, as such, gain an even greater understanding of whether and the degree to which different forms of civic education affect civic duty.

To test the link between different forms of civic education and civic duty, it is key that the dependent variable (civic duty) is well measured. The civic duty question in the 2016 ICCS dataset ("For you, how important are the following behaviors for being a good adult citizen? Voting in every national election") seems to fulfil this requirement, as the distribution of respondents (for 82%, voting is a duty; for 18%, voting is not a duty) resembles the distribution reported in other studies (Bowler and Donovan 2013; Weinschenk, 2014). It is also reassuring that previous work has relied on a similar type of civic duty measure (Dalton 2008; Klemmensen et al. 2012; Marien et al. 2010). (see Appendix B.2 for descriptive statistics of civic duty and of all other variables in the analyses).

¹⁷ In all these countries, a sample of students and schools were randomly drawn from the entire list of students and schools in the territory, except in Belgium, China and Germany, where only students and schools from Flanders (Belgium), Hong Kong (China) and North Rhine-Westphalia (Germany) were randomly selected.

As for the main independent variables, the 2016 ICCS measures different forms of civic education in a comprehensive and methodologically rigorous way by employing a total of 3 questions and 19 survey items. One question taps into what extent students were taught seven different civics topics in class. Another question measures whether students performed six different political activities in school, in the last 12 months. Yet another question captures students' experience with six different aspects of an open classroom environment (see Appendix B.3 for the three civic education questions and their corresponding items).¹⁸

In order to reduce these data to a unidimensional measure of different forms of civic education, I perform three principal component analyses (PCAs) to each of those three questions. A single eigenvalue crossing the 1.00 threshold is obtained: the highest eigenvalues are 3.32, 2.35 and 2.90, while the second highest eigenvalues are 0.87, 0.89 and 0.80, leading to the formation of three indices (the “learning,” the “participation” and the “openness” indices) (see Appendix B.3 again for the factor loading and uniqueness value of all items with respect to those indices).

Relying on these indices may be problematic, however, if some students in the data overestimate or underestimate their experience with civic education and if some students report an experience with civic education because of an a priori sense of civic duty to vote. Fortunately, the 2016 ICCS provides information on respondents' school, which I use to aggregate the civic education indices and consequently render the three indices and the regression estimates more reliable (for a similar approach to dealing with potential individual-level biases, see Page and Shapiro 2010, and Larcinese, Snyder and Testa 2013).

¹⁸ Importantly, the high internal reliability (0.81, 0.68, and 0.78) between the items in each question means that they are internally coherent and that they capture a single form of civic education.

In addition to aggregating the three indices, I standardize them to a mean of 0 and a standard deviation in order to be able to compare the effect of the three forms of civic education on civic duty (see Appendix B.4 for the indices' final distribution).

4. Method

This article is aimed at testing whether and the degree to which three forms of civic education affect duty. To achieve that goal, I start by examining the relationship between civic duty and the three civic education indices – first individually and then jointly – with the pooled 2016 ICCS data. Because the civic education indices correspond to school-level aggregates, I run multilevel models, nesting individuals within schools. To account for the country-nested structure of the data, I include country fixed effects and use country-clustered standard errors.

Using a multilevel model to examine the correlation between different forms of civic education and civic duty means that controlling for potential (individual-level) confounders is crucial. Notably, it is key that I control for parents' political interest and occupational status (measured in the 2016 ICCS based on the classification from Ganzeboom et al. 1992), as those who are politically interested and who come from a wealthy environment might choose a school with a strong civic education program and might consider voting a civic duty more than do their counterparts.¹⁹ It is also important that I control for respondents' gender, as male students might attend a school with a strong civic education program and might consider voting a civic duty less than female students do (Hooghe and Stolle 2004; Carreras 2018).

¹⁹ As in this article, previous work has relied on parent measures that are based on students' answers to survey research – e.g. see Nadeau et al. (1995).

Most of what we know about experiences with civic education comes from single case studies (see, for example, Hart et al. 2007; Kahne and Sporte 2008; Flanagan and Stout 2010; Galais 2018). Yet the education-duty nexus may play out differently in different contexts, not least because the quality of civic education, including the capacity to get students interested in the content of the civic education, varies between schools (Martin 2012; Fahmy 2006; Milner 2010). For this reason, in addition to an analysis of the cross-national pooled ICCS data, I examine the relationship between civic education and civic duty with country-specific data, which allows for a more detailed assessment of that relationship. More precisely, I perform 23 multilevel regressions, each of which correspond to a country in the 2016 ICCS. As in the main tests with the pooled 2016 ICCS data, these multilevel regressions contain control variables for parents' political interest and social status, as well as respondents' gender. Because these regressions focus on a single country each time, there is no need to add country fixed effects or to cluster the standard errors by country, as in the analysis with the pooled data.

5. Results

To test whether the link between civic education and civic duty is present for different forms of civic education, I run multilevel regressions of civic duty on the three civic education indices, first individually and then jointly with the pooled dataset, and then run multilevel regressions of civic duty on the same indices in each country.

Table 2.1 lists the results of the pooled analyses. They show that a sense of civic duty is higher among adolescents who are exposed to any of the three forms of civic education than those who are not, and this is so even in the more comprehensive model in which the effect of other

forms of civic education is taken into account (see Table 2.1, columns 1–4). In other words, individuals who are exposed to civics courses, to active learning strategies and to an open classroom environment in adolescence are more prone to consider voting a civic duty than are those with a different school experience.²⁰ The results in Table 2.1 further, and reassuringly, show that the coefficients for the control variables are in line with expectations. Those who are female, whose parents are interested in politics and whose parents are of a high occupational status are more likely to report a belief in the duty to vote than their counterparts.

Table 2.1. Effect of different forms of civic education on sense of civic duty to vote

	DV: Civic Duty			
	Model 1	Model 2	Model 3	Model 4
Civics Courses	0.332*** (0.017)			0.221*** (0.020)
Active Learning Strategies		0.207*** (0.016)		0.090*** (0.017)
Open Classroom Environment			0.266*** (0.015)	0.140*** (0.017)
Political Interest: Parents	0.610*** (0.025)	0.615*** (0.025)	0.613*** (0.025)	0.602*** (0.025)
Occupational Status: Parents	0.006*** (0.001)	0.006*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
Female	0.134*** (0.020)	0.126*** (0.020)	0.118*** (0.020)	0.123*** (0.020)
Constant	1.575*** (0.074)	1.420*** (0.075)	1.354*** (0.073)	1.585*** (0.074)
Fixed Effects: Country	Yes	Yes	Yes	Yes
N	79,888	79,886	79,885	79,885

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within schools. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. Higher values of the civic education indices indicate higher levels of a form of civic education. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

²⁰ Because cumulating civic education mechanisms may lead to an even higher effect on sense of civic duty, I interact the three civic education indices in a pairwise setup. These interactions turn out statistically insignificant in two out of three cases. The sole significant interaction (of the participation and the openness indices) is associated with a very weak coefficient (see Appendix B.5).

Given the difficulty of interpreting log-odds, I compute average marginal effects to assess how much civic duty is affected by each civic education mechanism and to compare their effects. They indicate that civics courses are by far the most influential civic duty determinant: an increase of one standard deviation in the learning index is associated with a 2.9 percentage point increase in the likelihood of considering voting a duty, while an increase of one standard deviation in the participation and in the openness indices is associated with only a 1.2 and a 1.8 percentage point increase, respectively (see Table 2.2). In short, these results suggest that civics courses are more correlated with civic duty than the other forms of school socialization.²¹ The effect of civics courses is particularly strong considering that school accounts for 14 per cent of the variance in civic duty in a null model without covariates (not reported).

Table 2.2. Average marginal effect of different forms of civic education on sense of civic duty to vote

	DV: Civic Duty
Civics Courses	0.029*** (0.003)
Active Learning Strategies	0.012*** (0.002)
Open Classroom Environment	0.018*** (0.002)
N	79,885

Notes: entries report the change in the predicted probability of considering voting a civic duty. The corresponding log-odds can be seen on Table 2.1, column 4.

The test of the link between civic education and civic duty by means of the pooled 2016 ICCS data indicates a stronger relationship between civics courses and civic duty than other civic education mechanisms. To gain a better understanding of those relationships, I turn to analyses of the country-specific 2016 ICCS data. The key question is now how often each form of civic

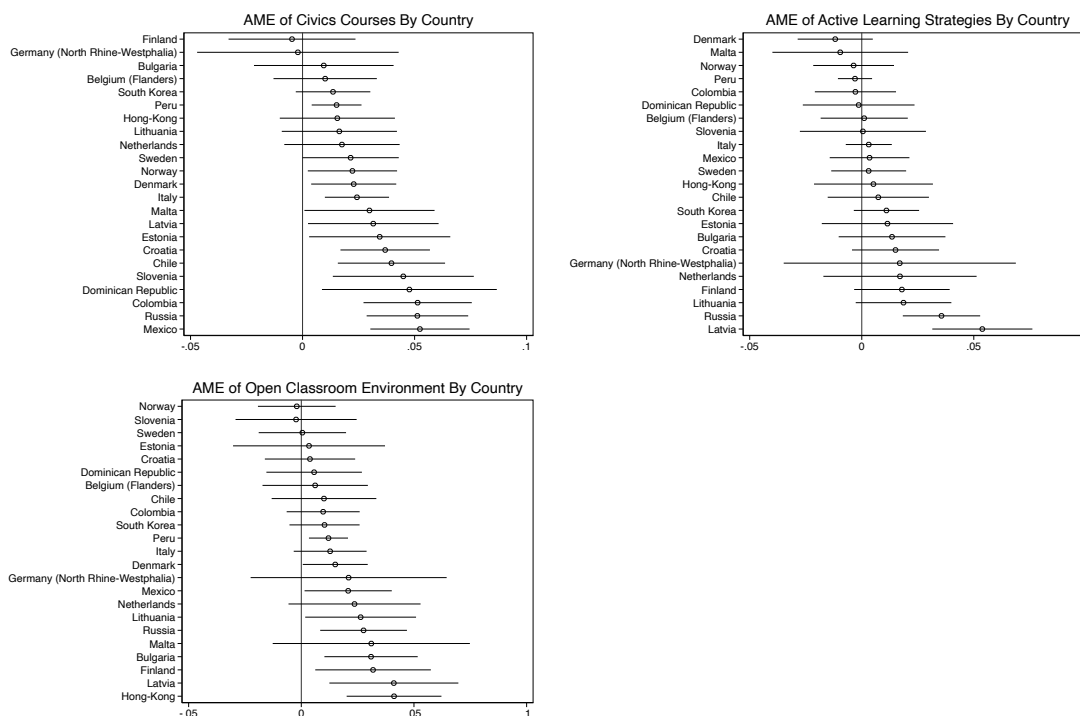
²¹ Wald tests confirm that the equality hypothesis can be rejected at the 5% level – the corresponding *p*-values are lower than .001 and equal to .01.

education affects civic duty. To answer this question, I test the relationship between civic education and civic duty 23 times, with all civic education indices and control variables in the model, and then I calculate the average marginal effect 23 times.²²

Consistent with the pooled findings, these analyses indicate that civics courses are more strongly associated with civic education than are active learning strategies and open classroom environment. As shown in Figure 2.1, in 61 per cent of the countries in the sample (14 out of 23), the effect of the civics courses index on duty is positive and statistically significant. By contrast, the effect of the active learning strategies and the open classroom environment indices is significant in only 9 and 39 per cent of the countries (2 and 9 out of 23). In summary, a relationship between civics courses and civic duty is more frequent cross-nationally than the other civic education mechanisms, suggesting that results with the pooled ICCS data are robust.

²² This analysis looks at all countries in the 2016 ICSS, including Russia. Although Russia's inclusion could be criticized because elections in that country are not free and fair (Freedom House 2020), I find with the ICCS data that having free and fair elections does not affect citizens' sense of duty to vote or the correlation between civic duty and turnout (see Appendices B.6 and B.7).

Figure 2.1. Average marginal effect of different forms of civic education on sense of civic duty in 23 countries



Notes: entries report the change in the predicted probability of considering voting a civic duty. For the sake of space, the corresponding log-odds are not reported.

6. Supplementary tests

The results so far suggest a role of different forms of civic education in the development of sense of duty, but civics courses are by far the most influential civic duty determinant. In this section I refine these tests in a number of ways. First, given my reliance on adolescents' reports regarding their experience with civic education, it is possible that my indices are inaccurate particularly if the number of students per school is low. I test this possibility by conducting additional tests with cases in which the number of observations per school is higher than 26 (the median). As shown in Appendix B.8, this leads to a slightly weaker effect of the learning and the participation indices but a slightly stronger effect of the openness index on civic duty. Still civics courses remain the most influential civic duty determinant.

Second, the previous tests are based on adolescents who are still in school. As suggested in Greene (2000), it is possible that over the long run, the force of civics courses, in particular, decays. Two Canadian studies – the 2011 and the 2015 Canadian National Youth Surveys (NYS) – allow testing the effect of civics courses on civic duty with individuals aged 18 years old or older who had already left school (see Appendix B.9 for the total number of respondents and their distribution across the Canadian provinces/territories, and Appendix B.10 for an overview of the variables' distribution in these supplementary data).

Table 2.3 reports the results of these additional tests (which replicate the ones with the pooled 2016 ICCS data but include some adjustments to fit the specificities of the NYS data like the use of year fixed effects). They indicate a positive and significant relationship between civics courses and civic duty. That is, Canadians who followed a course on politics in adolescence are more prone to consider voting a duty in adulthood than those with a different school experience. Computing average marginal effects, as well, indicates that exposure to civics courses is associated with a 4 percentage point increase in the likelihood of considering voting a duty, which is in fact somewhat higher than in the pooled estimations. In short, civics courses have a strong effect on civic duty also among individuals who had left school.²³

²³ Unfortunately the NYS does not allow testing the effect of active learning strategies and open classroom environment among adults. There is no reason to assume, however, that results would be different with these other forms of civic education.

Table 2.3. Effect of civic courses on sense of civic duty to vote

	DV: Civic Duty
Experience with Civics Courses	0.273*** (0.040)
Discussion: Parents	0.523*** (0.078)
Female	0.235** (0.075)
Age	0.016+ (0.008)
Education	0.529*** (0.081)
Aboriginal Identity	-0.481** (0.153)
Constant	-0.001 (0.158)
Fixed-Effects: Province	Yes
Fixed-Effects: Year	Yes
N	3,963

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of logistic regressions. Civic duty is dichotomous: 0=disagree or strongly disagree that voting is a civic duty/voting is a choice; 1= agree or strongly agree that voting is a civic duty/voting is a civic duty. Experience with civics courses is also dichotomous: 0=no; 1=yes. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Third, in line with the literature (Neundorf, Niemi and Smets 2016), the observed results might be sensitive to ceiling effects; that is, the effect of the civic education mechanisms might be weaker among students with a previous high sense of duty to vote. Interacting parents' political interest and social status with the three civic education indices indicates, however, no evidence of heterogeneous effects (see Appendix B.11). The observed relationships between different forms of civic education and civic duty is thus not affected by previous civic duty attitudes.

7. Discussion and conclusion

While previous work offers indications of a role of civic education in the development of civic duty (Galais 2018), it remains unclear whether different forms of civic education play a role and are all equally important to developing a sense of duty. In this article, I address that

limitation and find that three key forms of civic education (civics courses, active learning strategies and open classroom environment) have an effect on duty but also that civics courses constitute a far more influential civic duty predictor than do the other two forms.²⁴

Some caution is warranted when interpreting these findings. To begin with, while a study that interviews adolescents from 23 countries on their experience with civic education and their sense of duty is unique, the 2016 ICCS covers mostly developed countries that are also in either Europe or Latin America (the exceptions are Hong Kong, South Korea and Russia, to some extent). Still a focus on these specific countries should not be a problem. First, no discernible trend across regions is observed in the country-specific results, so it seems unlikely that cultural differences play a role in the relationship between civic education and civic duty. Furthermore, a focus on developed countries might mean that the estimated effects of civic education on civic duty are, in fact, conservative; one of the few studies to examine civic education in the context of a developing country reports an effect of civic education on political knowledge in South Africa that is twice as large as that in the United States (Finkel and Ernst 2005). The authors of the study, Finkel and Hearst, attribute this difference to the fact that there are fewer means of acquiring political information in South Africa than in the United States. In their words, civic education has the least potential to increase political knowledge “in advanced settings where civics instruction may be redundant to other sources of political information” (Finkel and Ernst 2005: 358). In short, while the fact that the 2016 ICCS focuses on developed countries that are also in either Europe or Latin America is unlikely a significant problem, it might actually mean that the estimated effects of civic education on duty are conservative.

²⁴ Schools in the 2016 ICCS data do not implement all three forms of civic education to the same degree: correlations are moderate between civics courses and active learning strategies, as well as between civics courses and open classroom environment. Leveraging the diversity in the implementation of civic education mechanisms across schools, I find that active learning strategies and open classroom environment do not add much to how individuals feel about voting when those individuals are already exposed to civics courses (see Appendix B.12).

Moreover, the use of reported measures of civic education is a cause of concern as individuals might falsely report exposure to civic education depending on their sense of duty. In line with previous research (Page and Shapiro 2010; Larcinese, Snyder and Testa 2013), I aggregate these measures at the school level in order to prevent that from happening. The aggregation of students' measures by schools seems particularly successful, as the number of students who report voting for class representative or school parliament is consistent with directors' perceptions of the number of voters in schools (see Appendix B.13). In short, it seems unlikely that my civic education indices are biased given my reliance on students' reports.

Having discussed potential issues with this study, it makes many important contributions to the literature. First, this study suggests that schools can develop duty not only by means of democratic governance, but also by teaching individuals about politics and community affairs, and by encouraging the development and the expression of personal views in an open context. Second, this study suggests that Galais (2018) possibly underestimated the role of civic education in instilling civic duty by focusing on democratic governance, as civics courses (defined as traditional, classroom-based instruction about politics and community affairs) play a bigger role than nontraditional forms of civic education. Third, this study suggests that the general trend toward active learning strategies to teach youngsters to become good citizens might be worrisome if it means a reduction in teaching civic education through civics courses, which has been shown to be the most effective in developing civic duty.²⁵

²⁵ While this study focused on the relationship between three common forms of civic education (civics courses, active learning strategies, open classroom environment) and duty, active learning strategies may be more influential than civics courses when it comes to developing a sense of efficacy – an attitude that plays an important role in predicting participation beyond elections.

To conclude, it stands out from the (additional) country-specific analyses that countries with poorer performance in competences that are also developed at school (e.g. Mexico) perform better than countries with higher performance (e.g. Finland) (Schleicher 2018). More detailed data are currently unavailable for conducting case studies that could help to explain these differences; doing so is also beyond the scope of this article. As such, future work should examine these differences, when the necessary data become available. From a review of the literature, it seems to be worth of investigating, in particular, if the presence of other socialization sources like the media affects the potential of civic education for developing duty.

Chapter 3

Does compulsory voting foster civic duty to vote?

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1. Introduction

The mobilizing effect of compulsory voting is one of the most consistent findings in the field of elections and voting behavior. Both individual and aggregate level research shows that turnout is higher in countries and regions that mandate voting (Cancela and Geys 2016; Stockemer 2017). In countries that enforce compulsory voting rules in particular, a substantial part of the electorate turns out to vote (Birch 2009; Singh 2011).

However the ways in which compulsory voting influences turnout are still debated. Some argue that compulsory voting has an indirect effect on turnout, by increasing the costs of abstaining (Panagopoulos 2008). Others claim that compulsory voting augments the belief in the duty to vote and, consequently, the number of citizens who vote in elections (Engelen 2007). Research has tested the latter argument and found no evidence that civic duty is higher when voting is mandated than when it is not (Maldonado 2015; Rangel 2017). We argue that those null findings are in fact due to the methodology employed by researchers (which we discuss in more detail below). Studying the relationship between compulsory voting rules and civic duty by leveraging Chile's transition to voluntary voting, we believe we are better capable of assessing whether those rules exert an effect on civic duty or not.

We explore that quasi-experimental setting by means of a synthetic control model. We rely on this method because it allows estimating a counterfactual that resembles pre-treated duty levels in Chile. With that synthetic control, we compare the average levels of duty in post-2012 Chile and its synthetic counterpart in order to find out if and the degree to which compulsory voting rules affect sense of duty. We find that Chile's transition to voluntary voting led to a substantial

decline (i.e. a 10 p.p. decline) in citizens' belief of the duty to vote, which might explain the strong decline in turnout following the 2012 reform given the attitude's strong influence in the voting decision (Blais 2000; Clarke et al. 2004). Our findings are confirmed by placebo tests in which we replicate the analyses with all control units, and by many other robustness checks.

In shedding light on one of the mechanisms through which compulsory voting likely affects turnout (duty), we make two important theoretical contributions. First, our study connects aggregate-level work on the determinants of electoral turnout to individual-level theories of what explains turnout: while scholars often refer to the effects of the sanctions that are associated with non-voting to explain the positive effect of compulsory voting on citizens' electoral participation, research on the individual-level determinants of voter turnout has shown that the cost of voting contributes fairly little to explaining why citizens vote (Blais 2000). By contrast, civic duty constitutes a strong individual-level predictor of citizens' engagement in elections, thus representing a more plausible explanation for why individuals are more likely to vote when voting is mandated than when it is left for citizens to decide.

Second, while duty is likely to be strongly shaped by the context in which individuals live, the extent to which contextual factors contribute to the development of duty is still very much unknown. With this research, we show that institutions can affect citizens' adherence to the voting norm in an important way, a finding that suggests the importance of looking at the bigger picture when investigating what drives individuals believe in the duty to vote.

2. Compulsory voting and civic duty

Compulsory voting entails a legal obligation to visit the polling station on Election day or to participate in a given election by other means, such as by proxy voting, advance voting or voting by mail (Birch 2009). Past work has documented several benefits from the legal compulsion to vote. Countries that adopt compulsory voting rules and that enforce them generally have a higher number of citizens who vote in all levels of elections than countries that adopt compulsory voting rules but that do not enforce them and countries in which voting is left for individuals to decide (Birch 2009; Norris 2004).²⁶ Some researchers further argue that, by augmenting turnout, compulsory voting serves as an important mechanism for reducing socio-demographic inequalities in voting (Dassonneville and Hooghe 2017; Lijphart 1997) and for adopting redistributive policies (Bechtel, Hangartner and Schmid 2016; Fowler 2013; Carey and Horiuchi 2017). Yet not all scholarly work on compulsory voting highlights its benefits. Many scholars warn that compulsory voting has negative side-effects as well, specifically when it comes to the “quality” of the vote (Dassonneville et al. 2019; Selb and Lachat 2009), the individuals’ satisfaction with democracy (Singh 2018), and the amount of invalid and blank balloting that is casted in elections (Barnes and Rangel 2018; Singh 2019a).

Given that the mobilizing effect of compulsory voting represents an established finding, recent scholarship has shifted the focus to studying its indirect effects; that is, the means through which compulsory voting affects turnout. For instance, Panagopoulos (2008) expands Downs’ classical rational calculus of voting to incorporate two cost terms: one, regarding the act of

²⁶ A recent study by Barnes and Rangel (2018) contradicts this argument, by showing that the abolition of compulsory voting rules, combined with the transition to automatic registration, led to a lower electoral participation in some Chilean districts, but not in others. For this reason, and to account for a potential overrepresentation of urban respondents in our sample, we conduct additional tests with individuals living in rural areas only. We return to this point in the section with all robustness checks.

voting (C_v); the other, the act of abstaining in a given election (C_{n-v}). Panagopoulos argues that compulsory voting rules increase the value of C_{n-v} (while C_v remains constant) and, as such, they increase the number of voters in elections.

Still there are good reasons to think other mechanisms are at play as well. Singh and Thornton (2013) and De Leon and Rizzi (2016), for instance, demonstrate that partisanship increases when voting is mandatory. They explain that when citizens have the legal obligation to vote, they are likely to resort to shortcuts like partisanship for deciding which candidate to support in an election, and, thus, to reduce the cost associated with the voting decision. Other studies offer evidence that compulsory voting rules spur the acquisition of political information either voluntarily or accidentally (Gordon and Segura 1997; Sheppard 2015; Shineman 2018), which may lead to a preference for a candidate and a reduction of the cost of voting.²⁷

We believe that another factor might explain the link between compulsory voting rules and turnout too: civic duty. To begin with, research suggests that compulsory voting can function as a signal of what actions are expected from a good citizen, thus serving as a duty source (Quintelier, Hooghe and Marien 2011; Chapman 2019). In addition, research reveals that political trust (Lundell 2012), partisanship (De Leon and Rizzi 2016; Singh and Thornton 2013), and political knowledge (Gordon and Segura 1997; Sheppard 2015; Shineman 2018) are higher in contexts of mandatory voting than where voting is a personal decision. Given that these variables are associated with duty to vote (Bowler and Donovan 2013; Galais and Blais 2017; Prior 2003; Riker and Ordeshook 1968), compulsory voting likely affects that attitude.

²⁷ In fact past work reports that political knowledge makes it more likely that differences between candidates are perceived, and fewer individuals are indifferent with respect to the voting choices (Palfrey and Poole 1987).

To the best of our knowledge, two studies investigate the connection between compulsory voting and civic duty. First, Maldonado (2015) builds on work in social-psychology and education and argues that (external) punishment in compulsory voting settings might crowd out internal motivations to vote, including civic duty.²⁸ Maldonado tests this argument empirically by means of a lab experiment in which 760 Peruvians are exposed (or not) to three different messages that either remind participants of the monetary, the non-monetary, or both monetary and non-monetary punishments for abstention. Maldonado finds that reminding participants of the monetary and social consequences of abstention does not affect their sense of duty, which leads Maldonado to conclude that compulsory voting does not influence duty.

Rangel (2017) further tests the association between compulsory voting rules and civic duty. Yet, contrary to Maldonado, Rangel expects a positive relationship between those variables as compulsory voting rules likely signal that the government values electoral participation, and, in doing so, they increase the number of citizens who believe in the duty to vote. Furthermore, children are likely more socialized into voting – and, with it, into civic duty – in countries where voting is mandatory than where voting is a personal decision. Rangel tests those predictions by means of a cross-national analysis of data from the 2010 Latinobarometer. More specifically, she regresses civic duty on compulsory voting and finds that civic duty is not significantly different, in statistical terms, in countries where voting is mandated compared to countries in which voting is a personal decision. Consequently, Rangel concludes (like Maldonado) that compulsory voting does not influence civic duty: “if compulsory voting has

²⁸ Some scholars disagree with the crowding-out argument and claim that compulsory voting in fact might serve to reinforce pre-existing internal motivations to vote. For example, Chapman (2019) and Elliot (2017) argue that the fact that Australians had been obliged to vote since 1924 and have one of the highest rates of sense of civic duty can be taken as evidence that external and internal motivations to vote are not enemies, but in fact reinforce each other.

any effect beyond increased turnout, it is likely not because it makes citizens feel more civically minded and consider voting to be a duty” (Rangel 2017: 44).

While both Maldonado’s and Rangel’s findings represent useful contributions to the literature, we believe the debate is not yet settled, as those studies suffer from important limitations. By exposing individuals to one of three messages (or none at all) about the penalties for abstaining in Peru, Maldonado likely assessed the impact of being reminded about those sanctions on civic duty, not the impact of compulsory voting *per se*. Furthermore, given that participants came from a country in which citizens are obliged by law to vote, those in the control group likely do not differ from those in the treatment groups when it comes to their sense of duty.

Rangel’s study, as well, is limited in an important respect. Her study relies on a cross-national investigation of the relationship between compulsory voting rules and duty. As such, her study might have misestimated the effect of those rules on duty given the difficulty in accounting for all confounders of the relationship between those factors in such a set-up (Hirczy 1994).

In conclusion past research claims that compulsory voting rules foster electoral turnout through the cost of abstaining in elections. Researchers have enlarged this narrative and included the expected benefit of voting and the civic duty to vote as mechanisms through which compulsory voting rules affect turnout. Focusing on the latter mechanism, civic duty, two studies have investigated it by means of a lab experiment and a cross-national analysis. The conclusion was that compulsory voting rules bear no effect on whether citizens conceive voting in dutiful terms. Because those studies can be criticized on methodological grounds and given that there are good reasons for believing in a positive effect of compulsory voting rules on civic duty, we propose a new study that relies on a quasi-experiment, which we detail below.

3. Quasi-experiment

We assess the impact of compulsory voting rules on citizens' sense of civic duty by means of a quasi-experiment. Specifically, we take advantage of Chile's transition from a compulsory voting system (that was not enforced) to a voluntary voting system, in 2012, to study the impact of compulsory voting rules on citizens' belief that voting is a duty.²⁹

We believe that Chile's 2012 electoral reform provides a good quasi-experiment for two reasons. First, a public rejection of duty to vote is unlikely to represent the reason why lawmakers decided to implement voluntary voting in Chile. If the change in voting rules was endogenous to a decline in civic duty, we should see a lower sense of civic duty preceding Chile's transition to voluntary voting. The data do not show evidence of that. Furthermore, before the reform, civic duty was higher in Chile than it was in other compulsory voting systems: for instance, while, on average, 67% of Chileans believed in the duty to vote before the intervention, 52% of Brazilians believed so. As such, it seems unlikely that Chile abolished mandatory voting because civic duty was low in the country.

Second, while the abolition of compulsory voting was combined with the introduction of automatic registration, the change in the registration rules should bias our estimates downwards. In fact Braconnier, Dormagen and Pons (2017) have provided compelling experimental evidence that, when registration is made easier (as in Chile), individuals are more

²⁹ In fact, before 2012, voting was mandatory for citizens who were registered to vote. However, registration was not automatic or compulsory. The few registration offices and their opening only on certain days yielded very low registration rates – especially among youth (Corvalan and Cox 2013: 53). Furthermore, only few registered citizens were penalized by means of a fee or even imprisonment (Barnes and Rangel 2014). With the promulgation of law 20.568, voting became voluntary (Biblioteca del Congreso Nacional de Chile 2018).

likely to be interested, pay attention to, and participate in political affairs. Because civic duty is positively associated with those variables, a rise in civic duty should follow from Chile's transition to automatic registration. Still we conduct additional tests incorporating the change in registration rules and report the results in the section with robustness checks.

In this study we explore that quasi-experiment with data from six Latinobarometers – from 2007, 2008, 2009, 2010, 2011 and 2015.³⁰ We do so for three reasons. First, these studies have measured Latinos' belief in the duty to vote before and after the abolition of compulsory voting in Chile. In those studies, civic duty is measured by the question “Which of the following things do you believe that a person must do if she wants to be considered a good citizen? Vote; Pay taxes; Obey all laws; Participate in social organizations; Participate in political organizations; Buy environmentally-responsible products; Help those in need; Serve in the military”. Respondents can choose as many response options as they want. Those who mentioned “vote” are coded as 1 and those who did not mention it are coded as 0. In the pre-treatment period (from 2007 to 2011), 68% of Chileans report a belief in the duty to vote, only 2 p.p. less than the full sample of Latin-Americans (70%). Given that in the synthetic control estimations we compare Chile and its synthetic control, we aggregate duty responses by country and year.³¹

³⁰ While we lack data on civic duty beyond those six years, this does not impede the use of the synthetic control method. Previous work has in fact implemented this method over as few as two or three time points (Dowling et al. 2012; Fowler 2013; Heersink and Peterson 2017). Yet because of a lack of data in the years immediately following the abolition of compulsory voting (i.e. 2012, 2013 and 2014), we cannot tell whether the effect of the abolition of compulsory voting rules on citizens' belief in the duty to vote strengthens, weakens or remains stable after the change in law.

³¹ We correlate “vote” with “obey all laws” in all compulsory and voluntary voting systems to find out if individuals simply identify that voting is a legal duty in our civic duty measures or not. The results indicate that the correlation between “vote” and “obey all laws” is marginally stronger under compulsory voting: the corresponding gamma statistics are .83 under compulsory voting and .81 under voluntary voting. Tests with likely registered and non-registered Chileans (i.e. those aged 50 or more, and 50 or less) suggest the same results: the corresponding gamma statistics are .86 and .81. It thus seems very likely that our measure captures citizens' sense of civic duty to vote.

Second, the Latinobarometer has been used extensively in comparative research (Farrer and Zingher 2019; Houle, Park and Kenny 2019; Singh 2019a) given its many qualities. Most importantly, the Latinobarometer includes, for each survey-wave, nationally representative samples of country nationals that are interviewed face-to-face in each of the participating countries. For instance, the Chilean sample includes around 2,000 citizens who are sampled randomly in a three-step approach: specifically, in regions, a variable number of blocks, and routes. With this strategy, 18% of Chilean respondents (in the 2015 study) come from rural areas (cities with less than 5,000 inhabitants) and 82 percent from urban areas, a distribution that is consistent with official census data (Instituto Nacional de Estadísticas 2015).

Third, by focusing on Latin-America, the Latinobarometer allows more refined analysis of the impact of Chile's abolition of compulsory voting on civic duty, as the participating countries share political and social characteristics, including the legal obligation to vote.

With Latinobarometer data, we examine the quasi-experiment represented by the abolition of compulsory voting rules in Chile by means of a synthetic control model.³² This model resembles difference-in-differences (a common strategy in quasi-experimental studies such as ours (Bullock and Clinton 2011; Ferwerda 2014; Lucardi 2019)) in that the impact of a treatment is obtained by subtracting the outcome value in the treated unit by its value in the control unit. Formally, we can write this estimation approach as:

³² The synthetic control method is not new within the comparative politics literature. Researchers have used it to study the impact of state policy on tobacco consumption (Abadie, Diamond and Hainmueller 2010), of federalism on social spending (Arnold and Stadelmann-Steffen 2017), and of a natural disaster (the 2007 Ica earthquake) on support for the political system (Katz and Levin 2016). Given the many qualities of this approach, students of compulsory voting have started to rely on the synthetic control method too. They have done so, for instance, to investigate the impact of compulsory voting rules on turnout (Bechtel, Hangartner and Schmid 2018), on parties' vote share (Fowler 2013), and on their vote-seeking strategies (Singh 2019b). In this research we estimate the synthetic control by means of the "synth" package that is available in R (Abadie, Diamond and Hainmueller 2011).

$$\alpha_i = Y_i^1 - Y_i^0$$

where Y entails the outcome value with treatment (marked by the superscript 1) and without treatment (marked by the superscript 0). The subscript i denotes the observation. In our case, the effect of compulsory voting rules results from the subtraction of the duty levels between treated Chile and non-treated Chile. Yet, in contrast to difference-in-differences, the synthetic control method allows estimating a non-treated (“synthetic”) Chile with similar pre-2012 duty trends as “real” Chile (Abadie, Diamond and Hainmueller 2010, 2015).

In our case, estimating a “synthetic” Chile is crucial because an important assumption of the difference-in-difference method (the parallel trend assumption) is likely to be violated (Angrist and Pischke 2009): in fact visually inspecting pre-2012 trends in civic duty in Chile and other compulsory voting countries in Latin America suggests different trends in civic duty between those countries (see Appendix C.1 for information on the Latin-American countries’ voting rules, Appendix C.2 for civic duty trends in Chile and in all CV countries, and Appendix C.3 for the levels of civic duty in each Latin-American country). Moreover, when regressing (pre-2012) civic duty among compulsory voting systems on survey year, a country dummy (Chile=1; All other CV countries=0) and their interactions, we find a statistically significant difference in civic duty trends between Chile and all other Latin-American compulsory voting systems before 2012 (see Appendix C.4).³³ As such, it seems likely that the parallel trend assumption is violated, reason for which we rely mainly on the synthetic control method.

³³ More specifically, three of the four tests reported in Appendix C.4 fail to confirm the parallel trend assumption.

To estimate “synthetic” Chile, we include information on pre-2012 duty levels and variables that are associated with individuals’ sense of duty to vote (Abadie, Diamond and Hainmueller 2015).³⁴ Those variables are represented by 1) compulsory voting, 2) trust in parliament, 3) support for democracy, 4) democratization level, 5) education, 6) age and 7) religion. We rely on those variables given the many reasons for a connection between compulsory voting and duty. In addition, trust in parliament, support for democracy, and democratization level are shown to be correlated with duty (Galais and Blais 2017). And the more educated, the elderly and the religious tend to consider voting as a duty more than their counterparts (Blais 2000; Galais and Blais 2017). Information on countries’s voting system comes from the 2018 Varieties of Democracy Project. Data on trust in parliament and support for democracy come from the Latinobarometer, and on democratization level, from Freedom House. At last, data on education come from the Varieties of Democracy Project, on age, from the World Bank, and on religion, from the Latinobarometer (see Appendix C.5 for a distribution of those variables).³⁵ Given that our synthetic control estimation takes place at the national level,

³⁴ Given the risk of overfitting, in additional tests we replace pre-2012 duty levels by the mean levels of civic duty in 2007 and in 2011, only, as well as the change in duty between these years (Beckley, Horiuchi, and Miller 2018). As the results reported in the robustness checks section indicate, our findings remain the same when we do not use pre-2012 duty levels in our analysis. Also note that in the synthetic control model, we estimate a counterfactual that resembles the treated unit at each individual time point (Abadie, Diamond and Hainmueller 2015). Consequently, the variance of predictors used for estimating the synthetic control and their strength are not a concern. “All that matters is that they help model the pre-treatment outcome as accurately as possible” (Koehler and König 2015).

³⁵ Countries are divided in either compulsory or voluntary voting system, and they are classified as “not free”, “partly free”, or “free”. Trust in parliament is measured by the question “How much trust you have in each of the following groups, institutions or persons: a lot, some, a little or no trust? National Congress/Parliament.” Support for democracy, by the question “With which of the following statements do you agree most? Democracy is preferable to any other kind of government; Under some circumstances, an authoritarian government can be preferable to a democratic one; For people like me, it doesn’t matter whether we have a democratic or non-democratic regime”. Education is measured by average years of formal education, age is the percentage of 19-34 year-old among the voting age population, and religion is measured by the question “How would you describe yourself? Very devout, Devout, Not very devout or Not devout at all”.

variables originally measured at the individual level (religion, trust in parliament and support for democracy) are aggregated by country and year.

With those variables, we calculate and assign weights (summing up 1) to all countries in our data (including compulsory and voluntary voting systems) in order to estimate “synthetic” Chile. Those weights are given according to the countries’ values on pre-2012 duty levels, their values on the variables that are associated with duty (compulsory voting, trust in parliament, support for democracy, democratization level, education, age and religion), and their linear combinations (Abadie, Diamond and Hainmueller 2015). Countries that mostly resemble the treated unit with respect to duty, its predictors, and the association between those variables are assigned a higher weight than countries that do not, meaning they contribute to a larger degree to the estimation of “synthetic” Chile.³⁶ As such, the formula for estimating the impact of compulsory voting on sense of duty with the synthetic control method reads:

$$\alpha_{Chile} = Y_{Chile}^1 - w_i * Y_i^0,$$

where α corresponds to the treatment effect of the abolition of compulsory voting on Chileans’ sense of duty, Y , the outcome (duty) value with treatment (marked by the superscript 1) and without treatment (marked by the superscript 0), and W , the country weights. The subscript i denotes the set of countries that are used in the synthetic control estimation (including countries that donate to the estimation of the synthetic control and those that do not).

³⁶ Note that a perfect match is uncommon, and many units in the donor pool are assigned a zero weight, meaning they do not contribute to the estimation of the synthetic control. Still the synthetic control generally represents a good approximation of the treated unit, constituting a valid counterfactual (Abadie, Diamond and Hainmueller 2011).

4. Results

We first estimate a “synthetic” Chile, which we then use to assess the impact of the abolition of compulsory voting on citizens’ sense of duty. We do so based on countries’ pre-treatment values on pre-2012 duty levels and seven other variables: compulsory voting, support for democracy, trust in parliament, and democratization level, education, age, and religion. Table 3.1 displays how close “synthetic” Chile is to “real” Chile before the intervention. We can observe a high correspondence between Chile and “synthetic” Chile (that was created from Peru (.69), Uruguay (.29) and Panama (.02)) on several variables. For instance, both constitute compulsory voting systems, and have a relatively high level of education and democratization. They also have an older population in comparison with other Latin-American countries. Most importantly, “synthetic” Chile has approximately the same mean level of civic duty than “real” Chile before the treatment. Given that we are mainly interested in obtaining a control with similar (parallel) duty trends, this may be taken as strong evidence that the estimated “synthetic” Chile is a valid counterfactual to the “real” Chile.

Table 3.1. Mean level of variables before the abolition of compulsory voting (in 2012)

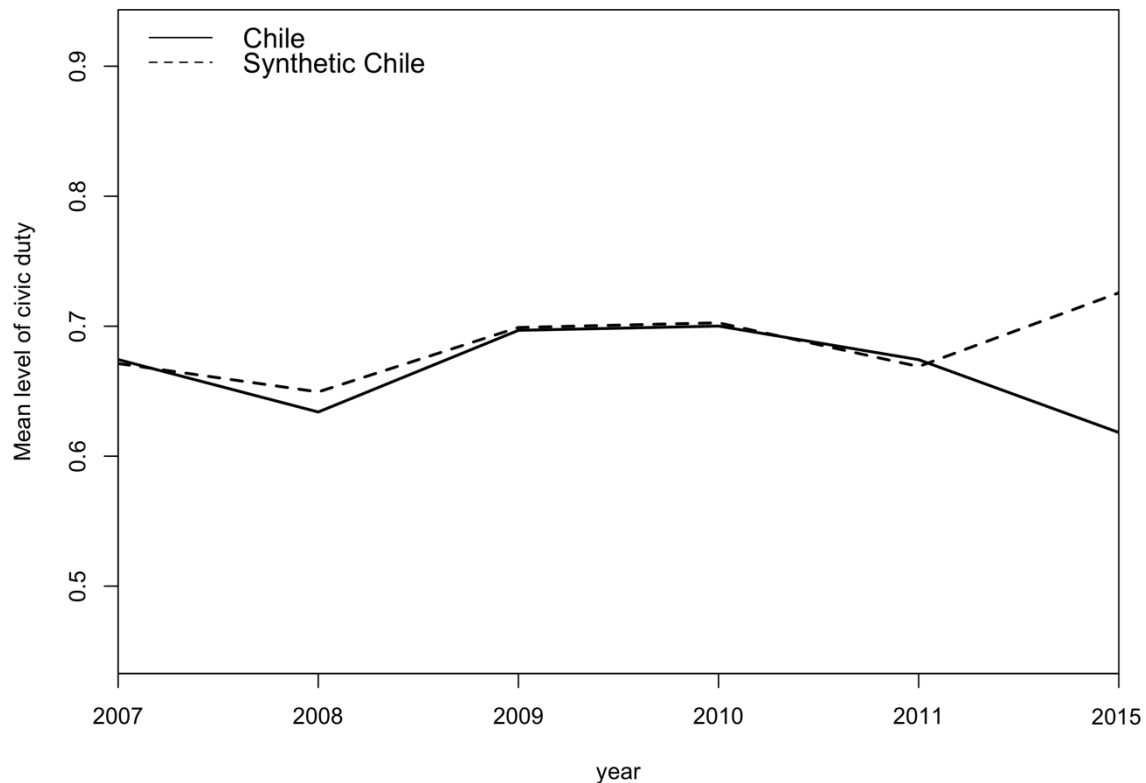
	“Real” Chile	“Synthetic” Chile	All countries in the donor pool
Compulsory voting (2007-2011)	1.00	1.00	0.75
Civic duty (2007-2011)	0.68	0.69	0.71
Education (2007-2011)	10.20	8.57	7.24
Religion (2007-2011)	2.29	2.30	2.52
Democratization level (2007-2011)	3.00	3.00	2.54
Age (2007-2011)	0.34	0.39	0.42
Trust in parliament (2007-2011)	2.16	1.97	2.09
Support for democracy (2007-2011)	2.35	2.47	2.41

Notes: The countries that contributed to the estimation of “synthetic” Chile are Peru, Uruguay and Panama (for the weights given to each Latin-American country, see Appendix C.6). While some differences are observed, “real” Chile and “synthetic” Chile have approximately the same civic duty mean level before the treatment. Because we focus on this variable in our subsequent analyses, this may be taken as strong evidence that the “synthetic” Chile can be used as a valid counterfactual.

Considering that the “synthetic” Chile seems to be a very good approximation of “real” Chile before the transition to voluntary voting, we measure the impact of compulsory voting on civic duty by comparing the civic duty mean level in “real” Chile and in “synthetic” Chile after 2012. As Figure 3.1 shows, “real” Chile and “synthetic” Chile have approximately the same mean levels of duty in all pre-treatment years. Yet they differ substantially after the abolishment of compulsory voting (in 2012). Civic duty declines to about 62 percent in “real” Chile, whereas it increases to about 72 percent in “synthetic” Chile. In short, while Chile and its synthetic control had very similar levels of duty before the treatment, a large difference (of 10 p.p.) is observed after the abolition of compulsory voting, which may be taken as an indication that this institutional reform negatively affected civic duty. According to the estimates of our synthetic control method, had compulsory voting not been abolished in Chile, civic duty would have been about 10 percentage points higher in 2015 than what it was in reality.

This effect seems rather large, but it is reassuring to note that an effect of this size is in line with the effect sizes of compulsory voting regarding other outcome variables. For example, previous studies report a 28 p.p. increase in partisanship, and a 20 p.p. increase in support for leftist policy positions (in a referendum) due to compulsory voting alone (Bechtel, Hangartner and Schmid 2016; Singh and Thornton 2013). We should note, as well, that a substantial effect on civic duty corresponds with the sizeable effects that the change of law had on citizens’ electoral participation: more specifically, turnout in Chile declined from 59 percent before 2012 to 45 and 49 percent in the 2013 and 2017 presidential elections, even though Chile’s mandatory voting rules were weakly enforced (IDEA 2018; Singh 2011).

Figure 3.1. Civic duty levels in “real” Chile and “synthetic” Chile before and after the abolition of compulsory voting



Notes: “Real” Chile and “synthetic” Chile have the same civic duty mean levels before the abolition of compulsory voting, in 2012. A difference is observed after that year, however, when Chile shifted to voluntary voting. We characterize this difference as the effect of compulsory voting on civic duty.

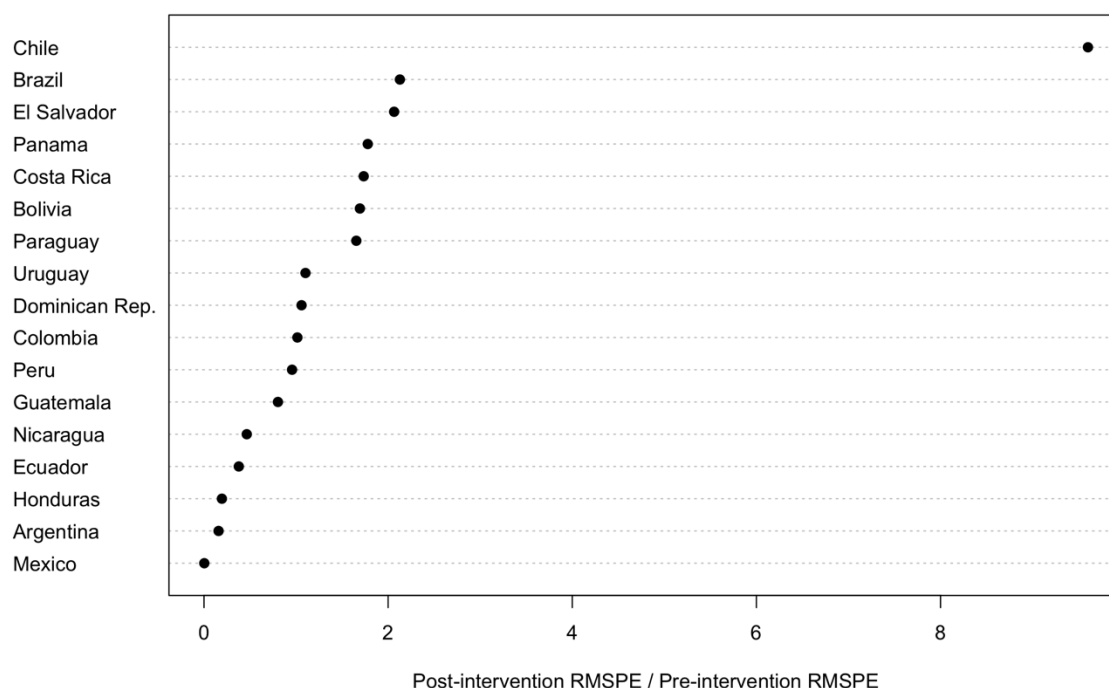
5. Supplementary tests

Given the specificity of the Chilean case, and particularities related to the synthetic control model, we perform eight robustness checks. First, we assess the likelihood of finding a similar effect in other Latin-American countries, where no change in the electoral rules related to the legal obligation to vote were observed. To this end, we follow previous studies that relied on synthetic control models and conduct placebo tests (Abadie, Diamond and Hainmueller 2015), in which we estimate a synthetic control for each country in the donor pool (including those who did not contribute to the estimation of “synthetic” Chile). If the difference that is observed between “synthetic” Chile and “real” Chile also occurs with other countries and their respective

synthetic control, it means that such a difference may not be due to Chile’s abolition of compulsory voting and, thus, we have a good reason to be skeptical about its influence on civic duty. In contrast, if we only find an effect of this size in Chile, we can argue with greater confidence that the the abolition of compulsory voting rules yielded a decline in duty.

Figure 3.2 displays the results of these placebo tests. It indicates the ratio of the root-mean-squared difference in civic duty between each country in the donor pool and their synthetic control after the intervention divided by the same difference before the intervention.³⁷ Chile is clearly an exceptional case. While the ratio is between 0 and 3 among all other countries, it is about 10 for Chile. Such a large gap suggests that the difference between “real” Chile and its synthetic control after 2012 is specific to this country, and likely the result of its transition to a system in which citizens have the choice to vote or abstain in elections.

³⁷ In mathematical terms, $\frac{\sqrt{\text{mean}(duty_{synthetic,post-2012} - duty_{real,post-2012})^2}}{\sqrt{\text{mean}(duty_{synthetic,pre-2012} - duty_{real,pre-2012})^2}}$.

Figure 3.2. Placebo tests with all Latin-American countries in the dataset

Notes: This figure indicates the ratio of the root-mean-squared difference in civic duty between each country in the donor pool and their synthetic control after the intervention divided by the same difference before the intervention (see Appendices C.7 and C.8 for the differences in civic duty levels between all countries and their synthetic control). We observe that Chile is clearly an exceptional case. We attribute the observed difference to its change to a voluntary voting system, in 2012.

Second, we explore the possibility that our synthetic control estimations over-fit the pre-treatment data due to the use of pre-2012 duty levels and replicate the model for each of the countries in the sample, creating a synthetic version of the country. After doing so, we calculate the difference between each country and its synthetic control in each survey year, then compute the pre-treatment and the post-treatment means of these differences within each country, divide these numbers, and average them with all countries in the dataset, except Chile. We find that the post-treatment gaps are not substantially lower than the pre-treatment gaps: the mean gap across all Latin-American countries is .047 before 2012, and .055 after 2012. When we remove countries whose pre-treatment gaps are twice larger than Chile, the differences remain

minimal: the pre-2012 mean gap is now .019 and the post-2012 mean gap, .030. It thus seems safe to reject the possibility that our analysis over-fitted the pre-treatment data.

Although an over-fit is unlikely, we still perform the synthetic control model without the lagged values of the dependent variable. As suggested by Beckley, Horiuchi and Miller (2017), we replace the pre-2012 duty levels for the civic duty mean levels in 2007 and in 2011 (i.e. in the first and last time point before the intervention), and a measure of its growth rate between these years. Doing so does not change much the results: the duty mean value still increases in “synthetic” Chile after 2012, whereas it declines in the “real” Chile in the same period, with a difference in these values of 10 percentage points (see Appendix C.9).

Third, we conduct the same synthetic control estimation with only three of our six Latinobarometers (from 2007, 2011 and 2015). One concern might be that we estimate the synthetic control model with data containing different time intervals: the difference between the post-treatment survey from 2015 and the pre-treatment survey from 2011 is four years, while all pre-treatment surveys (from 2007, 2008, 2009, 2010, and 2011) have a one-year interval. Using the 2007, 2011, and 2015 LatinoBarometers, we find that Brazil contributes to the estimation of the synthetic control as it is assigned a .09 weight. Peru and Uruguay remain the biggest donors (.57 and .31), followed by Panama (.02). Even though the control units are somewhat different, the observed difference between post-2012 “real” Chile and its synthetic control remains 10 percentage points (see Appendix C.10).

Fourth, we tested the robustness of our results by withdrawing one country from the donor pool each at a time. We do so in order to find out whether any particular country in the analysis is driving the results (Beckley, Horiuchi and Miller 2017). In these additional tests, we started

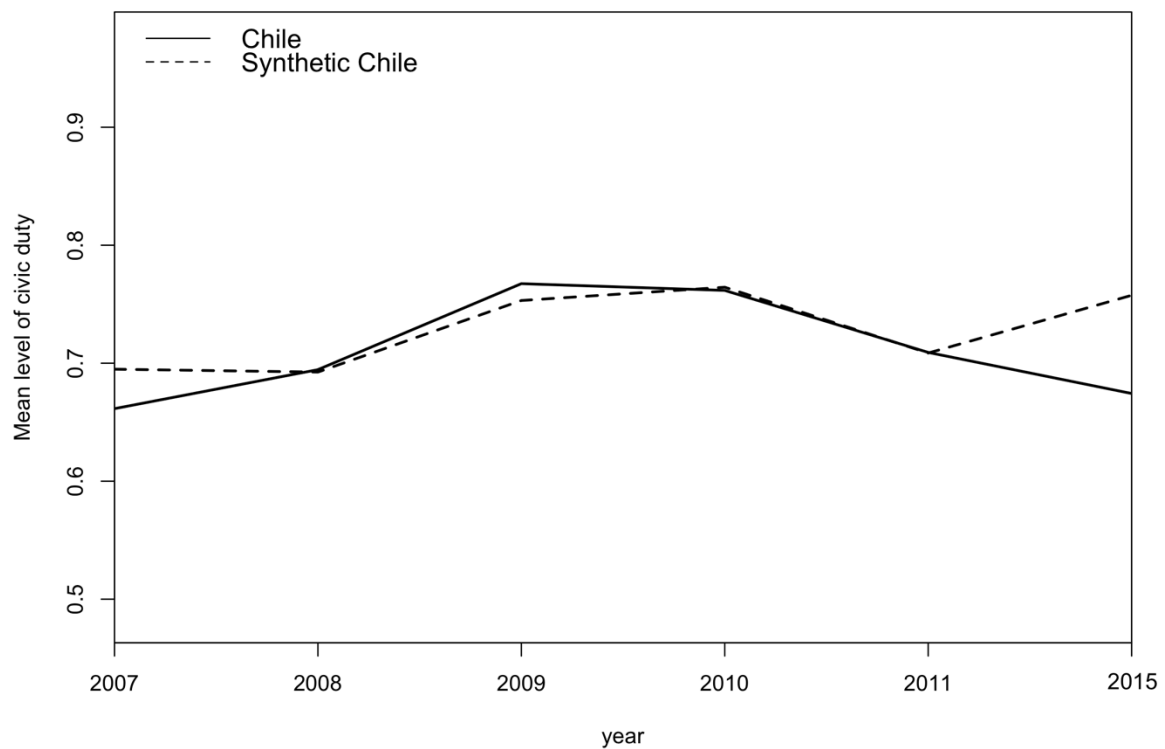
with Peru, then Uruguay, and then Panama, countries that contribute from the most to the least to the estimation of the “synthetic” Chile. While the fit between Chile and its synthetic control declines when we exclude one of these countries, the difference between them after the abolition of compulsory voting rules remains 10 percentage points, with civic duty increasing in “synthetic” Chile and declining in “real” Chile (see Appendices C.11–C.13).

Fifth, we re-estimate “synthetic” Chile varying the sample of individuals in our analysis. A recent study by Barnes and Rangel (2018) has shown that the negative turnout effect of the abolition of compulsory voting rules in Chile is conditional on district size: that effect is stronger in more populated districts than less populated ones. Consequently, based on Barnes and Rangel’s work, we might have overestimated the effect of compulsory voting on civic duty because citizens from urban centers are overrepresented in our data. To test this possibility, we conducted the same tests with only those coming from rural areas, defined as cities with less than 5,000 inhabitants. We do not find evidence of a bias: restricting the aggregate-level tests to those individuals does not change the observed decline in civic duty after 2012 in “real” Chile and its increase in “synthetic” Chile. The difference between Chile and its synthetic counterfactual remains 10 percentage points as well (see Appendix C.14).

Sixth, while the synthetic control model provides consistent evidence of a negative effect of the abolition of compulsory voting rules on duty, it is possible that another political event explain the decay in duty after 2012. We discuss two possibilities: the simultaneous transition to automatic registration, and the student protests from between 2011 and 2013. Starting with automatic registration, as previously indicated, it is unlikely that the simultaneous change in registration rules accounts for the observed decline in civic duty. Research by Braconnier, Dormagen and Pons (2017) shows that making registering easier increases attention to and

interest in politics, and, consequently, turnout. As such, the shift to automatic registration in Chile should have *increased* citizens' sense of duty, and thus their likelihood of turning out to vote, if it had affected our results.³⁸ Still, given that this is a very important point, we have sought other ways to accounting for the simultaneous change to voluntary voting and automatic registration. More specifically, we performed additional tests in which we limit our analysis to citizens aged 50 years old or more. This is the group of individuals that was most likely to have already registered to vote in elections before 2012 (partly as a consequence of the massive registration efforts that followed the fall of Pinochet's authoritarian regime (Barnes and Rangel 2018; Corvalan and Cox 2013)), and who should consequently be not much affected by the transition to automatic registration. When replicating the analysis for a sample of respondents of 50 years and older, we find that civic duty still declines after 2012, while it increases in "synthetic" Chile, with a difference of 8 p.p (see Figure 3.3). As a second test, we added the changing registration rates in the estimation of the synthetic control. Doing so does not alter the main findings: this test still suggests a decline of civic duty in "real" Chile following the 2012 electoral reform, and an increase in "synthetic" Chile in the same period, with a difference of 10 percentage points (see Appendix C.15). In short, accounting for the change to automatic registration in the synthetic control estimations does not change our findings.

³⁸ On the possible link between registration rules and civic duty, it should be further noted that a pre-2012 decline of registration rates in Chile, especially among new generations (Barnes and Rangel 2018), was not accompanied by a decline in the extent to which Chileans consider voting a duty or not. A connection between registration rules and civic duty is hence very unlikely.

Figure 3.3. Synthetic control model with citizens aged 50+ years old

Notes: The post-treatment difference in the civic duty mean levels between “real” Chile and its synthetic control is slightly lower (8 percentage points) when restricting the analysis to respondents aged 50 years old or more, who are likely registered to vote before 2012, and, hence, upon whom the transition to automatic registration likely exerted no influence.

Moving to the student protests (also referred to as the “Chilean winter”), while research indicates an influence from these demonstrations on the candidates’ agenda during the 2013 presidential election (Bernasconi 2014), they are unlikely the cause of the decline of duty. Given that they might have bolstered public interest in politics, the consequence should be an augmentation of citizens’ sense of civic duty to participate in elections (see Carreras 2018 for evidence on the strong positive link between political interest and civic duty). As such, we may conclude that the two most visible political events in Chile that might have affected citizens’ civic duty are unlikely the cause of the observed decline in civic duty after 2012, since both events should have resulted in a rise (not a decline) of civic duty in this period.

Seventh, while the synthetic control model implicitly controls for the factors that do not change or change little between 2007 and 2015, we have also pursued a more traditional estimation approach that leverages between-country, individual-level variation in compulsory voting and in civic duty: specifically, a multi-level analysis.³⁹ As can be seen on Appendix C.16, estimating a multi-level model on the pooled individual-level data from the Latinobarometer offers little indication of a positive relationship between compulsory voting rules and civic duty. However, when estimating effects in the larger, and for that reason more appropriate, International Social Survey Program dataset, the results are more supportive of our theory: levels of civic duty are, on average, significantly higher in countries that implement a system of compulsory voting. Importantly, these results do not change when we add both country and year fixed effects. As shown on Appendix C.17, the impact of compulsory voting remains insignificant with Latinobarometer data, but significant and positive with ISSP data.

Eighth, while there is a risk that the parallel trend assumption is violated, we conduct a difference-in-difference model to examine the relationship between compulsory voting rules and civic duty. Comparing Chile with all Latin-American countries where voting is compulsory immediately before and after treatment, we find that the effect is still about 10 percentage points (see Appendix C.18). To take into account the civic duty trends before the treatment, we also estimate the difference-in-differences model with data from all pre-treatment years (2007, 2008, 2009 and 2010). In this more conservative test, we obtain a much lower effect of the abolition of compulsory voting on civic duty: it is now 2.9 percentage points, or 3.4 points if

³⁹ The data from our Latinobarometers have a hierarchical structure, with individuals nested in years and countries. As such, given that civic duty is at the individual level and compulsory voting at the country level, we estimate multi-level models in which individuals are nested within countries. In turn, the over-time variation within countries is accounted for by means of year fixed effects.

we exclude Honduras, which seems to be an outlier in the dataset.⁴⁰ These effects are substantially lower than those obtained with the synthetic control model, but they are in the same direction: that is, they suggest a decline in civic duty after the abolition of compulsory voting. While this result leads us to be careful when interpreting the size of the effect from the synthetic control estimation, it strengthens our confidence that there is a positive relationship between compulsory voting and civic duty to vote.

6. Discussion and conclusion

In this paper, we tested the role of civic duty in linking compulsory voting and turnout. By means of a synthetic control method, we evaluated the impact of the abolition of compulsory voting rules in Chile on levels of civic duty in the country. We found that civic duty declined in post-2012 Chile, when compulsory voting was replaced by a voluntary voting system. We estimated a series of additional models and conducted placebo tests in order to assess the robustness of our results. In all cases we found evidence that individuals' sense of duty is partly driven by their exposure to compulsory voting rules or not.

While our results seem robust, our study is limited in at least five ways. First, we lack data from a longer timeframe and, as such, we cannot say if the effect of the abolition of compulsory voting rules in Chile will remain the same or not. Second (and relatedly), we do not have data from 2012, 2013, and 2014. The citizenship question that we use as a measure of civic duty was not employed in Latinobarometer studies in those years. As a result, we cannot determine if the effect was higher (or lower) right after the abolition of compulsory voting. Third, our

⁴⁰ While 64 percent of Latin-Americans report a sense of duty to vote before the treatment (i.e. in 2011), only 41 percent of Hondurans do so. Furthermore, from 2007 to 2010, on average 69 percent of Hondurans indicate that voting is a duty, which is a lot higher than the 41 percent found in 2011.

results might be driven by the fact that we rely on a single item to measure civic duty. Past scholarship has supported the use of multiple indicators for capturing a latent concept like civic duty to vote (Blais and Galais 2016). Yet as we are constrained to using Latinobarometer data, there is no other option than to capture civic duty by means of a single item. Fourth, our synthetic control is not perfectly identical to Chile before the intervention. Consequently, some of the observed difference between Chile and its synthetic control may be due to some unaccounted differences between the two. Fifth, while our analyses suggest a decline in civic duty following 2012 (which likely affected negatively turnout), some researchers have drawn attention to the fact that the abolition of compulsory voting yielded a turnout increase in some Chilean districts (Barnes and Rangel 2018). Unfortunately, because of data limitations, we cannot investigate if our effects differ between regions. Yet the main take-away from the observation that turnout increased after the reform in some districts is probably that mandatory voting may not be a panacea for fostering civic duty – and in this way turnout.

Despite those limitations, our study represents a first attempt to explore the association between compulsory voting and civic duty through a quasi-experiment. In showing evidence of a link between those variables, our study connects aggregate-level work on the determinants of turnout to individual-level theories of what explains turnout: specifically, the well-documented association between electoral participation and individuals' sense of civic duty to vote. While previous research on the connection between compulsory voting and turnout referred to the costs from abstaining to explain such association, individual-level research suggests that such rational considerations have little impact on the decision to vote or abstain. In showing that civic duty, a far more important turnout determinant, is also affected by compulsory voting, we offer a more plausible explanation of the positive link between compulsory voting and citizens' turning out to vote than previously given by researchers in the field.

Our study contributes, as well, to the literature on the origins of civic duty. Only a handful of studies have investigated where civic duty comes from, even though it constitutes one of the most important reasons why individuals bother to vote. Those studies also focus on the role of individual-level factors in explaining why some individuals develop a sense of civic duty to vote, and others do not (Galais 2018). Macro-level factors have so far not received much attention in this field. We hence make an important contribution by demonstrating that compulsory voting as well can serve as a source for the development of civic duty.

Finally, our results offer an important contribution to politics in general. They suggest one way in which the decline of civic duty, that is observed in many advanced democracies, may be deterred and civic duty augmented. Research has shown that new generations adhere less to the voting norm and, as such, they have been less keen to vote in elections (Blais, Gidengil and Nevitte 2004; Dalton 2008). We demonstrate that compulsory voting constitutes an important mechanism that might be able to foster a civic-spirited citizenry that vote in elections. We note, however, that despite the evidence of an association between compulsory voting and civic duty, an increase of blank and invalid voting likely follows the legal obligation to vote (Barnes and Rangel 2018; Singh 2019a). In this case, the implementation of mandatory voting is not necessarily good for democracy, given that it might increase the number of blank and invalid votes in elections while pushing more eligible citizens to the polls.

Chapter 4

Does corruption affect civic duty to vote?

This article was published in Electoral Studies. The full reference of the article is Feitosa, F., 2020. Theoretically, yes, but also empirically? How the corruption-turnout link is marginally explained by civic duty to vote. *Electoral Studies* 66: 1-22.

1. Introduction

A large literature provides evidence of lower levels of electoral participation in contexts of high corruption (Birch 2010; Dahlberg and Solevid 2016; McCann and Dominguez 1998; Sundström and Stockemer 2015; Stockemer 2013). Some researchers explain the negative impact of corruption on turnout with reference to individuals' potential for influencing policy decisions (Stockemer, LaMontagne and Scruggs 2013; Warren 2004). An argument based essentially on the rational choice theory, however, seems insufficient for explaining the link between corruption and turnout, as we know that rational factors add fairly little to explaining why citizens vote (Blais 2000; Blais, Young and Lapp 2000; Clarke et al. 2004).

Taking a different perspective, an influential group of scholars argues that corruption affects turnout in a more fundamental way as well: it erodes citizens' sense of civic duty to vote (defined as "the belief that a citizen has a moral obligation to vote" (Blais and Galais 2016: 61)) and, by doing so, affects their electoral participation (Bowler and Donovan 2013; Jones and Hudson 2000; Sundström and Stockemer 2015). A relationship between corruption and civic duty is explained with reference to the incentives for internalizing the voting norm and to political trust. More specifically, those incentives are weak when citizens think that politicians act unethically (Jones and Hudson 2000; Bowler and Donovan 2013). By enhancing political distrust, as well, corruption leads to an erosion of the duty to vote (Sundström and Stockemer 2015). Despite the existence of good reasons for believing in a connection between corruption and civic duty, to the best of my knowledge, no study has ever tested the corruption-duty nexus empirically. As a result, it remains unclear whether this link is real or not and, consequently, whether civic duty mediates the effect of corruption on turnout or not.

In this research I submit both the corruption-duty link and the mediation role of civic duty to an empirical test. To these ends, I use the pooled data from the Making Electoral Democracy Work (MEDW) project. These data are unique as they measure respondents' sense of duty through a duty/choice question, which is considered the best means of assessing duty (Blais and Achen 2019).⁴¹ In addition, the MEDW data measures civic duty with respect to four election levels: national, regional, European and municipal elections, thus allowing for a more detailed exploration of the link between corruption and duty, and the mediation effect of duty.

The empirical findings from the analyses are in line with expectations: corruption is negatively associated with duty, and it is so irrespective of the election level. Yet this relationship is substantively weak. Civic duty mediates, as well, the connection between corruption and citizens' electoral engagement. However, it does so marginally. In offering these pieces of evidence, this study corroborates the argument that corruption affects citizens' electoral participation through their sense of duty to vote. However, this study also suggests a weaker relationship between civic duty and corruption than that suggested by the literature, and what holds for other rational factors (expected benefit and cost of voting).

2. Corruption and sense of civic duty to vote

Corruption entails the “acts in which the power of public office is used for personal gain in a manner that contravenes the rules of the game” (Jain 2001: 73) – see Gerring and Thacker (2004) for a similar definition of this concept. Corruption may take several forms, which makes any classification insufficient (Heywood 1997). Still research has often discussed three forms

⁴¹ To reduce social-desirability bias, this question was also asked before an election. In addition, the order of in which duty and choice appear in this question was randomized: some respondents were first exposed to duty, while other respondents were first exposed to choice.

of corruption that are defined by the agent of corruption: grand, bureaucratic and legislative corruption (Jain 2001). Given that the MEDW data (used in this study) tap citizens' perception of corruption in government, in this research I focus on whether and the extent to which grand corruption affects turnout individuals' sense of duty to vote.

Scholars often associate corruption with a decline in electoral participation.⁴² For example, McCann and Dominguez (1998) reveal a negative association between perceived corruption and Mexicans' willingness to cast a vote in presidential elections. Specifically, the authors provide evidence that "those who believed that voting decided how Mexico was governed were more likely to turn out to vote while those who expected elections to be fraudulent were more likely to stay at home on Election Day" (McCann and Dominguez 1998: 497). In an analysis of 200 presidential elections from 70 countries, Stockemer (2013) observes, as well, a negative trend between corruption and electoral participation: as corruption becomes more widespread in a country, citizens are less likely to vote.

Scholars have investigated the reasons why corruption yields a decline in electoral participation. A recurrent argument made in the literature is that corruption negatively affects turnout because of a rational calculus on the part of citizens. More specifically, when citizens realize that their vote will have little or no influence on political decisions, they become less

⁴² In line with Bowler and Donovan (2013), Jones and Hudson, 2000, and Sundström and Stockemer (2015), I assume a negative relationship between corruption and turnout. This does not mean, however, that a positive or nil effect are impossible. A large literature has provided evidence that turnout may be actually higher in corrupt than non-corrupt settings. Such a mobilization effect may be explained by a process of indignation, in which citizens see elections as a means to overthrowing corrupt politicians (Kostadinova 2009), by an electioneering effort on the part of corrupt governments (Escaleras, Calcagno and Shughart 2012), or even by clientelist networks, which give some groups of individuals a reason to vote (Stokes 2005; Haveric, Ronchi and Cabeza 2019; Bauhr and Charron 2018). In what regards a nil effect of corruption on turnout, this may be explained by citizens' lack of information (Chong et al. 2015). Given my focus on if and the extent to which civic duty plays a mediation role, the positive or nil turnout effects of corruption will not be explored in this paper.

willing to cast a vote. Supporting this explanation of the corruption-turnout link, Warren (2004: 328) affirms that corruption “breaks the link between collective decision-making and people’s power to influence collective decisions through speaking and voting”, which results in citizens’ demobilization to turning out to vote. In the same vein, Stockemer et al. (2013: 76) argue that when private interests dictate governmental decisions, citizens “stop considering elections as ‘instruments of democracy’ that are worth their time and effort”, causing a reduction in the number of voters. The explanation for a negative association between corruption and turnout has thus been recurrently anchored on rational reasons for voting.

Yet it is well established in the turnout literature that this type of reasons adds fairly little to explaining voter turnout (Blais 2000; Blais, Young and Lapp 2000; Smets and van Ham 2013). By contrast, ever since the introduction of the “D” term citizens’ rational calculus of voting (Riker and Ordeshook 1968), civic duty has been perceived as an additional reason for and a more powerful predictor of electoral participation than rational considerations (Smets and van Ham 2013; Blais, Young and Lapp 2000; Clarke et al. 2004). Given the limited influence that rational calculations have on voting, and the strong relationship between civic duty and turnout, a group of influential researchers has defended that corruption affects turnout in a more fundamental way as well: it erodes civic duty, and, by doing so, affects turnout (Bowler and Donovan 2013; Jones and Hudson 2000; Sundström and Stockemer 2015).

These studies offer plausible theoretical reasons for assuming a connection between corruption and civic duty and for thinking of civic duty as a mediator. They do so with reference to the incentives for internalizing the voting norm, or to political trust. Regarding the first explanation, Jones and Hudson (2000) argue that unethical representatives signal a low value of the duty to vote, which results in a weak incentive for citizens to consider, on their part,

voting a duty. Using a similar rhetoric, Bowler and Donovan (2013) affirm that unethical representatives signal little concern for a social contract (connecting citizens and the state), which results in the same kind of (weak) incentive. With respect to the second explanation,, Sundström and Stockemer (2015) argue that by reducing citizens' trust in representatives, corruption decreases their sense of duty to vote in elections.⁴³

While there are thus plausible theoretical reasons for a connection between corruption and civic duty and for a mediation effect of civic duty, to the best of my knowledge, no study has ever gone beyond such theoretical reasoning and has put this link to an empirical test. With the pooled data from the Making Electoral Democracy Work (MEDW) project, I provide the first empirical test of the connection between corruption and duty, and the mediation effect of duty.

3. Data and measures

I use the data from the Making Electoral Democracy Work (MEDW) project to test the connection between corruption and civic duty, and the attitude's mediation effect for three reasons. First, the MEDW captures duty by means of a duty/choice question, which is considered the best operationalization of civic duty (Blais and Achen 2019).

Second, prior work has shown that many individuals do not attribute the same weight to different levels of elections (Galais and Blais 2016b): that is, citizens' evaluation of whether or not they have a duty to vote depends on the level of the election. In addition, individual-

⁴³ In addition to the reasons cited by Bowler and Donovan (2013), Jones and Hudson (2000), and Sundström and Stockemer (2015) for a mediation effect of civic duty, one can also argue that corruption erodes government legitimacy and, in this way, it leads to a lower sense of duty to vote or even a duty to abstain (see Davis, Camp and Coleman 2004).

level factors affect civic duty differently depending on the level of government at stake (Galais and Blais (2016b)). For example, political interest is a stronger predictor of the duty to vote in national than in European elections. Education is, in turn, a stronger predictor of duty to vote in European than in national elections. Given these differences, the MEDW likely represents the best data set to test the connection between corruption and duty, and the mediation role of duty because it asks individuals about their sense of duty to vote in national, regional, European, and municipal elections. In doing so, the MEDW allows a more detailed exploration of the relationship between corruption and duty, and duty's mediation role (see Appendix D.1 for details on when all four civic duty questions are asked in the same survey).

Third, the MEDW encompasses a total of 25 (pre- and post-) election studies. Those were administered in each year between 2011 and 2015: specifically, seven studies were so in 2011 (one in Ontario, one in Catalonia, one in Madrid, two in Lucerne and one in Zurich); four studies in 2012 (one in Quebec, one in Île-de-France, one in Provence, and one in Catalonia); three studies in 2013 (two in Lower Saxony and one in Bavaria); seven studies in 2014 (one in Lower Saxony, one in Provence, one in Île-de-France, one in Paris, one in Marseille, one in Catalonia and one in Madrid); and four studies in 2015 (one in Quebec, one in Ontario, one in British Columbia, and one in Madrid) (see Appendix D.2 for information on the election covered in each year and locality). In this way, the MEDW allows a large, comparative study of the relationship between corruption and duty, and the mediation role of duty.

As indicated, duty is measured in the MEDW as follows: "People have different views about voting. For some, voting is a [duty/choice]. For others, voting is a [choice/duty]. For you personally, is voting, first and foremost, a Duty or a Choice? In national/regional/European/municipal elections". Those who report a sense of duty in that

question are subsequently asked: “How strongly do you feel personally that voting is a duty?”. Respondents can answer “very strongly”, “somewhat strongly”, or “not very strongly”.

As expected, fewer respondents in the MEDW data consider voting a duty in second-order than in first-order elections.⁴⁴ For example, 41% indicate that it is a duty to vote in European elections, which is 14 percentage points less than in national elections (see Table 4.1). A similar pattern is found for other second-order elections: for instance, 50% consider voting in municipal elections a duty, while 55% do so in national elections. Note that these values are likely to be not much affected by social-desirability bias, as the duty question is asked before an election. In addition, some individuals are first presented with “duty”, while others are first presented with “choice” in the MEDW surveys, implying the observed duty levels are unlikely the result of the way in which the duty question is structured.

Table 4.1. Mean of civic duty, self-reported turnout, and perceived corruption (hardly any – a lot) in the MEDW data

Election/ Government Level	Civic Duty Mean	Self-Reported Turnout Mean	Perceived Corruption Mean
National	0.55	.90	2.97
Regional	0.53	.86	2.84
European	0.41	.78	3.13
Municipal	0.50	.80	2.38

Notes: Unweighted pooled data from the Making Electoral Democracy Work (MEDW) project. Civic duty and self-reported turnout are measured on a 0-1 scale. Perceived corruption is measured on a 1-4 scale.

The MEDW also interviews individuals on their participation in national, regional, European, and municipal elections. It does so by means of one of two questions: “In each election we find

⁴⁴ Reif and Schmitt (1980) classify national elections as first-order elections. By contrast, they classify other elections like municipal elections, regional elections, and European elections as second-order elections. In this study, I follow Reif and Schmitt and classify elections in the same way.

that a lot of people were not able to vote because they were not registered, they were sick, or they did not have time. Which of the following statements best describes you? I did not vote in the election; I thought about voting this time but didn't; I usually vote but didn't this time; I am sure I voted in the election". Or "In each election we find that a lot of people were not able to vote because they were not registered, they were sick, or they did not have time. Were you personally able to vote in this election? Yes; No". Merging answers to these two questions, I find turnout rates to be 90, 86, 78, and 80% in national, regional, European, and municipal elections, respectively (see Table 4.1). Despite efforts to reduce social desirability bias in the MEDW surveys (e.g. by means of those questions' preamble), it comes with no surprise that respondents over-report their participation in these elections. Yet such over-reporting should not be a cause for concern, as the force of relationships is not much affected by whether turnout is measured by a self-reported or by a validated voting measure (Highton 2005).

Regarding corruption, the MEDW data measure this by asking respondents "Would you say that there is hardly any corruption, a little corruption, some corruption, or a lot of corruption in: The present federal/regional/European/municipal government". The quality of this question is it taps perception of corruption in different government levels, thus allowing a test of the relationship between corruption, civic duty and turnout in specific election/government levels.⁴⁵ Moreover, while relying on a subjective measure of corruption might be problematic because of potential individual-level biases (Anduiza, Gallego and Munoz 2013), research has shown that citizens and experts' perceptions of corruption are highly correlated (Charron 2016), reason for which many corruption studies rely on a similar measure of corruption (see, for example, Blais, Gidengil and Kilibarda 2015; Dahlberg and Solevid 2016; Kostadinova

⁴⁵ Still the results on the mediation effect of civic duty at the European level should be interpreted with a grain of salt, as it is unclear what respondents perceive as European government.

2009; McCann and Dominguez 1998; Sundström and Stockemer 2015). Consistent with the literature (Bąkowski and Voronova 2017), I find that perceived corruption is somewhat higher in national than in sub-national government levels: for instance, the mean score given to the municipal government is 2.38, while the mean score given to the national government is 2.97.

4. Method

I assess the relationship between corruption and civic duty, and the attitude's mediation effect by means of a two-step analytical approach. First, I regress each duty measure on the corruption measure that corresponds to the same government level, without and with controls, with the pooled MEDW data. For example, I regress respondents' sense of duty to vote in European elections on their perceptions of corruption in that level of government, first without and then with controls. In all regressions, I include study fixed effects to control for the influence of all unobserved context-specific factors that could bias the estimates of the perceived corruption items. I also cluster standard errors by election study because of dependent observations.

The control variables in some of those analyses are chosen based on relevant work on the duty to vote (Carreras 2018; Galais and Blais 2017; Goerres 2007). They correspond to four socio-demographic factors: gender, age, education, and religiosity. Female, older, more educated and religious individuals tend to be less tolerant of corruption and to consider voting as a duty more than their counterparts. In addition, I control for three political attitudes (political interest, political trust, and partisanship) given that individuals who are interested in politics, who are trustful of representatives, and who identify with any political party are generally less prone to perceive corruption in government and more likely to feel a sense of duty to vote than their

counterparts (Anduiza, Gallego and Munoz 2013; Blais, Gidengil and Kilibarda 2015) (see Appendix D.3 for some descriptive statistics of all variables in the analysis).

After testing the relationship between corruption and duty, I analyze the mediation effect of civic duty. To this end, I rely on the conventional Baron and Kenny (1986) mediation test. Not only it is applicable to fixed-effect models, Baron and Kenny's mediation test has been largely employed in the literature (e.g. see Blais and St-Vincent 2011; De Neve 2015; Moehler 2009; Mondak et al. 2010; Van Spanje and Azrout 2019). The test comprises three regressions: one in which civic duty is regressed on perceived corruption, another in which self-reported turnout is regressed on perceived corruption, without civic duty as a control variable, and a third regression in which self-reported turnout is regressed on perceived corruption, but this time with civic duty as a control variable. If civic duty and self-reported turnout are each correlated with perceived corruption, and if the coefficient associated with perceived corruption declines with the inclusion of civic duty in the regression, it means that civic duty mediates the relationship between perceived corruption and turnout. If not, the contrary holds.

In line with Baron and Kenny's study, I conduct Sobel z -test as well.⁴⁶ This test indicates how significant is the indirect effect of perceived corruption on self-reported turnout through civic duty, if there is evidence of a mediation effect. A statistically significant z -score means that a null hypothesis of no mediated (indirect) effect can be safely rejected. If insignificant, this score means that a null hypothesis of no mediated effect cannot be safely rejected.

⁴⁶ The Sobel z -test is expressed mathematically by the formula $\sqrt{b^2 s_a^2 + a^2 s_b^2 + s_a^2 s_b^2}$, where a corresponds to the coefficient associated with the effect of perceived corruption on civic duty, b , with the effect of civic duty on self-reported turnout (with perceived corruption in the regression), and s , with the respective standard errors.

As every approach to mediation analysis, Baron and Kenny's tests have some limitations, which have led scholars to propose an approach that is outlined in Imai, Keele and Tingley (2010). Following prior work that has used Imai, Keele and Tingley's mediation test (e.g. Becher and Donnelly 2013, and Baker 2015), I conduct these tests to examine the mediation effect of civic duty, and present the corresponding results as additional evidence of a mediation effect of duty or not.⁴⁷ I concentrate on the results from Baron and Kenny's test because Imai, Keele and Tingley's test does not allow the inclusion of fixed-effects, which may lead to biased estimates. Furthermore, only minor differences are found in the estimates from Baron and Kenny's mediation tests on the one hand, and Imai, Keele and Tingley's mediation tests on the other (Karpowitz, Mendelberg and Shaker 2012).

5. Results

For testing the relationship between perceived corruption and civic duty across different election levels (the first step of the analyses), I conduct logistic regressions without and with control variables. Table 4.2 reports the results. They indicate a statistically significant negative correlation between perceived corruption and sense of duty, a finding that is consistent across national, regional, European, and municipal elections. Substantively, this means that individuals are *less* likely to consider voting in national, regional, European, and municipal elections a duty when they perceive the corresponding government to be corrupt, and *more* likely to believe in the duty to vote in these elections when they do not perceive the corresponding government to be corrupt. Importantly, these results remain significant and in the negative direction when including control variables in the regressions, although doing so weakens the estimated association between perceived corruption and civic duty.

⁴⁷ For these additional tests, I use the "mediation" package in Stata 15.

Table 4.2. Association between perceived corruption and civic duty to vote

	DV: Civic Duty to Vote							
	National		Regional		European		Municipal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption: National (Hardly any – A lot)	-0.167*** (0.021)	-0.094*** (0.021)						
Corruption: Regional (Hardly any – A lot)			-0.166*** (0.024)	-0.094*** (0.021)				
Corruption: European (Hardly any – A lot)					-0.177*** (0.028)	-0.130*** (0.025)		
Corruption: Municipal (Hardly any – A lot)							-0.155*** (0.022)	-0.116*** (0.020)
Political Trust		0.019 (0.032)		0.020 (0.035)		0.059+ (0.033)		0.007 (0.028)
Partisanship		0.333*** (0.035)		0.297*** (0.031)		0.265*** (0.047)		0.260*** (0.052)
Political Interest		0.112*** (0.020)		0.111*** (0.024)		0.091*** (0.020)		0.086*** (0.023)
Female		0.006 (0.039)		0.033 (0.038)		0.155** (0.055)		0.008 (0.039)
Age		-0.004 (0.004)		-0.001 (0.005)		-0.005 (0.004)		0.003 (0.004)
Education		0.007 (0.054)		0.043 (0.049)		0.095* (0.044)		0.019 (0.051)
Religiosity		0.174*** (0.037)		0.200*** (0.054)		0.177*** (0.048)		0.255*** (0.040)
Constant	-0.141** (0.049)	-1.218*** (0.340)	-0.360*** (0.052)	-1.648*** (0.346)	0.465*** (0.078)	-0.634* (0.258)	-0.270*** (0.046)	-1.449*** (0.358)
Fixed Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	28,446	18,453	24,733	16,464	15,440	10,237	22,106	15,063

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. Study fixed-effects' coefficients are not reported.

Log odds are hardly interpretable. For this reason, I compute the average marginal effects of the perceived corruption measures as well, and list them in Table 4.3. While the coefficients are statistically significant and in the negative direction, they are surprisingly small: each unit

increase in the perceived corruption measures is associated with a 2 p.p. decline in citizens' likelihood of considering voting in national, regional, European, and municipal elections a duty. As such, while there is an effect of corruption perceptions on how dutiful a person feels (irrespective of the election level), that effect is substantively weak.

Table 4.3. Average marginal effect of perceived corruption on civic duty to vote

	DV: Civic Duty to Vote			
	National	Regional	European	Municipal
Corruption: National (Hardly any – A lot)	-0.020*** (0.004)			
Corruption: Regional (Hardly any – A lot)		-0.020*** (0.004)		
Corruption: European (Hardly any – A lot)			-0.029*** (0.005)	
Corruption: Municipal (Hardly any – A lot)				-0.025*** (0.004)
N	18,453	16,464	10,237	15,063

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to changes in predicted probabilities of considering voting a duty than a choice. The corresponding log-odds coefficients can be seen on Table 4.2, columns 2, 4, 6 and 8.

The weak connection between perceived corruption and sense of duty suggests that this attitude plays a minor role in explaining why a low turnout normally results from a high level of corruption. To verify whether that is indeed the case, I shift the focus to the mediation tests – the key question in these tests is what fraction of the effect of perceived corruption on turnout is mediated by civic duty. The results from the conventional Baron and Kenny (1986) mediation test suggest that civic duty mediates the relationship between perceived corruption and turnout, a finding that is also confirmed by Sobel z-tests.⁴⁸ However, the

⁴⁸ The corresponding z-scores for the national, regional, European and municipal mediation tests are 4.61, 4.55, 4.92, and 5.44, all of which with a standard error of 0.001, and a p -value < 0.001 , meaning the indirect effect of perceived corruption on self-reported turnout through individuals' sense of civic duty is statistically significant at conventional levels.

estimated mediation effect of civic duty is small. As shown in Table 4.4, the estimated coefficient declines from -0.246 to -0.204 , -0.297 to -0.263 , -0.244 to -0.183 , and -0.336 to -0.288 (i.e. 17, 11, 25, and 14%), respectively, when adding civic duty in the national, regional, European, and municipal regression models.⁴⁹ This substantively small mediation effect is found in Imai, Keele and Tingley's (2010) mediation tests too: the estimated percent of civic duty's mediation effect is 12, 16, 40, and 11 in national, regional, European, and municipal elections, respectively (results not shown).

Not only the estimated mediation effect of duty is small, the statistical significance of civic duty's mediation effect fails tests of the sequential ignorability assumption (i.e. the estimated mediation effect of duty is unlikely conflated due to omitted variables). In fact treating civic duty and turnout as continuous variables, as in Baghdasaryan, Iannantuoni and Maggian (2019), and Finseraas (2017), I not only find an ACME of zero when ρ equals 0.1, in the case of turnout in national elections, and 0.2 in all other cases (i.e. turnout in regional, European and municipal elections), the R^2 that needs to be explained by an unobserved confounder for a nil mediation effect of civic duty is also very small (0.04 or lower) (see Appendix D.7).

⁴⁹ Given that Baron and Kenny's mediation approach entails a system of equations, a different number of respondents in the equations may lead to biased estimates of the mediation effect of civic duty. Using a single duty measure per individual, as in the case of the turnout measure, yields the same findings: the mediation effect of civic duty remains weak. Evidence of such an effect even weakens in the case of national, regional and municipal elections. Comparing predicted probabilities, as well, leads to the same conclusion about the mediation of civic duty (see Appendices D.4– D.6).

Table 4.4. Association between perceived corruption and self-reported turnout

	DV: Self-Reported Turnout							
	National		Regional		European		Municipal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption: National (Hardly any – A lot)	-0.246*** (0.070)	-0.204*** (0.076)						
Corruption: Regional (Hardly any – A lot)			-0.297*** (0.083)	-0.263** (0.092)				
Corruption: European (Hardly any – A lot)					-0.244*** (0.049)	-0.183*** (0.052)		
Corruption: Municipal (Hardly any – A lot)							-0.336*** (0.022)	-0.288*** (0.013)
Civic Duty		1.316*** (0.162)		1.478*** (0.153)		1.318*** (0.163)		1.268*** (0.004)
Constant	2.396*** (0.179)	1.937*** (0.207)	2.242*** (0.187)	1.831*** (0.221)	2.174*** (0.156)	1.400*** (0.160)	2.474*** (0.064)	1.648*** (0.036)
Fixed Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12,250	12,250	4,886	4,886	2,983	2,983	1,247	1,247

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All self-reported turnout items are dichotomous: 0=no; 1=yes. Adding civic duty in the regressions lead to a 17, 11, 25, and 14 percent decline in the effect of the perceived corruption measures corresponding to national, regional, European, and municipal elections, respectively – and 12, 16, 40, and 11 percent in a mediation test without fixed effects (see Imai, Keele and Tingley 2010).

To gain additional knowledge on the magnitude of civic duty's mediation effect, I also examine how much corruption affects turnout through external efficacy and political disenchantment, which constitute overall weaker turnout determinants than civic duty (Smets and Van Ham 2013). As shown on Appendices D.8–D.10, civic duty performs in most cases a weaker mediation role when compared to these two variables, and to external efficacy, in particular: indeed, despite civic duty's systematically strong effect on turnout, external efficacy and political disenchantment stand out as stronger mediators than civic duty in the context of the link between corruption and turnout in national and regional elections. When it comes to turnout in municipal elections, external efficacy mediates as much as civic duty the corruption-

turnout nexus. In only one case (turnout in European elections) civic duty turns out to be a stronger mediator than external efficacy and political disenchantment.

Taking the same comparative approach, I also verify whether the mediation effect of civic duty surpasses that of the benefit and cost of voting, variables that are assumed to also explain corruption's effect on turnout (Stockemer, LaMontagne and Scruggs 2013; Warren 2004). Although the cost of voting and civic duty have a similar performance (the cost of voting is a stronger mediator in two cases, while civic duty is stronger in the other two), the benefit of voting consistently accounts for a larger share of the corruption-turnout association than duty (see Appendices D.11–D.16). We may thus conclude that the mediation effect of duty is not only likely to be insignificant, it is also substantively weak, compared to alternative mediators.

6. Supplementary tests

In the previous analyses, I found that perceived corruption is weakly associated with sense of civic duty, and that the attitude has a weak mediation effect. An explanation for these small effects might regard respondents' age, as this factor is positively correlated with attitude stability (Krosnick and Alwin 1989). However, the estimated effect of perceived corruption on civic duty does not vary much by respondents' age. As can be seen in Appendix D.17, in one case out of four (in the regression of duty to vote in municipal elections) the interaction of perceived corruption and age is statistically significant at $p\text{-value} < 0.05$, and in the expected (positive) direction. Hence, the overall weakness of perceived corruption's effect on civic duty is fairly general, and not driven by older respondents who are already set in their ways.

In addition, although self-reported turnout is common in the literature, such a turnout measure might be plagued with nonresponse bias given that assuming a socially deviant behavior tends to be challenging for survey respondents. I address nonresponse bias and overreporting in the MEDW data by means of post-stratification survey weights and a face-saving turnout question. Research has relied on the same techniques to address these issues (Malhotra, Monin and Tomz 2019; Ward 2019; Morin-Chassé et al. 2017). Correcting for nonresponse bias and overreporting in my turnout measures does not lead to an increase in the mediation effect of civic duty; rather, this effect weakens or turns insignificant (see Appendices D.18–D.24).

7. Discussion and conclusion

The observed results are in line with the central hypothesis of this paper: corruption is negatively associated with citizens' sense of duty to vote. Civic duty also accounts for part of the relationship between perceived corruption and turnout. Importantly, these results hold regardless of the election level. However, the observed results also suggest that the relationship between corruption and civic duty is weak. So is the mediation effect of civic duty. As such, these findings go against past works suggesting a strong mediation role of civic duty (i.e. Bowler and Donovan 2013; Jones and Hudson 2000; Sundström and Stockemer 2015).

This study has some limitations. Most importantly, the MEDW data do not allow ascertaining the causal direction between the dependent and independent variables. It is possible that the relationship between perceived corruption and civic duty also runs in the opposite direction – that is, from civic duty to perceived corruption. Yet an effect of civic duty on citizens' perceptions of corruption means the observed results are in fact conservative, and that the relationship between these variables might be even weaker than what is reported here.

Despite this limitation in the cross-sectional nature of the MEDW data, these data have a number of features that allow a comprehensive systematic study of the mediation effect of civic duty. First and foremost, the MEDW data employ what is perceived as the best means of capturing individuals' sense of duty to vote: the duty/choice question (Blais and Achen 2019). In this way, we can be confident that the results are not driven by a poor measure of the variable of interest (civic duty). Second, respondents to the MEDW surveys are interviewed about their sense of duty to vote in four election types: national, regional, European, and municipal. To my best knowledge, in no other data, we find such measures. Third, the MEDW measures civic duty in the same way across 25 elections, from five countries. Such a large amplitude of the MEDW data is unique, as far as civic duty is concerned. In short, exploring the mediation effect of civic duty seems impossible without the MEDW data.

In conclusion, this study makes two important contributions to the literature. First, while previous research assumed a strong relationship between corruption and turnout through civic duty, this study provides empirical evidence that goes in the opposite direction. More specifically, civic duty plays a marginal mediation role, and this also when compared to other turnout determinants. Second, and more broadly, this study provides valuable insights into what may drive citizens' sense of duty. Consistent with previous works that found a relationship between compulsory voting rules and civic duty (Feitosa and Galais 2019), as well as between individual-level experiences with civic education in school and civic duty (Galais 2018), this study provides evidence suggesting that long-term factors may be more influential on civic duty than short-term but salient factors, such as corruption. This insight into the sources (and consequent nature) of civic duty may prove itself useful not only for specialists on civic duty but also for scholars who employ standard models of voter turnout in their

research: by understanding better where civic duty comes from, these scholars may be in a better position to study and explain why citizens vote in elections.

Conclusion

This dissertation had two main objectives. The first objective was testing the stability of civic duty. The second goal was testing the predictive role of civic education, compulsory voting, and corruption. Exploring the stability of duty and the role of those factors provides important insights about what it means to feel a duty to vote and where duty comes from. In what follows, I summarize the main findings from the four empirical chapters, and I point out what I believe to be important contributions from this dissertation to the civic duty literature.

1. The stability of civic duty

The first objective of this dissertation was exploring the stability of duty. This was done in Chapter 1. In this chapter, I used structural equation models because they allow accounting for errors in the survey-based measures of duty. I fitted those models to two nine-wave panel studies – one from Spain and the other from the United Kingdom. With this research strategy, I found that duty is a very stable political attitude. More precisely, stability coefficients were often above .94, that is, close to the maximum possible value of 1.00. In a few instances the stability coefficients were actually 1.00, implying complete stability. This led me to conclude that duty is a very stable political attitude, like political interest and partisanship.

The high stability of duty provides some important insights about the nature of duty. To begin with, it informs us about the role that rationalization plays in reported levels of duty. A common critique among students of the duty to vote is that duty is merely the rationalization of individuals' voting behavior. Specifically, it is argued that individuals who voted in one election will report a sense of duty to vote. In turn, those who abstained in an election will

report that voting is a matter of personal choice. In other words, it is argued that individuals “generate rationales that are more plausible than veridical” in order to show consistency between their opinions, attitudes and behaviors (Lodge and Taber 2013, 22).

There is much evidence of rationalization in the public opinion literature. For example, citizens appear to rationalize their expectations of how secession will impact the economy of a place to match their prior support (or not) of the secession (Munõz and Tormos, 2015). They also rationalize their policy preferences (Lachat 2008) and their partisanship (Dinas 2014) to match their vote. Furthermore, research shows that individuals rationalize their vote to match their class or ethnicity (Mohamed 2018). And that they rationalize their health condition (whether sick or not) to match their voting behavior (Pacheco and Fletcher 2015).

The fact that duty is very stable – and so even among individuals who switched behavior – suggests, however, that rationalization does not strongly affect reported levels of duty. In line with Blais and Achen (2019), if duty was mainly the result of rationalization, duty should not be a very stable attitude. The reason is individuals would change their responses to a duty question when they switch behavior between elections. For example, those who voted in an election but abstained in a subsequent one would change from duty to choice. And those who abstained in an election but voted in a subsequent one would change from choice to duty. My analyses of the stability of duty among those individuals indicate no variation in their reported sense of duty. They are likely to report that voting is a duty or a choice in adjacent waves irrespective of their voting behavior. As such, it seems likely that, unlike partisanship or individuals’ policy preferences, duty is not much contaminated by rationalization.

The high stability of duty also provides insights into a related and important aspect of the duty-turnout nexus: the potential of duty in explaining why individuals vote despite being an irrational act. In fact, the classic rational model of turnout posits that individuals vote when the benefits of voting in an election, multiplied by the probability of casting a pivotal vote, are higher than the costs from voting (Downs 1957). As such, voting is irrational because individuals generally have little influence on who wins an election (Blais and Daoust 2020). But most individuals vote in elections. To explain this paradox, Riker and Ordeshook (1968) propose the addition of a D term in the rational model of turnout, yielding a voting equation that reads $V = B * P - C + D$, where V stands for voting or abstaining in an election, B the expected benefits of voting in an election, C the costs from voting, and D the sense of duty to vote. Hence, according to this model, many individuals vote because of a sense of duty to vote. Yet duty can provide an explanation for why individuals vote only if it mainly precedes turnout. The finding that duty is very stable suggests that this is likely to be the case and, consequently, duty may represent a motivation to vote even though voting is an irrational behavior.

The high stability of duty also provides insights into the extent to which duty is conditional on election-specific factors. For quite some time now, some researchers defend the view of duty being an attitude that is in part responsive to external, change-inducing stimuli. Aldrich (1993, 273), for example, argues that “the D term is not politically inert, but may include highly political, even election-specific values”. Following this reasoning, Aldrich argues that “strategic politicians can both ‘manipulate’ costs and, perhaps, sense of duty” (Aldrich 1993, 273). Kanazawa (1998) sees duty in a similar way. He argues that both duty and voting benefits are subject to stochastic learning processes, meaning that individuals update their sense of duty depending on electoral results. More precisely, “if a citizen votes for a candidate who wins the election, then both her instrumental and normative behavior are reinforced and she becomes

somewhat more likely to vote again in the next election because her p and D are both larger now than before” (Kanazawa 1998, 983). In more recent study, Goodman (2018) reinforces this view of duty. She argues that the strength of duty “varies depending on considerations related to political information and the importance or competitiveness of an election” (Goodman 2018, 42). That is, duty is stronger when citizens are well informed and in competitive elections. Yet the high stability of duty suggests it is likely not much affected by election-specific factors. If duty was strongly affected by election-specific factors, duty would change far more than what Chapter 1 revealed. In short, some degree of responsiveness to election-specific factors is possible, but duty seems mostly impervious to those factors.

A related observation that follows from the finding that duty is highly stable concerns the sensitivity of duty to get-out-to-vote (GOTV) appeals. Scholars who study the impact of GOTV messages often assume that GOTV messages can trigger a sense of duty within individuals. Michelson (2003), for instance, believes that duty appeals can foster individuals’ sense of duty and, as a result, yield an increase in voting. Gerber et al. (2008) also defend the possibility of triggering duty by means of GOTV appeals. More precisely, they argue that their duty appeals led to an augmentation of duty in the population, and, consequently, a 1.8 p.p. increase in turnout. Another example of a study that argues that duty appeals have an effect on duty is Arceneaux and Nickerson (2009). They affirm that “a GOTV campaign could be effective via the benefit term by persuading individuals to care about the outcome of the election, through the cost term by lowering the informational costs associated with voting, [or] through the duty term by reminding people about their role as democratic citizens” (p 12). That duty can be fostered by means of duty appeals seems quite plausible for all these scholars. However, the high stability of duty suggests that duty appeals might at best put some additional pressure on dutiful citizens to comply with their sense of duty and vote (see Mason and Mason, 1992, for

a similar argument regarding individuals' tax payment). In other words, duty appeals might be able to affect individuals' willingness to comply with their sense of duty and vote in elections, which is not the same thing as fostering their sense of duty to vote.

A second (and final) related aspect that we learn from the high stability of duty involves the extent to which duty is conditional on the voting behavior of others, something discussed in Fieldhouse and Cutts (2018). Based on social norm research (e.g. Maloney, Lapinski, and Neuberger 2013; Park et al. 2009), Fieldhouse and Cutts propose a three-way classification of social norms of voting. The first type of social norms is what they call *normative beliefs*. Normative beliefs “represent how a person believes they ought to behave”. As such, they “do not depend on what others do” (Fieldhouse and Cutts 2018, 2). The second type of norms is called *descriptive norms*. They differ from normative beliefs in that “behavior is conditional on empirical expectations. That is, a person prefers to vote if people in their relevant reference network vote” (Fieldhouse and Cutts 2018, 2). The third type of social norms is called *injunctive norms*. These norms rely “on a perception of how others believe one should behave”, and not the behavior of people from a relevant network (Fieldhouse and Cutts 2018, 2). To put it differently, an individual votes because people from a relevant network believe a citizen must vote, not because those people vote. The high stability of duty suggests that Fieldhouse and Cutts may be right in classifying duty as a normative belief that is independent from others' behavior. In fact, duty would be less stable if this was not the case.

In conclusion, I believe that the high stability of duty tells much about the nature of duty. It suggests that duty is a deeply held belief that is not strongly affected by election-specific factors, GOTV appeals or others' voting behavior. In the following section, I discuss the

implications of the findings regarding the second objective of this dissertation: if and the extent to which civic education, compulsory voting and corruption affect duty.

2. The sources of duty

Having established that duty is a very stable attitude, the second objective of this dissertation was examining the role of context-based factors in the formation of duty. More precisely, I studied whether and to what extent civic education, compulsory voting, and corruption affect individuals' perception of voting as a duty or not. Chapter 2 focused on the first of those factors: civic education. In this chapter, I showed that three common forms of civic education (civics courses, active learning strategy, and open classroom climate) contribute to a stronger sense of duty to vote, but civics courses have the most potential when it comes to fostering duty.

In Chapter 3, I moved to the analysis of the second of those factors: compulsory voting. I showed that duty declined substantially after the Chilean 2012 electoral reform bringing compulsory voting rules to an end. Placebo tests revealed that the decline in duty is unique to Chile, the only Latin-American country where compulsory voting was abolished in 2012. And cross-national assessments of the relationship between compulsory voting and duty with data from the 2004 and 2014 Citizenship Modules of the International Social Survey Programme (ISSP) confirmed the findings from the quasi-experiment.

Finally, in the fourth and final empirical chapter of this dissertation, I showed that corruption has a marginal effect on duty. Specifically, I relied on data from the MEDW project, and two approaches to study mediation – one outlined in Imai, Keele and Tingley (2010), the other in Baron and Kenny (1986) – to show that an increase in perceived corruption is associated with

a small decline in duty. In addition, adding duty in turnout models yielded a marginal decline in the effect of perceived corruption on the decision to vote or not in an election.

These findings provide four important insights into what it means to feel a duty to vote and how sense of duty is formed. First, we know from the education literature that the schools and education can reinforce “the inequalities of social structure and cultural order found in a given country” (Collins 2009, 34). That is, “parents’ social class and cultural capital are reproduced and then reflected in those of their children” by means of the school (Aschaffenburg and Maas 2016, 575). The mechanism by which the school reinforces social inequalities is twofold. First, parents’ economic status determines pupils’ achievement in the school (Cheng and Kaplowitz 2016). Research shows, for instance, that “having a scientist in the family and higher SES are positively associated with better outcomes” in science (Diamond 2020, 1029). Second, individuals’ socio-demographics – which are correlated with their parents’ socio-demographics – determine access to certain types of education in the school. It has been shown, for example, that high school seniors with no post-secondary education plans report “significantly fewer opportunities to develop civic and political capacities and commitments than those with post-secondary plans” (Kahne and Middaugh 2009, 13). Moreover, research shows that African-Americans are “significantly ($p < .05$) less likely than white students to report having civically oriented government courses, discussions of social problems and current events, and experiencing an open classroom climate” (Kahne and Middaugh 2009, 13).

Some scholars question, however, that the school reinforces social inequalities. Neundorff, Niemi, and Smets (2016), for example, offer evidence that the school can foster political attitudes and behaviors especially among those who are little exposed to politics at home, implying that the school can, in fact, compensate for poor parental socialization into politics.

My findings suggest no compensation effect when it comes to the development of duty. In fact, interacting parents' political interest and social status with the three civic education indices – accounting for civics courses, active learning strategies, or an open classroom environment – reveals no heterogeneous effects among those who are little exposed to politics at home or who have wealthier parents. As such, it is possible that the school contributes to an unequal development of duty among pupils, in places where duty is unequally distributed.

The second insight relates to the development of sense of duty to vote in compulsory voting contexts. Prior work in psychology and sociology suggests that the “internal” and “external” motives for doing a behavior like paying taxes are contradictory. Deci and Cascio (1972), for example, report that negative feedback and threats of punishment (stronger external motivation) can decrease intrinsic motivations for solving puzzles. Following the same logic, Borgonovi (2006) reveals that public support and private donations to American theaters can be substitutes depending on the size of government support. More precisely, individuals will donate less to theaters when the government makes large donations.

Within the voting behavior literature, some political scientists argue that duty will be lower in places where voting is a legal obligation. The reason is that legal sanctions for abstaining in elections will reduce individuals' “internal” motives for voting like their sense of duty to vote (Maldonado 2015). It turns out, however, that compulsory voting contributed to a widespread sense of duty in the Chilean population, implying that compulsory voting actually fosters the development of duty. As such, my findings are in line with the view that laws “can communicate certain normative standards to society” (Van Der Burg 2001, 40). When it comes to duty, compulsory voting rules may signal that a “good” citizen must vote in election, thus serving as a source of duty (Quintelier, Hooghe, and Marien 2011; Chapman 2019).

My findings also inform about the belief that a citizen must vote because she *owes* it to her state. Bowler and Donovan (2013), for example, argue that “citizens have a set of duties and obligations to the state [like voting in elections] in return for which the state will provide or perform a set of obligations” (Bowler and Donovan 2013, 268). Hur (2017, 2020) also conceives duty as a moral obligation toward the state. She claims that “a sense of ethical obligation to the group’s welfare” can lead to a sense of duty toward the state “when the state is seen to represent ‘my’ nation” (Hur 2020, 3). The idea that duty results in part from a feeling of obligation toward the state is theoretically interesting. But the results that are presented in this dissertation – specifically in Chapter 4 – suggest that an obligation toward the state is unlikely to represent the main reason why some individuals believe in the duty to vote. In fact, we know from the corruption literature that citizens are highly concerned with corruption in government (Agerberg 2020). Moreover, individuals who are aware of corruption in government tend to punish those involved electorally, suggesting that corruption represents an important element in an implicit social contract between the public and the state (Ferraz and Finan 2008). As such, we may interpret the weak impact of corruption on duty as evidence that duty can include a sense of obligation toward the state, but that it is unlikely to represent the main reason why some individuals believe in the duty to vote.

The fourth (and final) insight pertains to the influence of democracy on the development of duty. Galais and Blais (2017) show with large, cross-national data from the 2nd and 3rd waves of the AsianBarometer that duty is generally higher in countries classified as “free” by Freedom House, than those classified as “partly free” or “not free” by the same institution. Unfortunately, the authors are not able to provide more detailed evidence on the mechanisms linking democratic development to duty. While we know that corruption is strongly correlated

with democratic development, as it depends on “the ability of voters to monitor their representatives, to detect those responsible for unsatisfactory outcomes, and to hold them accountable by voting them out of office” (Tavits 2016, 218), my analyses suggest corruption unlikely represents the main reason why duty is higher in developed than in developing democracies. If it was the case, corruption would affect more strongly individuals’ sense of duty. As my analyses show, individuals are only slightly less likely to feel a duty to vote in more corrupt contexts than in their less corrupt counterparts.

In short, while studying the relationship between civic education, compulsory voting, corruption and duty, this dissertation furthers our understanding of the nature of duty and how sense of duty is formed. To be more precise, this dissertation suggests that, in contexts where duty is unequally distributed, the school may yield an unequal development of sense of duty among pupils. In addition, it suggests that compulsory voting (an “external” motive for voting) fosters (rather than erodes) sense of duty (an “internal” motive). Finally, this dissertation suggests that an obligation toward the state is unlikely the main reason why some individuals believe in the duty to vote, and that corruption is unlikely the main mediating factor in the democracy-duty nexus. In what follows, I discuss the main limitations of this dissertation.

3. Limitations

While making important contributions to the civic duty literature, this dissertation is limited in at least three ways. The first limitation relates to the civic duty measure I employed in the empirical chapters. In an overview of different duty indicators, Blais and Galais (2016) discuss the merits and limits of the various duty measures in survey research. More precisely, they indicate that the most common duty question, the “good” citizenship question, is problematic

because it measures the presence of a social norm to vote, not necessarily whether individuals personally consider voting as a duty. That is, the “good” citizenship question taps what Gallego and Oberski (2012) consider the first step in the translation of voting norms into actual actions (the second step entails the internationalization of the voting norm, the third step acting out of a sense of duty to vote). In addition, Blais and Galais note that there is a clear risk of strong desirability bias in the “good” citizenship question, “as the great majority of respondents say that it is very important for the good citizen to vote” (Blais and Galais 2016, 61). They see other common duty measures such as agree/disagree questions as problematic too. The reason is that “some respondents think only superficially about an offered statement and do so with a confirmatory bias” (Holbrook, Green, and Krosnick 2003, 85). And, given that voting represents a widespread norm in many contemporary democracies, individuals may pay lip service and falsely report a sense of duty in agree/disagree questions.

In turn, Blais and Galais have a very positive opinion about a recent duty question that is proposed in Blais and Achen (2019). This question reads: “For you personally, is voting in an election first and foremost a duty or a choice?”. Individuals who answer “duty” in this question are subsequently asked: “How strongly do you feel that voting in an election is a duty: very strongly, somewhat strongly, or not very strongly?”. The reason why Blais and Galais (2016) are so positive about the duty/choice question is that it provides a worthy alternative to duty (choice), thus reducing the risk of social desirability bias in survey responses. In addition, the duty/choice question directly asks individuals about their view of voting as a duty (or not). For these reasons, Blais and Galais defend measuring duty mainly through a duty/choice question.

I am convinced about the qualities of the duty/choice question, and the reasons for operationalizing duty through this question. Yet, unfortunately, few surveys contain a

duty/choice question, making it difficult to study civic duty with this question. When it comes to my dissertation, while I examined the stability of duty and the effect of corruption on duty with a duty/choice question, it was impossible to explore the connection between civic education, compulsory voting and duty with a duty/choice question. As a result, the results from Chapters 2 and 3 might mean that civic education and compulsory voting foster individuals' perception of voting as a citizenship norm, but not their sense of duty to vote.

The second limitation concerns the generalization of the results. This dissertation focuses on a limited set of countries. In fact, I tested the stability of duty with data from Spain and the United Kingdom. In addition, the ICCS dataset that was used in Chapter 2 contains mainly advanced democracies from North America and Europe. In a similar way, the analysis of the impact of compulsory voting on duty is based on the abolition of compulsory voting in Chile. And the MEDW includes individuals from five countries only (Canada, France, Germany, Spain, and Switzerland). As such, there is a clear risk that the results are not generalizable.

To be sure, I believe that the results would not be too different if I used other datasets with more countries. In particular, with respect to the impact of civic education on duty, while there is an overrepresentation of advanced democracies in the ICCS dataset, no discernible trend across regions or groups of countries is observed in additional country-specific results. Regarding the effect of compulsory voting on duty, cross-national analyses with the 2004 and 2014 Citizenship Modules of the ISSP reveal a clear link between compulsory voting and duty. Finally, as for the connection between corruption and duty, there is enough variability in the MEDW dataset to allow a reliable estimation of the effect of corruption on duty. Still, I am not capable of assessing in this dissertation if we would find high stability in other countries, and

if the results would be the same when it comes to the impact of civic education, compulsory voting, and corruption on duty with more countries – especially developing democracies.

The third and final limitation involves the use of observational data in this dissertation. Most importantly, there is a risk that I misestimated the relationship between civic education, corruption and duty because of my reliance on observational data. Although I took some precautions to prevent biased estimations like aggregating all civic education measures at the school-level, controlling for all potential confounders in the statistical models, and conducting many robustness tests, school characteristics affecting civic education and duty, and attitudinal factors influencing corruption perceptions and duty might bias the results.

4. Avenues for future research

My dissertation offers evidence that duty is a very stable attitude, and that civic education and compulsory voting contribute to the development of duty, but corruption has a marginal effect on how dutiful a person feels. With these findings, I believe my dissertation opens up many research avenues. In what follows, I discuss the five most relevant avenues.

First, I think future research should re-examine the relationship between civic education and civic duty, and it should investigate whether this relationship holds with other datasets too. As of now, only two studies explore the extent to which exposure to civic education fosters a sense of duty: Galais (2018) and this dissertation.⁵⁰ While we both offer evidence that duty is in part the result of exposure to civic education in the school, prior work could not find an effect of

⁵⁰ Campbell (2006) makes a connection between the school and duty, but he focuses on the degree to which duty is conditional on the level of shared partisanship within a school, not civic education.

civic education on duty-related political attitudes and behaviors. For instance, Weinschenk and Dawes (2021) reveal with American data that “the best-case scenario is that civic education exerts very small effects” on individuals’ voting behavior, implying that civics courses might actually have no impact on duty (Weinschenk and Dawes 2021, 13).

To provide reliable estimates of the effect of civic education on duty, I think it would be useful to study the education-duty nexus through an experiment. With this experiment, researchers would be able to obtain reliable estimates of the effect of civic education on duty because civic education would be exogenous. Moreover, no omitted variables like school characteristics would bias the results. In this sense, researchers could contact, for instance, school directors and propose their participation in a study on the effect of civic education on individuals’ attitudes. Those who agree to participate in the study would then implement either civics courses or active learning strategies (e.g. a student assembly), they would foster an open classroom environment in school, or they would manipulate no educational element of the school, but they would still allow interviewing students on their sense of duty to vote (see Persson et al., 2020, for an experimental design along these lines). An extension of this experiment could include variations of the civics course materials. For example, some materials contain a debate about different citizenship norms (like the norm to vote), while others do not.

Researchers conducting this experiment could also explore the long-term effect of exposure to civic education by administering follow-up surveys when individuals reach adulthood and when their political attitudes and behaviors are more established. The 2006-2011 Belgian Political Panel Survey (BPPS) represents a good example of how that panel research could look like (Hooghe et al. 2011). The BPPS interviews a sample of Belgians at 16 years old, when individuals are still in school. The BPPS subsequently interviews the same individuals

at 18 years old, when they become eligible to vote. The third and final survey of the BPPS takes place when individuals are 21 years old. In this way, although the BPPS does not include a duty question, it provides a good example for an experiment that would allow evaluating the long-term effects of exposure to civic education on individuals' sense of duty.

Second, future research should investigate the mediation effect of duty with respect to the connection between compulsory voting and electoral turnout. While Chapter 3 offers evidence that duty is strongly correlated with compulsory voting, it seems worth investigating if and the degree to which duty accounts for the relationship between compulsory voting and turnout. Besides duty, this research could include other attitudinal factors like partisanship and political knowledge for comparative purposes. Both partisanship and political knowledge are shown to be influenced by the presence of compulsory voting rules in a country (e.g. see Singh and Thornton 2013; De Leon and Rizzi 2016; Gordon and Segura 1997; Sheppard 2015; Shineman 2018). For this research, scholars could use the 2004 and 2014 Citizenship Modules of the International Social Survey Programme (ISSP), as they include measures of duty and partisanship. While there are not specific political knowledge questions in the 2004 and 2014 ISSP, researchers could merge education level and political interest and use it as a proxy of political knowledge. In line with Chapter 4, researchers could use those datasets and estimate the mediation effect of duty, partisanship and knowledge with a sequence of three regressions (Baron and Kenny 1986) or a single model (Imai, Keele, and Tingley 2010).

An extension of this project could look into how influential is duty, in comparison with the voting (or abstaining) cost. In other words, which of those two factors would be most relevant when it comes to explaining the link between compulsory voting and citizens' participation in elections. The absence of individual-level data containing both duty and cost measures is an

obstacle to this study. As such, researchers would need to field original surveys. In those surveys, duty could be measured by Blais and Achen's duty/choice question. And cost by questions tapping different dimensions like how easy or difficult it is to go to the polling station and to decide for whom to vote (see Blais et al. 2019 for a similar approach). The combination of these measures with how costly it is to abstain in an election would provide a good indication of the cost for each individual that is involved in voting (or abstaining) in an election.

Third, when it comes to the relationship between corruption and duty, we know from the corruption literature that corruption perceptions differ from corruption experience. For example, Klasnja et al. (2014, 71) examine the association between corruption perceptions and experience with Eurobarometer and Transparency International data, and they report a weak correlation between exposure and perception of corruption. In light of the difference between corruption perceptions and experience, Klasnja et al. argue that corruption experience might affect individuals' voting behavior more than perceptions because "being asked to pay a bribe imposes a direct cost on the citizen and provides a highly precise signal about this aspect of performance of the political system" (73). The 2nd and 3rd waves of the AsianBarometer include a question on corruption experience, as well as a question on individuals' sense of duty. Researchers could thus use those datasets to provide first evidence on whether personal experience yields a stronger effect of corruption on duty (or not), and, in this way, advance knowledge on the relationship between corruption and the duty to vote.

Fourth, while this dissertation sheds light on the sources of duty with a focus on civic education, compulsory voting, and corruption, many other sources are worth investigating. Researchers could explore, for instance, how citizens' views about democratic functioning affect their sense of duty. Webb (2013, 767) argues that "there are two quite different types of attitudes prevalent

among citizens who are disaffected with politics: the ‘dissatisfied democratic’ and ‘stealth democratic’ orientations”. These two orientations affect voting in different ways. Dissatisfied democrats are likely to engage in all forms of political participation (including voting), while stealth democrats are less willing to act politically. Atkinson et al. (2016) provide similar results with Canadian data. They show that “on the stealth democracy scale itself, the non-voter mean (2.70) is significantly higher than the voter mean (2.50)” (Atkinson et al. 2016, 67). Given that duty and democratic preferences are both linked to the belief in the importance of voting (Gallego and Oberski 2012; Webb 2013), views about democratic functioning might affect sense of duty and, in doing so, affect individuals’ propensity to vote. Researchers could examine this possibility by fielding original surveys with questions on democratic attitudes and the duty to vote. Those preferences could be assessed by Hibbing and Theiss-Morse’s (2002) classic measures, which tap individuals’ opinion about experts in government, for example. And duty could be measured by a duty/choice question.

Another source that is worth investigating is the process of democratization and democratic consolidation. A wealth of evidence suggests that turnout reaches a peak shortly after the transition to a democratic regime of government but that it declines soon after. For instance, Kostadinova (2003, 754) examines the turnout trends in fifteen East European countries and reveals that “the first parliamentary elections had the highest turnout compared to ensuing contests”. Yet, turnout “drops by more than 8 per cent for the second election, and then further drops by around 6 per cent for the third and another 5 per cent for the fourth” (Kostadinova 2003: 754). Fornos, Power, and Garand (2004) find a similar pattern among Latin-American democracies. They show that “in elections representing either redemocratization or the initiation of democracy, citizens respond with turnout rates that are approximately 16% higher than in other elections”, an enthusiasm for voting that vanishes rapidly in subsequent elections

(Fornos, Power, and Garand 2004: 925) And although Kostelka (2017) reveals that in 34% of the 91 democratic consolidations that took place between 1939 and 2015 there is an augmentation of electoral participation (not a decline), still in the majority of cases electoral participation decays at varying levels. Building on this literature, and the established connection between democratic level and duty (Galais and Blais 2017), researchers could investigate the impact of democratization and democratic consolidation on duty. They could do so by examining the relationship between the number of years since a country's first democratic election – Kostelka's dataset would be useful in this respect – and individuals' sense of duty with comparative data like the Latinobarometer, and the 2004 and 2014 Citizenship Modules of the International Social Survey Programme. This analysis would not tell, however, about the impact of democratization on duty. For this, researchers could leverage a case of democratic transition and collect dataset both before and after the first democratic election, which could then be analyzed through a synthetic control model.

Fifth, I believe that we would be able to understand in more depth the relationship between civic education, compulsory voting, and corruption on duty if we examined how much these factors affect other types of duty like the duty to be informed (Blais et al. 2019) and the duty to contribute to political campaigns (Weinschenk and Panagopoulos 2020). With this research, we would learn if other types of duty are equally affected by civic education, compulsory voting, and corruption, or if there is something specific about the duty to vote that makes it susceptible to civic education and compulsory voting, but not to corruption.

Prior research suggests that the sources of the duty to vote might not be the same as the sources of the duty to be informed and the duty to contribute to political campaigns. Regarding the duty to be informed, Blais et al. (2019) show that 30 percent of respondents in a Canadian survey

report a duty to vote but not to be informed. And 6 percent of the respondents report that voting is not a duty but that there is a duty to be informed. When it comes to the duty to contribute to political campaigns, Weinschenk and Panagopoulos (2020, 8) reveal that “most of the factors that predict the sense of duty to vote (e.g., age, education, sex) are inversely related to views about the obligation to contribute to campaign”, and that duty to vote is weakly correlated with duty to contribute. The lack of data on those duty sources and types (to vote, to be informed about politics and to contribute to political campaigns) impedes the present realization of this research, and it calls for surveys that include measures of all relevant variables.

In conclusion, this dissertation advances important knowledge on the duty to vote. It shows that duty is a very stable attitude. And it shows that civic education and compulsory voting can both affect how dutiful a person feels. A key message from this dissertation is that there is little rationalization in duty. In addition, duty likely precedes turnout, thus representing a potential explanation of the voting paradox. Another important message from this dissertation is that civic education helps to foster a dutiful citizenry. And that compulsory voting rules are not incompatible with duty; rather, they contribute to the development of this political attitude. I hope that this dissertation is useful for students of the duty to vote and for those from the broader research community who use duty in their research.

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Appendix A (Chapter 1)

Appendix A.1. Fieldwork dates of the 2005-2010 British Election Panel Study (BEPS) and the 2010-2017 Spanish Democracy, Elections and Citizenship (DEC) Panel Study

	2005-2010 BEPS	2010-2017 DEC
Wave 1	March/April 2005*	November/December 2010
Wave 2	April/May 2005*	May 2011*
Wave 3	May 2005*	November 2011*
Wave 4	May 2006	May 2012*
Wave 5	June 2008	May/June 2013
Wave 6	July 2009	May 2014
Wave 7	March/April 2010*	May 2015*
Wave 8	April/May 2010*	May 2016*
Wave 9	May 2010*	May/June 2017*

Notes: the BEPS and the DEC have measured respondents' sense of civic duty to vote in all the above waves. Waves that correspond to pre-, campaign, or post-election studies are marked with an asterisk (*).

Appendix A.2. Goodman–Kruskal gamma-type rank correlations of civic duty measures from adjacent BEPS and DEC panel waves

	2005-2010 BEPS: 9-wave Panelists	2005-2010 BEPS: All Panelists	2010-2017 DEC: 9-wave Panelists	2010-2017 DEC: All Panelists
W1-W2	.86	.85	.77	.76
W2-W3	.92	.90	.86	.78
W3-W4	.84	.82	.89	.84
W4-W5	.83	.82	.83	.81
W5-W6	.86	.84	.82	.80
W6-W7	.86	.85	.85	.78
W7-W8	.91	.91	.84	.81
W8-W9	.95	.94	.89	.85
N	1,524	-	204	-
N (W1-W2)	-	5,909	-	1,813
N (W2-W3)	-	4,803	-	1,979
N (W3-W4)	-	4,896	-	1,717
N (W4-W5)	-	4,426	-	1,375
N (W5-W6)	-	3,704	-	1,002
N (W6-W7)	-	2,945	-	726
N (W7-W8)	-	2,980	-	740
N (W8-W9)	-	2,581	-	530

Notes: 0 means no correlation; -1, negative and perfect correlation; +1, positive and perfect correlation.

Appendix A.3. Civic duty stability as indicated by stability coefficients from baseline and alternative structural equation models (without and with covariance between error terms), with only BEPS respondents who voted in the 2005 election and abstained in the 2010 election, or vice-versa

2005-2010 BEPS (Waves 1-9)		
	Baseline model (no covariance between error terms)	Alternative model (covariance between error terms)
W1-W2	.99	.98
W2-W3	.95	.99
W3-W4	.91	.98
W4-W5	.89	.87
W5-W6	.98	.96
W6-W7	1.00	1.00
W7-W8	.97	.93
W8-W9	.83	.77
Var ϵ_1	173.27	212.64
Var ϵ_2	136.41	226.73
Var ϵ_3	219.12	196.39
Var ϵ_4	276.06	196.31
Var ϵ_5	269.73	225.03
Var ϵ_6	191.48	122.07
Var ϵ_7	231.65	90.34
Var ϵ_8	100.97	147.69
Var ϵ_9	272.89	396.22
Santorra-Bentler adjusted χ^2	39.76	13.00
df	19	11
<i>p</i> -value	.004	.29
CFI	.98	1.00
RMSEA	.09	.04
[90% c.i.]	.06;.12	.00;.09
N	131	

Notes: stability coefficients are unstandardized. Values close to zero indicates instability; values close to one, stability.

Appendix A.4. Civic duty stability as indicated by stability coefficients from baseline and alternative structural equation models (without and with covariance between error terms), with DEC waves 1-8

	2010-2017 DEC (Waves 1-8)	
	Baseline model (no covariance between error terms)	Alternative model (covariance between error terms)
W1-W2	1.00	.99
W2-W3	.88	.93
W3-W4	1.00	1.00
W4-W5	.96	1.00
W5-W6	.92	.94
W6-W7	.98	.99
W7-W8	.97	.94
Var ϵ_1	604.23	687.26
Var ϵ_2	538.97	659.35
Var ϵ_3	558.12	639.81
Var ϵ_4	517.19	604.01
Var ϵ_5	547.32	565.63
Var ϵ_6	505.32	453.57
Var ϵ_7	508.43	506.58
Var ϵ_8	626.94	702.23
Santorra-Bentler adjusted χ^2	13.95	5.73
df	13	6
<i>p</i> -value	.38	.45
CFI	1.00	1.00
RMSEA	.02	.00
[90% c.i.]	.00;.07	.00;.11
N	376	

Notes: stability coefficients are unstandardized. Values close to zero indicates instability; values close to one, stability.

Appendix B (Chapter 2)

Appendix B.1. Number of respondents and schools per country

Country	Number of schools	Number of respondents
Belgium (Flanders)	162	2,702
Bulgaria	147	2,935
Chile	178	4,986
Colombia	150	5,575
Denmark	185	5,985
Germany (North Rhine-Westphalia)	64	1,387
Dominican Republic	141	3,882
Estonia	164	2,801
Finland	179	3,062
Hong-Kong	128	2,173
Croatia	177	3,799
Italy	170	3,203
South Korea	106	2,593
Lithuania	183	3,562
Latvia	148	3,174
Mexico	213	5,330
Malta	47	3,474
Netherlands	123	2,705
Norway	148	6,271
Peru	206	5,110
Russia	352	7,080
Slovenia	145	2,738
Sweden	155	2,900
Total	3,671	86,914

Appendix B.2. Descriptive statistics of variables in the analyses

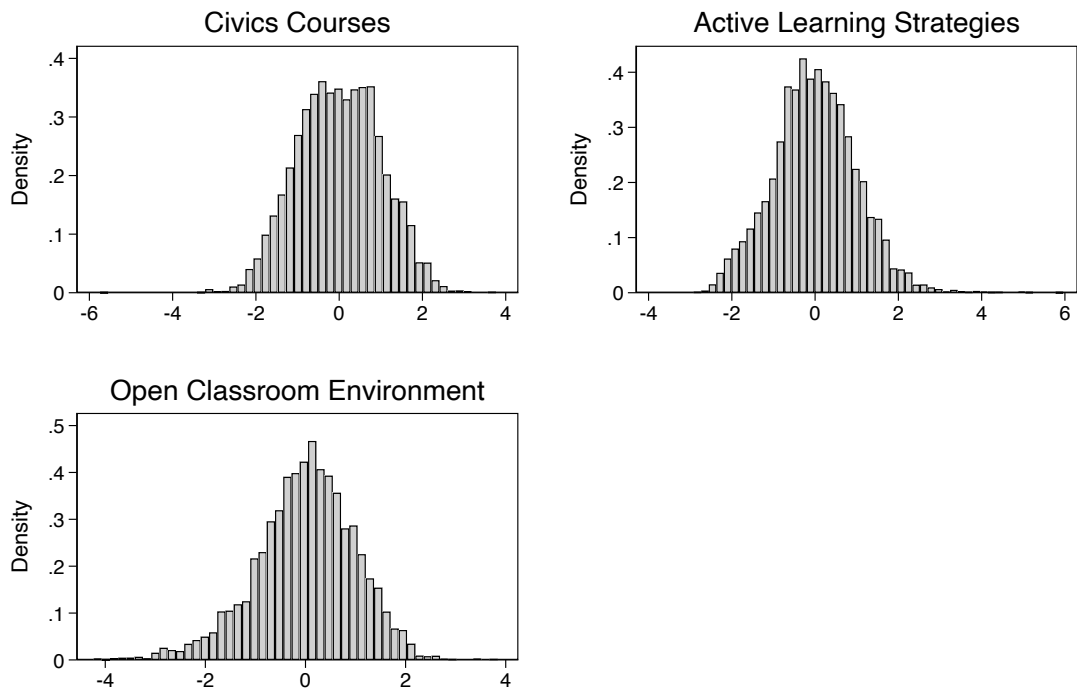
	Mean	Standard Deviation	Minimum Value	Maximum Value	N
Civic Duty (0=not important at all or not very important; 1=quite important or very important)	0.82	0.38	0.00	1.00	85,005
Civics Courses (Standardized values. Higher values indicate higher levels civic education through civics courses)	0.00	1.00	-5.74	3.77	86,818
Active Learning Strategies (Standardized values. Higher values indicate higher levels of civic education through active learning strategies)	0.00	1.00	-2.92	5.96	86,816
Open Classroom Environment (Standardized values. Higher values indicate higher levels of civic education through open classroom environment)	0.00	1.00	-4.23	3.84	86,812
Political Interest: Parents (0=not interested or a little interested; 1= somewhat interested or very interested)	0.27	0.44	0.00	1.00	84,569
Occupational status: Parents (Higher values indicate higher occupational status. Classification based on Ganzeboom et al. [1992])	47.88	17.10	10.00	89.00	82,241
Gender (0=male; 1= female)	0.50	0.50	0.00	1.00	86,828

Notes: unweighted pooled data from the 2016 International Civic and Citizenship Education Study (ICCS).

Appendix B.3. Factor loading and uniqueness value of items forming the three civic education indices

	Factor Loading	Uniqueness Value
<i>Civics Courses:</i>		
<i>(At school, to what extent have you learned about the following topics? Not at all; to a small extent; to a moderate extent; to a large extent)</i>		
- How citizens can vote in local or national elections	0.7028	0.5060
- How laws are introduced and changed	0.7391	0.4538
- How to protect the environment	0.5691	0.6761
- How to contribute to solving problems in the local community	0.7278	0.4703
- How citizen rights are protected	0.7747	0.3999
- Political issues and events in other countries	0.6482	0.5798
- How the economy works	0.6557	0.5700
<i>Active Learning Strategies:</i>		
<i>(At school, have you ever done any of the following activities? No, I have never done this; yes, I have done this but more than a year ago; yes, I have done this within the last twelve months)</i>		
- Active participation in an organized debate	0.5684	0.6769
- Voting for class representative or school parliament	0.5739	0.6706
- Taking part in decision-making about how the school is run	0.7096	0.4965
- Taking part in discussions at a student assembly	0.7095	0.4966
- Becoming a candidate for class representative or school parliament	0.6463	0.5823
- Participating in an activity to make the school more environmentally friendly	0.5305	0.7185
<i>Openness Classroom Environment:</i>		
<i>(When discussing political or social issues during regular lessons, how often do the following things happen? Never; rarely; sometimes; often)</i>		
- Teachers encourage students to make up their own minds	0.7115	0.4937
- Teachers encourage students to express their opinions	0.7510	0.4360
- Students bring up current political events for discussion in class	0.5978	0.6426
- Students express opinions in class even when their opinions are different from most of the other students	0.6760	0.5430
- Teachers encourage students to discuss the issues with people having different opinions	0.7314	0.4651
- Teachers present several sides of the issues when explaining them in class	0.7075	0.4994

Appendix B.4. Distribution of the three civic education indices



Notes: for a list of items, see Appendix 3. All indices are standardized to a mean of 0, and a standard deviation of 1, for comparative purposes. They are also aggregated at the school-level to reduce the risk of over- (or under-) estimations of civic education exposure, and of reverse causation.

Appendix B.5. Effect of different forms of civic education on sense of civic duty to vote, with interactions of these different forms of civic education

	DV: Civic Duty		
	Model 1	Model 2	Model 3
Civics Courses	0.222*** (0.020)	0.221*** (0.020)	0.222*** (0.020)
Active Learning Strategies	0.090*** (0.017)	0.090*** (0.017)	0.089*** (0.017)
Open Classroom Environment	0.139*** (0.017)	0.139*** (0.017)	0.142*** (0.017)
Civics Courses* Active Learning Strategies	0.010 (0.012)		
Active Learning Strategies* Open Classroom Environment		-0.011 (0.012)	
Active Learning Strategies* Open Classroom Environment			0.025* (0.012)
Political Interest: Parents	0.602*** (0.025)	0.602*** (0.025)	0.602*** (0.025)
Occupational Status: Parents	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
Female	0.124*** (0.020)	0.123*** (0.020)	0.123*** (0.020)
Constant	1.579*** (0.074)	1.589*** (0.074)	1.580*** (0.074)
Fixed Effects: Country	Yes	Yes	Yes
N	79,885	79,885	79,885

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within schools. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. Higher values of the civic education indices indicate higher levels of a form of civic education. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix B.6. Effect of democracy level on civic duty

DV: Civic Duty	
Not Free	0.398 (0.646)
Partly Free	0.455 (0.575)
Constant	1.156* (0.559)
N	
	82,916

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within countries. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. “Free” represents the category of reference for my democracy measure, that comes from Freedom House. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix B.7. Effect of civic duty on vote intention, moderated by democracy level

DV: Intention to Vote	
Civic duty	1.674*** (0.076)
Civic Duty *Not Free	0.160 (0.096)
Civic Duty *Partly Free	-0.101 (0.081)
Not Free	-0.222 (0.476)
Partly Free	-0.248 (0.423)
Constant	0.910* (0.411)
N	80,540

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within countries. Intention to vote is dichotomous: 0=I would certainly/probably not do this; 1=I would certainly/probably do this. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. "Free" represents the category of reference for my democracy measure, that comes from Freedom House. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix B.8. Effect of different forms of civic education on sense of civic duty to vote, with at least 26 (the median number of) students per school

	DV: Civic Duty			
	(1)	(2)	(3)	(4)
Civics Courses	0.344*** (0.028)			0.214*** (0.033)
Active Learning Strategies		0.209*** (0.028)		0.073* (0.028)
Open Classroom Environment			0.293*** (0.024)	0.174*** (0.028)
Political Interest: Parents	0.606*** (0.036)	0.608*** (0.036)	0.605*** (0.036)	0.599*** (0.036)
Occupational Status: Parents	0.007*** (0.001)	0.007*** (0.001)	0.006*** (0.001)	0.006*** (0.001)
Female	0.177*** (0.029)	0.173*** (0.029)	0.163*** (0.029)	0.165*** (0.029)
Constant	1.771*** (0.302)	1.790*** (0.316)	1.570*** (0.305)	1.812*** (0.300)
Fixed Effects: Country	Yes	Yes	Yes	Yes
N	40,703	40,703	40,703	40,703

Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within schools. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. Higher values of the civic education indices indicate higher levels of a form of civic education. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix B.9. number of respondents per province/territory

Province/Territory	Number of Respondents
Newfoundland	82
New Brunswick	135
Nova Scotia	206
Prince Edward Island	26
Quebec	785
Ontario	1272
Manitoba	234
Saskatchewan	153
Alberta	667
British Columbia	781
Nunavut	13
Northwest Territories	27
Yukon	28
Total	4,409

Notes: data from the 2011 and 2015 Canadian National Youth Survey (NYS). Provinces are ordered as in the 2011 and 2015 NYS.

Appendix B.10. Descriptive statistics of variables in the NYS analyses

	Mean	Standard Deviation	Minimum Value	Maximum Value	N
Civic Duty (0=disagree or strongly disagree that voting is a civic duty/voting is a choice; 1= agree or strongly agree that voting is a civic duty/voting is a civic duty)	0.73	0.44	0.00	1.00	4,259
Experience with Civics Courses (0=no; 1=yes)	0.66	0.47	0.00	1.00	4,236
Discussion: Parents (0=not often; 1=often)	0.22	0.41	0.00	1.00	4,283
Gender (0=male; 1=female)	0.55	0.50	0.00	1.00	4,398
Age	26.02	5.04	18.00	35.00	4,416
Education (0=less than tertiary; 1=tertiary)	0.68	0.47	0.00	1.00	4,376
Aboriginal Identity (0=no; 1=yes)	0.09	0.28	0.00	1.00	4,385

Notes: unweighted pooled data from the 2011 and 2015 Canadian National Youth Survey.

Appendix B.11. Effect of different forms of civic education on sense of civic duty to vote, with interactions of these different forms of civic education and parents' political interest and occupational status

	DV: Civic Duty					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Civics Courses	0.221*** (0.021)	0.220*** (0.020)	0.220*** (0.020)	0.272*** (0.036)	0.220*** (0.020)	0.220*** (0.020)
Active Learning Strategies	0.100*** (0.017)	0.108*** (0.018)	0.100*** (0.017)	0.099*** (0.017)	0.126*** (0.033)	0.100*** (0.017)
Open Classroom Environment	0.135*** (0.018)	0.135*** (0.018)	0.136*** (0.019)	0.135*** (0.018)	0.135*** (0.018)	0.116** (0.035)
Civics Courses* Political Interest: Parents	-0.007 (0.026)					
Active Learning Strategies* Political Interest: Parents		-0.037 (0.026)				
Open Classroom Environment* Political Interest: Parents			-0.006 (0.027)			
Civics Courses* Occupational Status: Parents				-0.001 (0.001)		
Active Learning Strategies* Occupational Status: Parents					-0.001 (0.001)	
Open Classroom Environment* Occupational Status: Parents						0.000 (0.001)
Political Interest: Parents	0.605*** (0.026)	0.603*** (0.026)	0.605*** (0.026)	0.605*** (0.026)	0.605*** (0.026)	0.605*** (0.026)
Occupational Status: Parents	0.006*** (0.001)	0.006*** (0.001)	0.006*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.006*** (0.001)
Female	0.124*** (0.020)	0.124*** (0.020)	0.124*** (0.020)	0.124*** (0.020)	0.124*** (0.020)	0.124*** (0.020)
Constant	1.598*** (0.075)	1.600*** (0.075)	1.597*** (0.074)	1.602*** (0.074)	1.600*** (0.075)	1.594*** (0.075)
Fixed Effects: Country	Yes	Yes	Yes	Yes	Yes	Yes
N	77982	77982	77982	77982	77982	77982

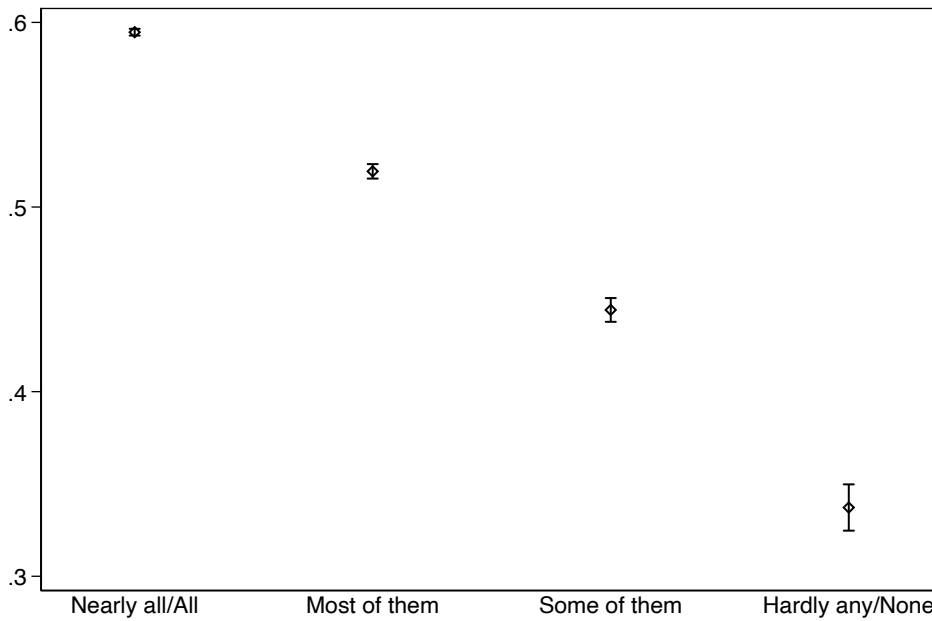
Notes: entries report log-odds and clustered-standard errors (in parentheses). Effects are estimated by means of multi-level logistic regressions, in which students are nested within schools. Civic duty is dichotomous: 0=voting is not important at all or not very important; 1=voting is quite important or very important. Higher values of the civic education indices indicate higher levels of a form of civic education. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix B.12. Mean levels of civic duty by school type

School Type	Mean Levels of Civic Duty
<i>Type I:</i> All three forms of civic education	.89
<i>Type II:</i> Civics courses and active learning strategies	.82
<i>Type III:</i> Civics courses and open classroom environment	.87
<i>Type IV (baseline):</i> Civics courses only	.80

Notes: 22, 11, 10, and 7 percent of the ICCS schools fall in each school type, respectively. The rest (50 percent) of the ICCS schools does not implement civics courses as a form of civic education.

Appendix B.13. Correlation between students' and directors' assessments of voting in school



Notes: students' measure of vote for class representative or school parliament comes from the 2016 ICCS student sample. Directors' measure of vote in school parliament elections comes from the 2016 ICCS director sample. To allow a student-director comparison, the students' measure is dichotomized before the school-level aggregation: 0=I have done this but more than a year ago or no, I have never done this; 1=Yes, I have done this within the last twelve months. Fewer students reports voting as one moves from "nearly all/all" to "most of them", and so on, denoting a consistency between students' and directors' measures.

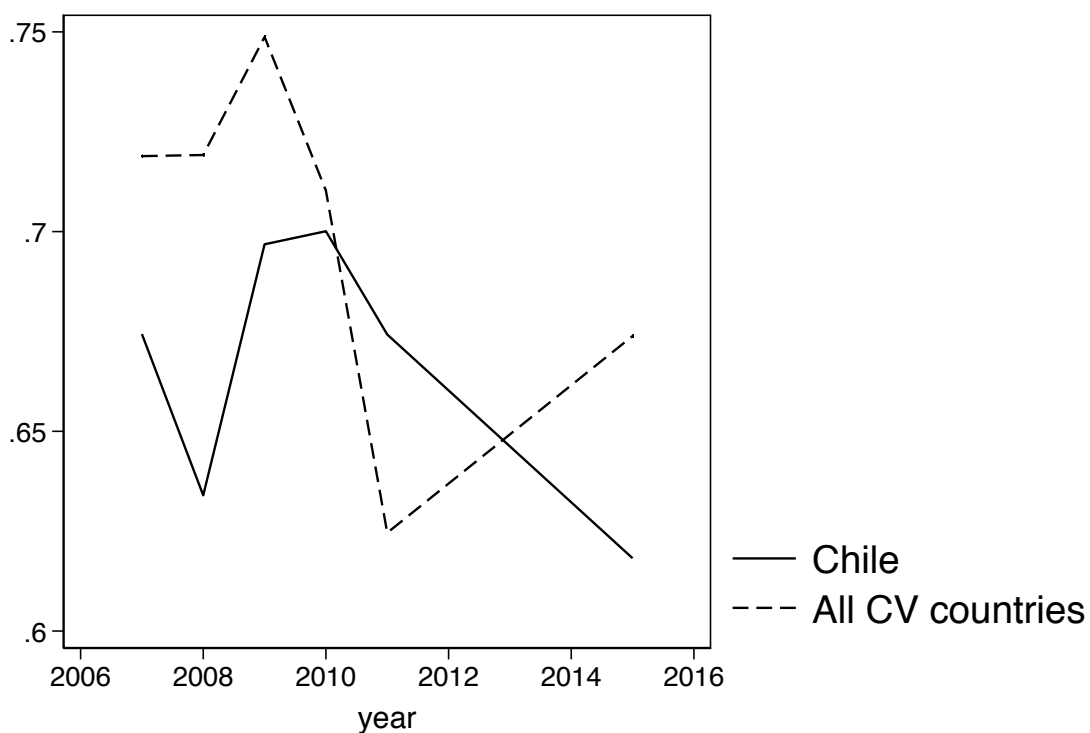
Appendix C (Chapter 3)

Appendix C.1. Voting rules from the 17 Latin-American countries in our sample

Country	Compulsory voting	Enforcement & Severity
Argentina	Yes	1
Bolivia	Yes	2
Brazil	Yes	2
Chile	Yes (until 2012)	1
Colombia	No	-
Costa Rica	Yes	1
Dominican Republic	Yes	1
Ecuador	Yes	2
El Salvador	No	-
Guatemala	No	-
Honduras	Yes	1
Mexico	Yes	1
Nicaragua	No	-
Panama	Yes	1
Paraguay	Yes	1
Peru	Yes	3
Uruguay	Yes	3

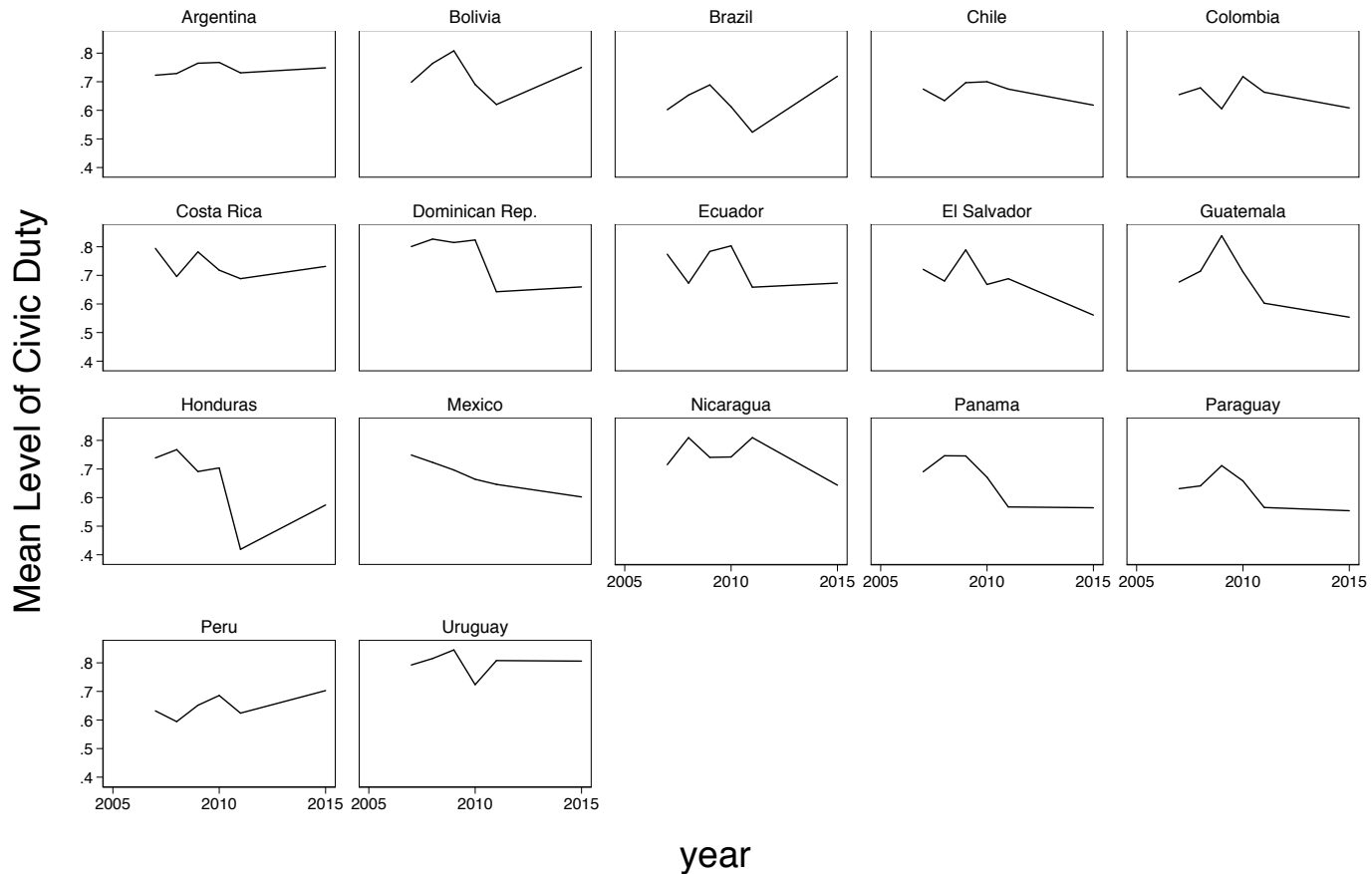
Notes: Data come from the 2018 Varieties of Democracy Project. 3 means “sanctions are enforced, and impose considerable costs”; 2, “sanctions are enforced, but they impose minimal costs”; 1, “there are no sanctions or they are not enforced”. We exclude Venezuela as it also abolished compulsory voting rules, in 1993. Previous work has made use of this classification of voting systems (Singh 2018), as well as the same trichotomous categorization of compulsory voting systems (Panagopoulos 2008).

Appendix C.2. Civic duty trends in Chile and in all CV countries (except Chile)



Notes: The trend in all Latin-American compulsory voting systems (except Chile) differs from that in Chile before the 2012 intervention. Particularly, we observe a decline of civic duty right before Chile's electoral reform among all other countries and, consequently, an important dissonance of the duty trends. See Appendix 3 for the civic duty trends in each Latin-American country, and Appendix 4 for a formal test of the parallel trend assumption.

Appendix C.3. Mean levels of civic duty in Latin-American countries



Notes: Values correspond to the aggregate civic duty mean levels, by country. In most countries where voting is mandatory, civic duty increased from 2011 to 2015.

Appendix C.4. Formal tests of the parallel trend assumption

	DV: Civic Duty			
	Model 1	Model 2	Model 3	Model 4
Chile	-51.458** (18.815)	-0.042* (0.019)	-42.935* (18.215)	-0.041 (0.021)
Year	-0.019* (0.008)		-0.015* (0.007)	
Chile × Year	0.026** (0.009)		0.021* (0.009)	
2008		-0.001 (0.027)		-0.003 (0.029)
2009		0.031 (0.026)		0.037 (0.027)
2010		-0.008 (0.026)		-0.005 (0.028)
2011		-0.092** (0.033)		-0.073* (0.031)
Chile × 2008		-0.040 (0.027)		-0.037 (0.029)
Chile × 2009		-0.008 (0.026)		-0.015 (0.027)
Chile × 2010		0.033 (0.026)		0.031 (0.028)
Chile × 2011		0.092* (0.033)		0.073 (0.031)
Constant	39.034* (15.282)	0.717*** (0.019)	30.511* (14.531)	0.715*** (0.021)
N	66,217	66,217	61,524	61,524

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Clustered standard errors (by country-year) in parentheses. Only compulsory voting systems are analyzed, including Chile. In models 1 and 3, year is taken as a continuous variable. In models 2 and 4, it is taken as a categorical variable. Following Coppock (2015), we correct for multiple comparisons by means of the Holm's approach. For the sake of parsimony, the original and adjusted p-values of the interaction terms in models 2 and 4 are not printed in the table. To test whether Honduras – where civic duty declined to below 50 percent in 2011 – drives the results, we excluded it in additional analyses. They correspond to models 2 to 4. Three of the four models fail to confirm the parallel trend assumption.

Appendix C.5. Descriptive statistics of all variables in the synthetic control estimations (2007-2011 period)

	Mean	Standard deviation	Minimum	Maximum	N
Civic duty*	0.71	0.08	0.42	0.85	85
Voting system	0.76	0.43	0.00	1.00	85
Religion*	2.51	0.23	1.84	2.91	85
Education	7.42	1.32	5.01	10.35	85
Age	0.42	0.05	0.31	0.51	85
Support for democracy*	2.41	0.18	2.04	2.78	85
Trust in parliament*	2.09	0.25	1.45	2.78	85
Democratization level	2.56	0.50	2.00	3.00	85

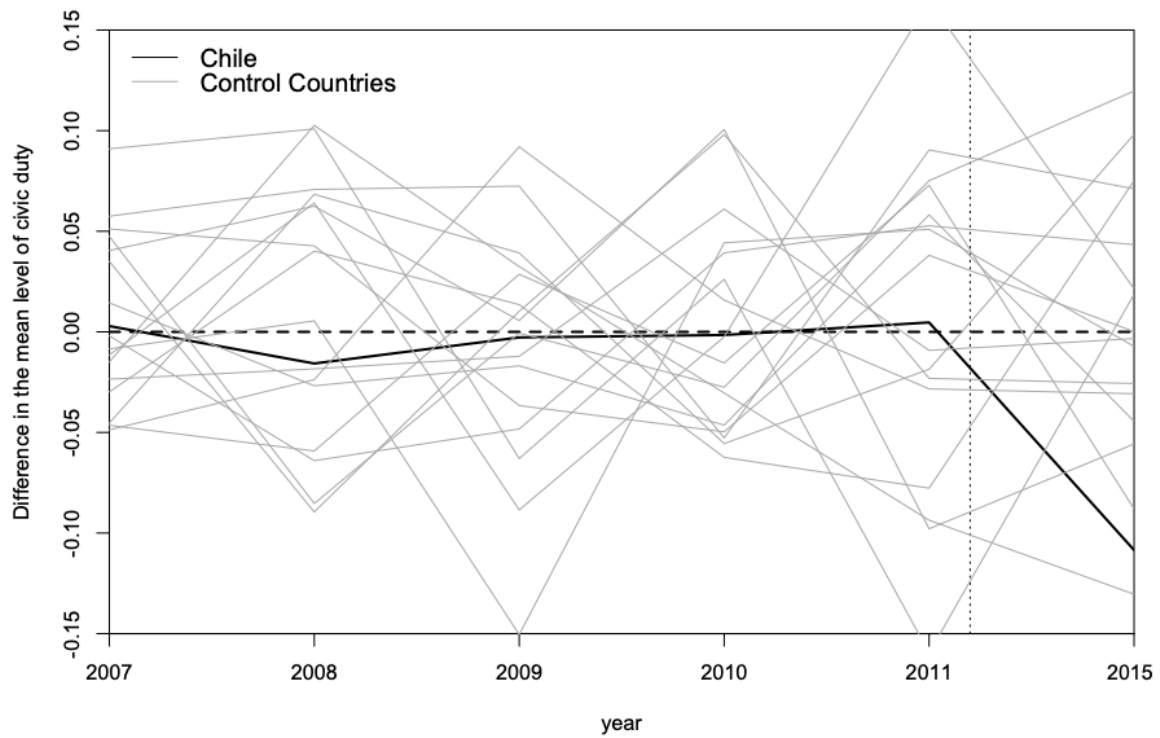
Notes: Variables marked with an asterisk (*) correspond to country-level aggregations of individual-level variables. At that level, 1= “duty”, 0= “choice”. 4=“very devout”, 3=“devout”, 2=“not very devout”, 1=“not devout at all”. 3=“Democracy is preferable to any other kind of government”, 2=“under some circumstances, an authoritarian government can be preferable to a democratic one”, 1=“For people like me, it doesn’t matter whether we have a democratic or non-democratic regime”. 4=“trust a lot”, 3=“some”, 2=“a little”, 1=“do not trust”. Variables not marked with an asterisk (*) are originally country-level variables. 1= “compulsory”, 0=“voluntary”. 3=“free”, 2=“partly free”, 1= “not free”. Education correspond to average years of formal schooling. Age corresponds to percentage of 19-34 year-old in the voting-age population.

Appendix C.6. Weights assigned to Latin-American countries

Country	Synthetic Control Weight	Country	Synthetic Control Weight
Peru	.69	Honduras	0
Argentina	0	Mexico	0
Bolivia	0	Panama	.02
Brazil	0	Paraguay	0
Costa Rica	0	Uruguay	.29
Dominican Republic	0	Colombia	0
Ecuador	0	El Salvador	0
Nicaragua	0	Guatemala	0

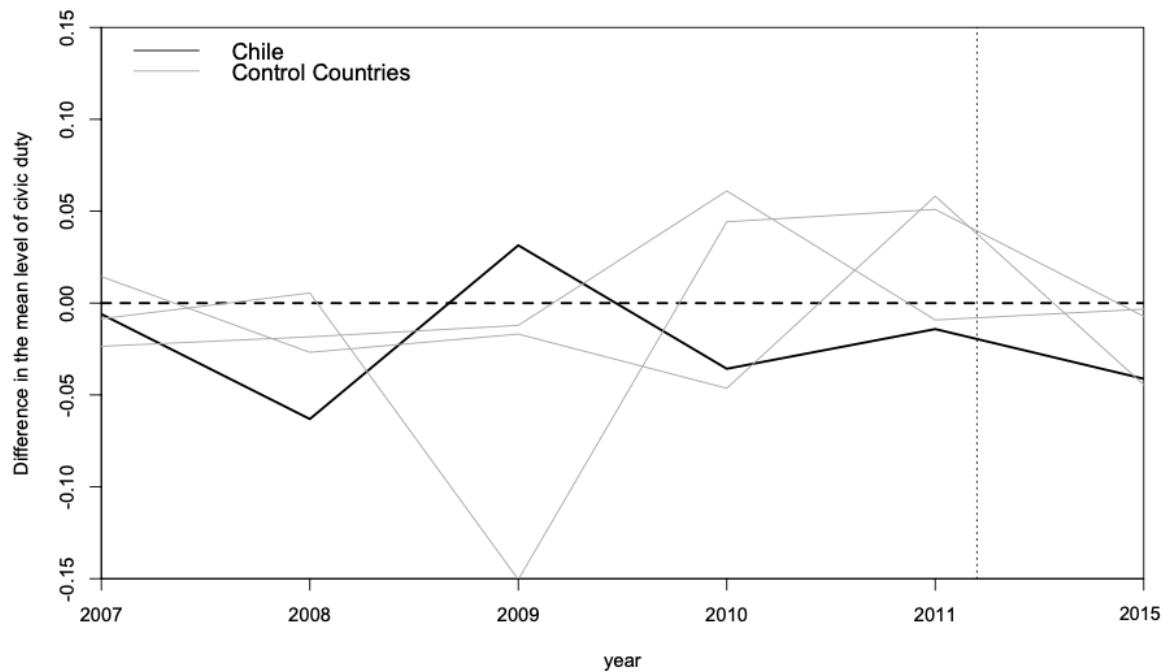
Notes: Weights were assigned based on a country's pre-treatment score on the following variables: civic duty, voting system, religion, education, age, support for democracy, trust in parliament, and democratization level. Higher values mean that a country contributed more to the estimation of synthetic Chile and 0 means that a country did not contribute at all. Consistent with previous studies (Arnold and Stadelmann-Steffen 2017; Fowler 2013), many countries did not contribute to the estimation of "synthetic" Chile.

Appendix C.7. Placebo tests with Chile and 16 Latin-American countries



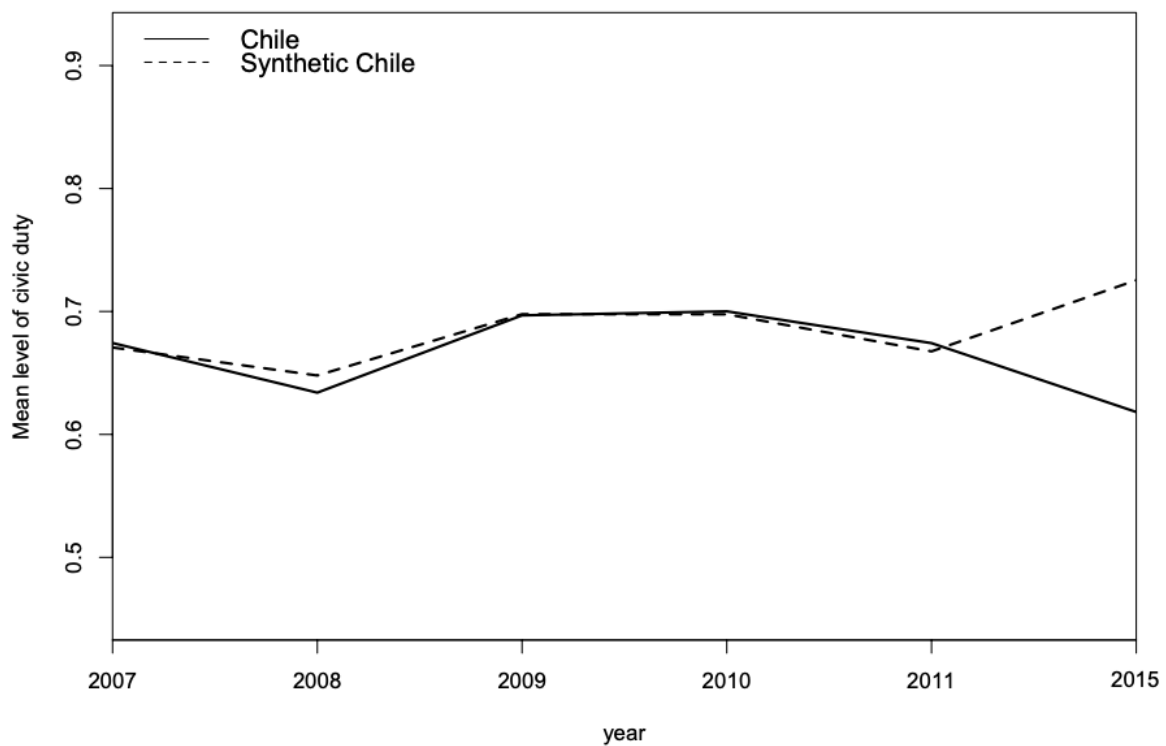
Notes: In these placebo tests, a synthetic control is estimated for Chile and all 16 countries in the donor pool. Values in the figure correspond to the difference between the mean values of civic duty in the treated unit and its synthetic control.

Appendix C.8. Placebo tests with Chile and three countries in the donor pool (Argentina, Colombia and El Salvador)



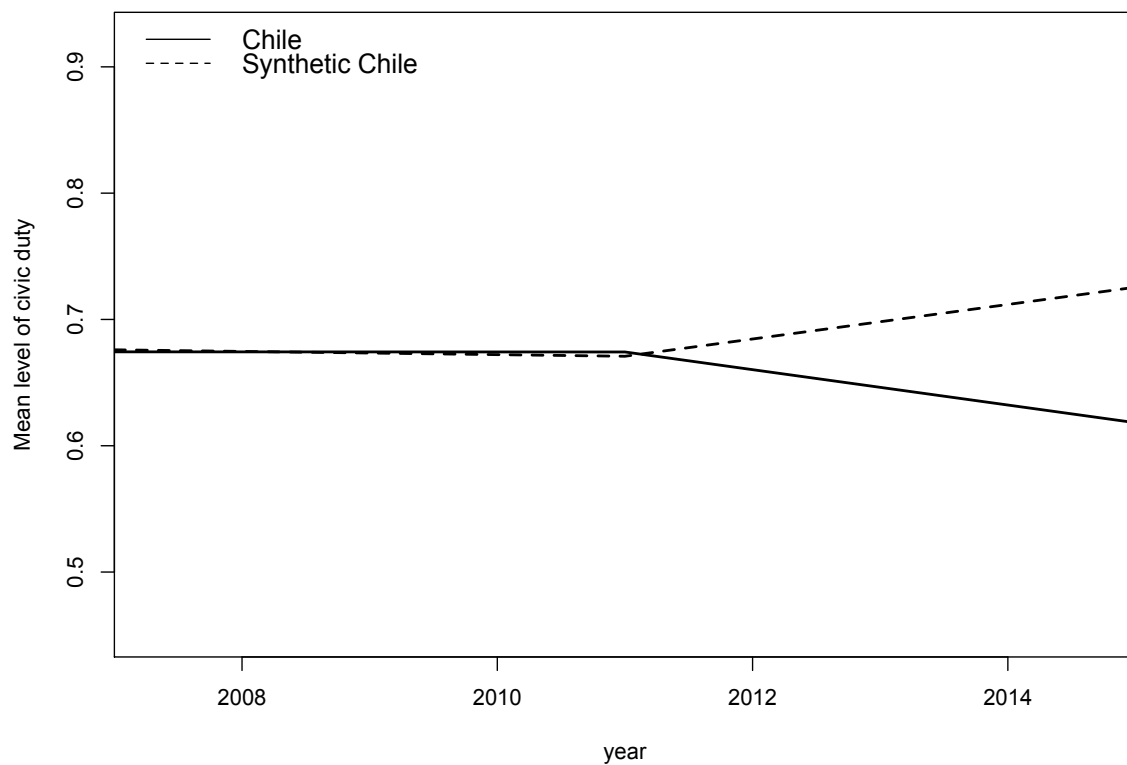
Notes: In these placebo tests, a synthetic control is estimated for Chile and three countries whose difference in the mean levels of civic duty was not twice larger than Chile in appendix 7. They are Argentina, Colombia and El Salvador.

Appendix C.9. Synthetic control with civic duty means in 2007 and 2011, and their growth rate, as suggested by Beckley, Horiuchi and Miller (2017)



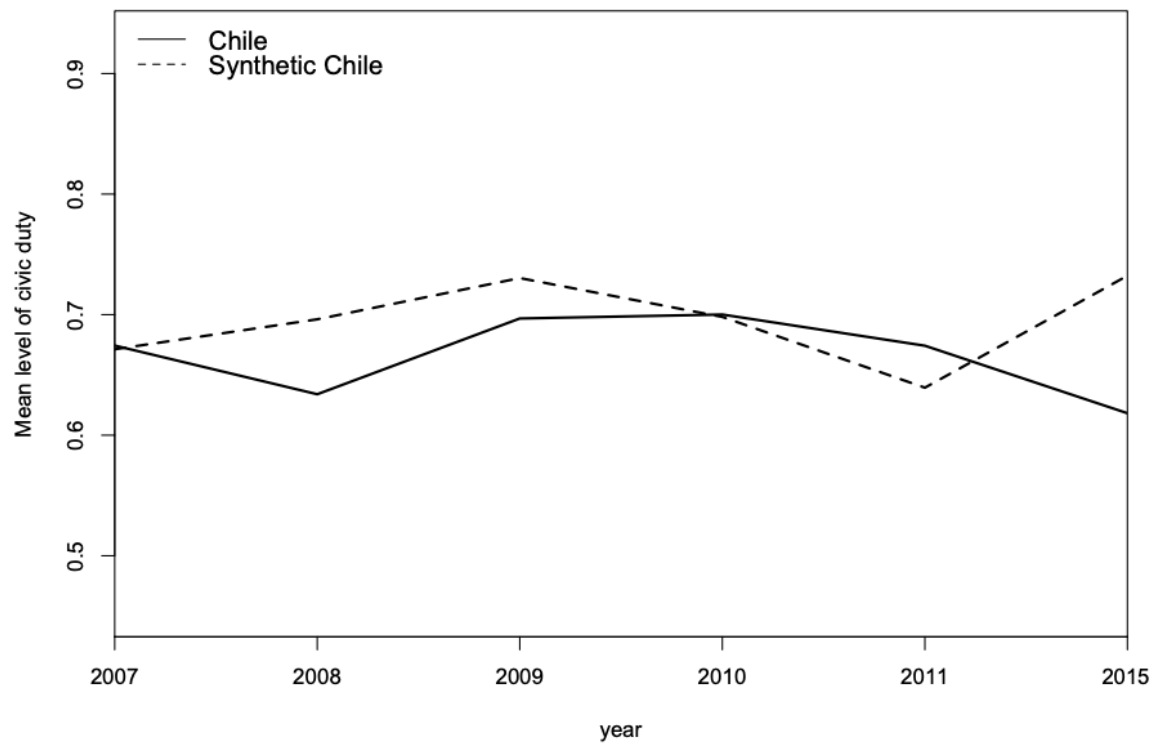
Notes: To further test whether results are the same when accounting for over-fitting, we follow Beckley, Horiuchi and Miller (2018) and substitute the lagged civic duty for its 2007 and 2011 values, and the corresponding growth rate between these years, as well. Results are essentially the same, with a remaining 10 percentage-point difference between “real” Chile and its synthetic control after the abolition of compulsory voting.

Appendix C.10. Analysis with three Latinobarometer studies (from 2007, 2011 and 2015)



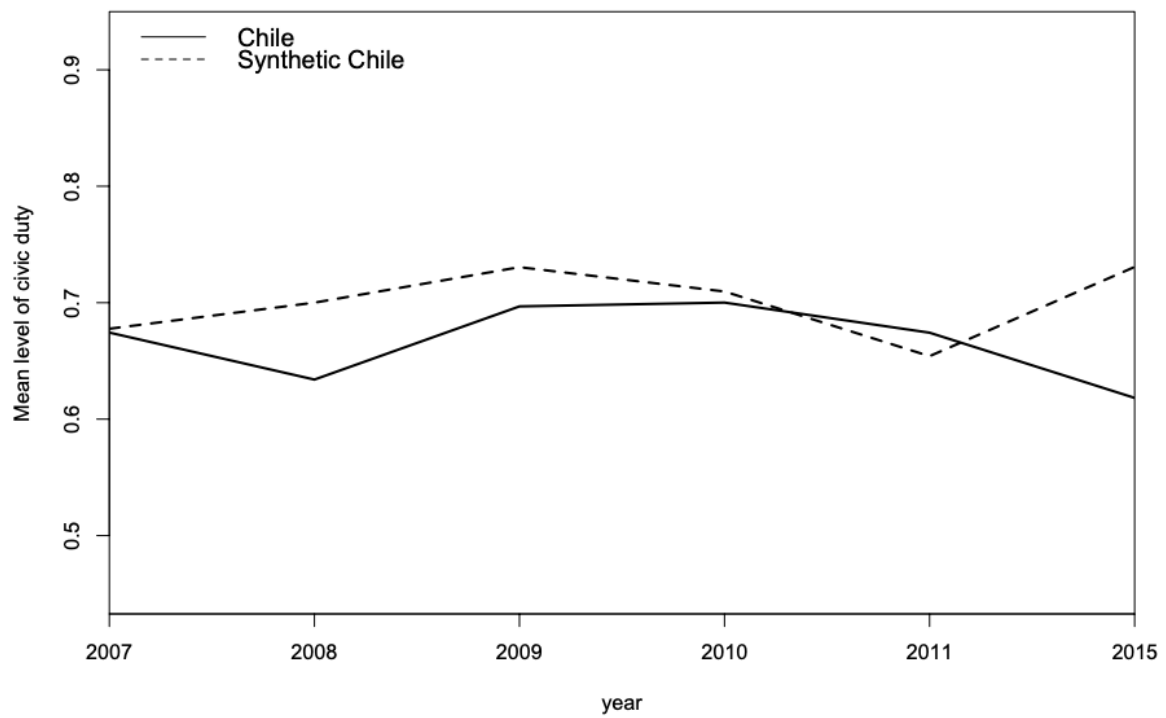
Notes: In this analysis, we use data from only three Latinobarometers (from 2007, 2011 and 2015). Each study is thus separated by a 4-year gap. Results point to a difference between “real” Chile and synthetic Chile after the abolition of compulsory voting of 10 percentage points.

Appendix C.11. Synthetic control without Peru as a potential donor country



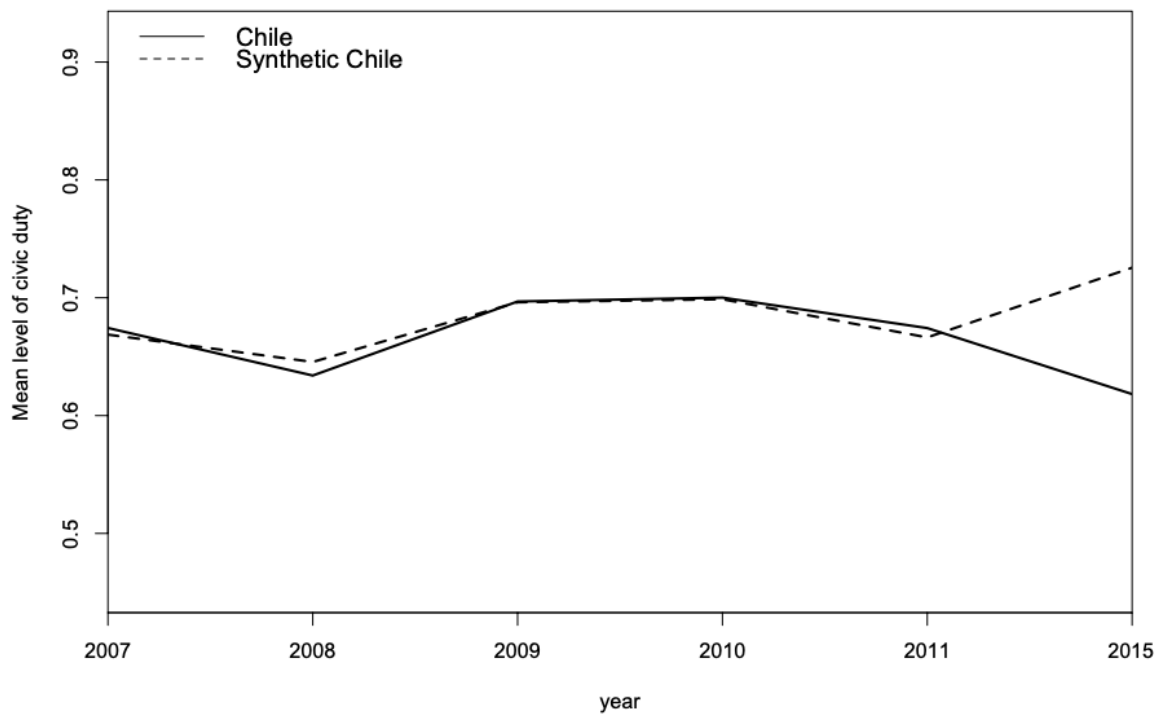
Notes: Before the estimation of the synthetic control model, Peru is excluded from the data and, thus, from the pool of potential donor countries. After this exclusion, the synthetic control is then calculated based on the same set of (8) variables.

Appendix C.12. Synthetic control without Uruguay as a potential donor country



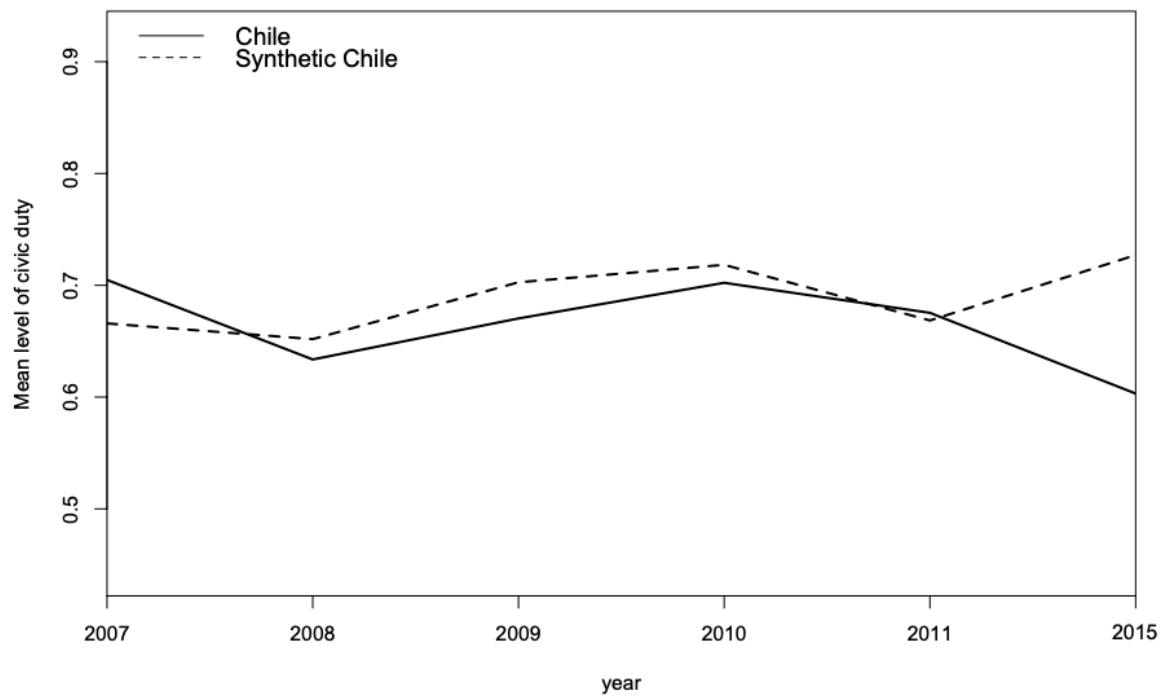
Notes: Before the estimation of the synthetic control model, Uruguay is excluded from the data and, thus, from the pool of potential donor countries. After this exclusion, the synthetic control is then calculated based on the same set of (8) variables.

Appendix C.13. Synthetic control without Panama as a potential donor country



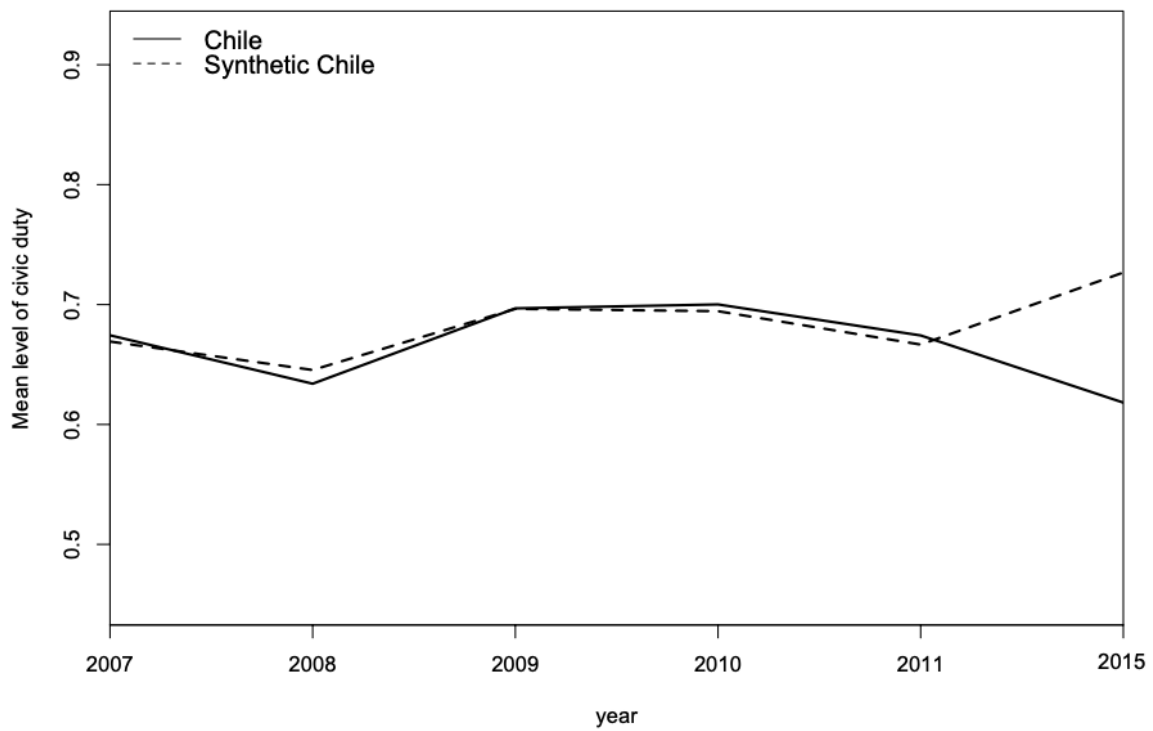
Notes: Before the estimation of the synthetic control model, Panama is excluded from the data and, thus, from the pool of potential donor countries. After this exclusion, the synthetic control is then calculated based on the same set of (8) variables.

Appendix C.14. Synthetic control model with rural respondents (i.e., those living in cities with under 5,000 inhabitants)



Notes: Before the calculation of the mean civic duty levels by country, we exclude respondents who do not live in a city with less than 5,000 inhabitants, which we define as rural area, from the data. We subsequently estimate the synthetic control model with such new data.

Appendix C.15. Synthetic control model with registration rates as a predictor



Notes: To account for the temporal change in registration rates across countries, we added this variable in the estimation of the synthetic control. Such inclusion does not alter the main findings of a negative (10 percentage-point) effect of the abolition of compulsory voting on civic duty.

Appendix C.16. Effect of compulsory voting rules on sense of civic duty to vote, estimated by multi-level models

	Latinobarometer		ISSP			
	Duty: 0-1 Variable		Duty: 1-7 Scale		Duty: 0-1 Variable	
	(1)	(2)	(3)	(4)	(5)	(6)
Compulsory Voting	0.029 (0.065)	-0.048 (0.081)	0.392*** (0.076)	0.376*** (0.080)	0.363*** (0.087)	0.314*** (0.091)
Age		0.004*** (0.001)		0.021*** (0.001)		0.021*** (0.001)
Education		0.051*** (0.007)		0.072*** (0.006)		0.103*** (0.008)
Female		0.057*** (0.016)		0.074*** (0.014)		0.061** (0.019)
Religion		-0.011 (0.010)		0.064*** (0.004)		0.075*** (0.005)
Support for Democracy		0.190*** (0.010)				
Trust in Parliament		0.098*** (0.009)				
Trust in Politicians				0.129*** (0.007)		0.219*** (0.010)
Democratization Level		0.205*** (0.049)		-0.109 (0.100)		0.058 (0.137)
Constant	0.882*** (0.077)	-0.384* (0.157)			1.421*** (0.085)	-0.701 (0.406)
Var (Country)	0.055	0.054	0.262	0.245	0.299	0.276
Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes
Year						
N (countries)	17	17	45	45	45	45
N (respondents)	102169	79335	82623	74214	84146	75253

Notes: *** p<0.01; ** p<0.05; * p<0.1. Values correspond to log-odds. They are calculated by means of (ordered) logistic multi-level regressions of sense of civic duty with pooled Latinobarometer and ISSP data. Individuals are nested within countries (n=17 and 45 in the Latinobarometer and the ISSP data, respectively). Year fixed effects are included but not shown. Computing the average marginal effect from the dichotomized civic duty measure in the ISSP data (column 6), we obtain a 4-percentage-point increase in civic duty with the presence of compulsory voting (results not shown).

Appendix C.17. Effect of compulsory voting rules on sense of civic duty to vote, estimated by country and year fixed effects

	Latinobarometer		ISSP
	Duty: 0-1 Variable	Duty: 1-7 Scale	Duty: 0-1 Variable
Compulsory Voting	-0.022 (0.101)	0.328*** (0.073)	0.263** (0.099)
Age	0.004*** (0.001)	0.021*** (0.002)	0.021*** (0.001)
Education	0.052*** (0.007)	0.072*** (0.016)	0.103*** (0.008)
Female	0.057*** (0.016)	0.075** (0.023)	0.062** (0.019)
Religion	-0.011 (0.010)	0.064*** (0.009)	0.075*** (0.005)
Support for Democracy	0.189*** (0.010)		
Trust in Parliament	0.098*** (0.009)		
Trust in Politicians		0.128*** (0.021)	0.219*** (0.010)
Democratization Level	0.233*** (0.053)	-0.081 (0.076)	0.181 (0.181)
Constant	-0.415* (0.203)		-1.116* (0.563)
Fixed Effects: Country	Yes	Yes	Yes
Fixed Effects: Year	Yes	Yes	Yes
N (countries)	17	45	45
N (respondents)	79335	74214	75253

Notes: *** p<0.01; ** p<0.05; * p<0.1. Values correspond to log-odds. They are calculated by means of (ordered) logistic regressions of sense of civic duty with pooled Latinobarometer and ISSP data, with clustered standard errors (by country). Country and year fixed effects are included but not shown. Computing the average marginal effect with ISSP data we obtain a 4-percentage-point increase in civic duty with the presence of compulsory voting, as in the multi-level regressions (results not shown).

Appendix C.18. Analyses of compulsory voting's effect on civic duty with the difference-in-differences approach

	All CV countries	Chile	Difference between Chile and all CV countries
<i>With 2011 and 2015 Latinobarometers:</i>			
Before abolition of compulsory voting	0.625	0.674	0.049*** (0.015)
After abolition of compulsory voting	0.674	0.618	-0.056*** (0.016)
Difference-in-differences	-	-	-0.105*** (0.022)
<i>With all Latinobarometer Studies:</i>			
Before abolition of compulsory voting	0.703	0.676	-0.027*** (0.007)
After abolition of compulsory voting	0.674	0.618	-0.056*** (0.015)
Difference-in-differences	-	-	-0.029* (0.017)
<i>With all Latinobarometer Studies and Excluding Honduras:</i>			
Before abolition of compulsory voting	0.706	0.676	-0.030*** (0.007)
After abolition of compulsory voting	0.683	0.618	-0.064*** (0.015)
Difference-in-differences	-	-	-0.034** (0.017)

Notes: *** p<0.01; ** p<0.05; * p<0.1. In addition to testing with data from immediately before and after the abolition of compulsory voting, we further tested the impact of compulsory voting on civic duty including all pre-treatment data from Latinobarometers and excluding Honduras as well, where civic duty drops to below .50 in 2011 – right before the treatment. These more conservative tests yield a lower but still negative (and significant) effect.

Appendix D (Chapter 4)

Appendix D.1. Civic duty measures in the MEDW data

Country	Region/ Municipality	Election Level	Duty: National	Duty: Regional	Duty: European	Duty: Municipal
Canada	Ontario	Regional	Yes	Yes	No	Yes
	Quebec	Regional	Yes	Yes	No	Yes
	Ontario	National	Yes	Yes	No	Yes
	British Columbia	National	Yes	Yes	No	Yes
	Quebec	National	Yes	Yes	No	Yes
France	Île-de-France	National	Yes	Yes	Yes	Yes
	Provence	National	Yes	Yes	Yes	Yes
	Provence	European	Yes	No	Yes	Yes
	Île-de-France	European	Yes	No	Yes	Yes
	Paris	Municipal	Yes	No	Yes	Yes
	Marseille	Municipal	Yes	No	Yes	Yes
Germany	Lower Saxony	National	Yes	Yes	Yes	Yes
	Lower Saxony	Regional	Yes	Yes	Yes	Yes
	Bavaria	National	Yes	Yes	Yes	Yes
	Lower Saxony	European	Yes	Yes	Yes	No
Spain	Catalonia	National	Yes	Yes	Yes	No
	Catalonia	Regional	Yes	Yes	Yes	No
	Madrid	National	Yes	Yes	Yes	No
	Catalonia	European	Yes	Yes	Yes	No
	Madrid	European	Yes	Yes	Yes	No
	Madrid	Regional	Yes	Yes	Yes	Yes
Switzerland	Lucerne	National	Yes	Yes	No	Yes
	Lucerne	Regional	Yes	Yes	No	Yes
	Zurich	National	Yes	Yes	No	Yes
	Zurich	Regional	Yes	Yes	No	Yes

Notes: The analyses are conducted with the pooled MEDW data. All civic duty questions were in the pre-election studies.

Appendix D.2. Election studies in the MEDW data

Country	Region/ Municipality	Election Level	Fieldwork Dates (Pre-Election)	Fieldwork Dates (Post-Election)	N
Canada	Ontario	Regional	25/09-05/10/2011	07/10-20/10/2011	1,309
	Quebec	Regional	23/08-03/09/2012	05/09-25/09/2012	990
	Ontario	National	09/10-18/10/2015	20/10-06/11/2015	1,891
	British Columbia	National	09/10-18/10/2015	20/10-05/11/2015	1,869
	Quebec	National	09/10-18/10/2015	20/10-06/11/2015	1,849
France	Île-de-France	National	31/05-09/06/2012	18/06-03/07/2012	966
	Provence	National	31/05-09/06/2012	18/06-03/07/2012	983
	Provence	European	12/05-21/05/2014	26/05-09/06/2014	1,039
	Île-de-France	European	12/05-20/05/2014	26/05-09/06/2014	975
	Paris	Municipal	13/03-22/03/2014	31/03-14/04/2014	1,208
	Marseille	Municipal	13/03-22/03/2014	31/03-14/04/2014	725
Germany	Lower Saxony	National	12/09-19/09/2013	23/09-30/09/2013	975
	Lower Saxony	Regional	11/01-19/01/2013	21/01-04/02/2013	983
	Bavaria	National	16/09-21/09/2013	23/09-28/09/2013	4,684
	Lower Saxony	European	12/05-21/05/2014	26/05-09/06/2014	978
Spain	Catalonia	National	10/11-19/11/2011	21/11-04/12/2011	951
	Catalonia	Regional	16/11-23/11/2012	26/11-10/12/2012	993
	Madrid	National	10/11-19/11/2011	21/11-04/12/2011	976
	Catalonia	European	12/05-20/05/2014	26/05-10/06/2014	985
	Madrid	European	12/05-20/05/2014	26/05-09/06/2014	974
	Madrid	Regional	14/05-20/05/2015	25/05-08/06/2015	921
Switzerland	Lucerne	National	13/10-22/10/2011	25/10-06/11/2011	1,108
	Lucerne	Regional	31/03-09/04/2011	12/04-19/04/2011	1,176
	Zurich	National	13/10-22/10/2011	25/10-06/11/2011	1,057
	Zurich	Regional	24/03-02/04/2011	04/04-09/04/2011	1,192

Appendix D.3. Descriptive statistics of all variables in the analysis

	Mean	Standard Deviation	Minimum Value	Maximum Value	N
Duty: National	0.56	0.50	0	1	30,434
Duty: Regional	0.52	0.50	0	1	26,631
Duty: European	0.41	0.49	0	1	17,841
Duty: Municipal	0.50	0.50	0	1	24,619
Self-Reported Turnout: National	0.90	0.30	0	1	13,574
Self-Reported Turnout: Regional	0.86	0.34	0	1	5,318
Self-Reported Turnout: European	0.78	0.41	0	1	3,626
Self-Reported Turnout: Municipal	0.80	0.40	0	1	1,373
Corruption: National	2.97	0.98	1	4	29,087
Corruption: Regional	2.84	0.95	1	4	28,849
Corruption: European	3.13	0.91	1	4	18,461
Corruption: Municipal	2.38	1.04	1	4	23,562
Political Interest	6.46	2.71	0	10	31,593
Political Trust	1.58	0.70	1	4	30,866
Partisanship	0.43	0.49	0	1	29,717
Female	0.52	0.50	0	1	31,757
Age	46.67	14.69	18	89	31,754
Education	2.09	0.61	1	3	31,747
Religiosity	0.71	0.45	0	1	21,760

Notes: Unweighted pooled MEDW data. All duty items are dichotomous: 0=choice; 1=duty. All self-reported voting items are dichotomous: 0=no; 1=yes. All corruption perceptions measures are categorical: 1=hardly any corruption, 2=a little corruption, 3=some corruption, 4=a lot of corruption. The political interest question read “How much interest do you have in politics generally?”. 0=no interest at all; 10=a lot of interest. The political trust question, “Do you strongly disagree, somewhat disagree, somewhat agree, or strongly agree with the following statements: Politicians make campaign promises they have no intention of keeping?”. 1=Strongly agree; 4=Strongly Disagree. And the partisanship question, “Do you usually think of yourself as close to any particular political party?”. 0=no; 1=yes. Education is categorical: 1=no schooling or primary education, 2=secondary education or technical training, 3= university diploma or a higher level of instruction.

Appendix D.4. Association between perceived corruption and civic duty to vote with all observations (AO) and with unique observations (UO)

	DV: Civic Duty to Vote							
	(AO1)	(UO1)	(AO2)	(UO2)	(AO3)	(UO3)	(AO4)	(UO4)
Corruption: National (Hardly any – A lot)	-0.094*** (0.021)	-0.093*** (0.025)						
Corruption: Regional (Hardly any – A lot)			-0.094*** (0.021)	-0.053 (0.042)				
Corruption: European (Hardly any – A lot)					-0.130*** (0.025)	-0.185*** (0.033)		
Corruption: Municipal (Hardly any – A lot)							-0.116*** (0.020)	-0.064 (0.134)
Political Trust	0.019 (0.032)	0.019 (0.047)	0.020 (0.035)	0.041 (0.027)	0.059+ (0.033)	0.155* (0.061)	0.007 (0.028)	0.321+ (0.173)
Partisanship	0.333*** (0.035)	0.301*** (0.045)	0.297*** (0.031)	0.481*** (0.091)	0.265*** (0.047)	0.285* (0.136)	0.260*** (0.052)	0.745*** (0.051)
Political Interest	0.112*** (0.020)	0.083** (0.028)	0.111*** (0.024)	0.145*** (0.029)	0.091*** (0.020)	0.161*** (0.026)	0.086*** (0.023)	0.083 (0.059)
Female	0.006 (0.039)	0.061 (0.051)	0.033 (0.038)	-0.091 (0.078)	0.155** (0.055)	0.042 (0.139)	0.008 (0.039)	0.007*** (0.002)
Age	-0.004 (0.004)	-0.009 (0.006)	-0.001 (0.005)	0.001 (0.003)	-0.005 (0.004)	0.002 (0.005)	0.003 (0.004)	0.003 (0.002)
Education	0.007 (0.054)	0.008 (0.077)	0.043 (0.049)	0.030 (0.086)	0.095* (0.044)	0.005 (0.117)	0.019 (0.051)	-0.196 (0.394)
Religiosity	0.174*** (0.037)	0.142* (0.061)	0.200*** (0.054)	0.181+ (0.096)	0.177*** (0.048)	0.109* (0.054)	0.255*** (0.040)	0.310*** (0.040)
Constant	-1.218*** (0.340)	-0.788 (0.493)	-1.648*** (0.346)	-2.216*** (0.355)	-0.634* (0.258)	-0.756 (0.809)	-1.449*** (0.358)	-0.474 (0.682)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	18,453	8,477	16,464	3,122	10,237	1,682	15,063	671

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. Election fixed effects are not reported.

Appendix D.5. Association between perceived corruption and self-reported turnout with all observations (AO) and with unique observations (UO)

	National				Regional				European				Municipal			
	(AO1)	(AO1)	(UO1)	(UO1)	(AO2)	(AO2)	(UO2)	(UO2)	(AO3)	(AO3)	(UO3)	(UO3)	(AO4)	(AO4)	(UO4)	(UO4)
Corruption: National (Hardly any – A lot)	-0.246*** (0.070)	-0.204*** (0.076)	-0.235** (0.075)	-0.201* (0.080)												
Corruption: Regional (Hardly any – A lot)					-0.297*** (0.083)	-0.263** (0.092)	-0.285** (0.109)	-0.262* (0.116)								
Corruption: European (Hardly any – A lot)									-0.244*** (0.049)	-0.183*** (0.052)	-0.238*** (0.029)	-0.175*** (0.024)				
Corruption: Municipal (Hardly any – A lot)													-0.336*** (0.022)	-0.288*** (0.013)	-0.298*** (0.021)	-0.246*** (0.016)
Civic Duty		1.316*** (0.162)		1.229*** (0.184)		1.478*** (0.153)		1.370*** (0.185)		1.318*** (0.163)		1.182*** (0.225)		1.268*** (0.004)		1.262*** (0.075)
Constant	2.396*** (0.179)	1.937*** (0.207)	2.355*** (0.188)	1.923*** (0.226)	2.242*** (0.187)	1.831*** (0.221)	2.296*** (0.240)	1.937*** (0.266)	2.174*** (0.156)	1.400*** (0.160)	2.362*** (0.092)	1.598*** (0.147)	2.474*** (0.064)	1.648*** (0.036)	2.439*** (0.062)	1.551*** (0.010)
Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12,250	12,250	8,477	8,477	4,886	4,886	3,122	3,122	2,983	2,983	1,682	1,682	1,247	1,247	671	671

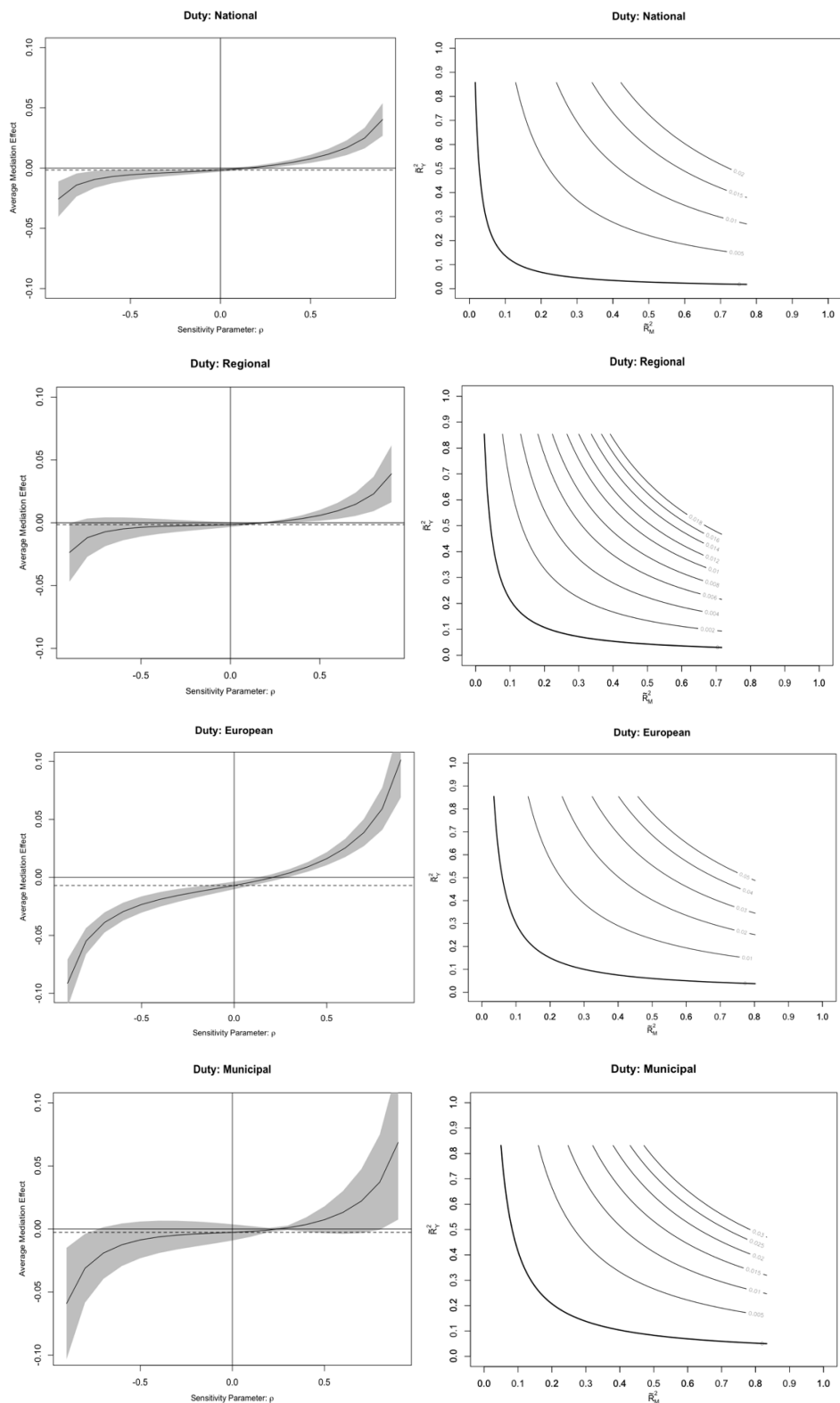
Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All self-reported turnout items are dichotomous: 0=no; 1=yes. Adding civic duty in the models with unique observations lead to a 14, 8, 26, and 17 percent decline in the effect of the perceived corruption measures corresponding to national, regional, European, and municipal elections, respectively. This corresponds to a mere 1 percentage point increase in the mediation effect of duty to vote in European elections, level that is also significant in the analysis of the corruption-duty nexus.

Appendix D.6. Association between perceived corruption and self-reported turnout with log-odds (LO) and predicted probabilities (PP)

	National				Regional				European				Municipal			
	(LO1)	(LO1)	(PP1)	(PP1)	(LO2)	(LO2)	(PP2)	(PP2)	(LO3)	(LO3)	(PP3)	(PP3)	(LO4)	(LO4)	(PP4)	(PP4)
Corruption: National (Hardly any – A lot)	-0.246*** (0.070)	-0.204*** (0.076)	-0.019*** (0.005)	-0.015*** (0.006)												
Corruption: Regional (Hardly any – A lot)					-0.297*** (0.083)	-0.263** (0.092)	-0.030*** (0.008)	-0.026** (0.009)								
Corruption: European (Hardly any – A lot)									-0.244*** (0.049)	-0.183*** (0.052)	-0.038*** (0.007)	-0.027*** (0.024)				
Corruption: Municipal (Hardly any – A lot)													-0.336*** (0.022)	-0.288*** (0.013)	-0.051*** (0.003)	-0.041*** (0.002)
Civic Duty		1.316*** (0.162)		0.098*** (0.011)		1.478*** (0.153)		0.145*** (0.014)		1.318*** (0.163)		0.193*** (0.021)		1.268*** (0.004)		0.180*** (0.000)
Constant	2.396*** (0.179)	1.937*** (0.207)			2.242*** (0.187)	1.831*** (0.221)			2.174*** (0.156)	1.400*** (0.160)			2.474*** (0.064)	1.648*** (0.036)		
Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12,250	12,250	12,250	12,250	4,886	4,886	4,886	4,886	2,983	2,983	2,983	2,983	1,247	1,247	1,247	1,247

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds or predicted probabilities from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All self-reported turnout items are dichotomous: 0=no; 1=yes. Adding civic duty in the regressions lead to a 21, 13, 29, and 20 percent decline in the effect of the perceived corruption measures corresponding to national, regional, European, and municipal elections, respectively. These values are marginally superior than the original ones (17, 11, 25, and 14 percent).

Appendix D.7. Sensitivity tests of the sequential ignorability assumption (SIA)



Notes: Linear-model estimates with political trust, partisanship, political interest, gender, age, education level and religiosity as covariates. ρ touches an AME of zero when it equals 0.1 in the case of turnout in national elections, and 0.2 in all other cases. Furthermore, the AME is zero (as indicated by the first curve that is in dark gray) when the product of the R^2 is .04 or lower.

Appendix D.8. Association between perceived corruption, civic duty to vote (CD), external efficacy (EE), and political disenchantment (PD)

	National			Regional			European			Municipal		
	(CD1)	(EE1)	(PD1)	(CD2)	(EE2)	(PD2)	(CD3)	(EE3)	(PD3)	(CD4)	(EE4)	(PD4)
Corruption: National (Hardly any – A lot)	-0.098*** (0.023)	-0.324*** (0.018)	-0.180*** (0.009)									
Corruption: Regional (Hardly any – A lot)				-0.101*** (0.021)	-0.366*** (0.029)	-0.167*** (0.008)						
Corruption: European (Hardly any – A lot)							-0.140*** (0.025)	-0.206*** (0.015)	-0.173*** (0.009)			
Corruption: Municipal (Hardly any – A lot)										-0.116*** (0.020)	-0.155*** (0.017)	-0.093*** (0.013)
Partisanship	0.339*** (0.034)	0.136*** (0.024)	0.151*** (0.014)	0.306*** (0.030)	0.156*** (0.023)	0.151*** (0.014)	0.276*** (0.044)	0.147*** (0.022)	0.166*** (0.021)	0.269*** (0.053)	0.170*** (0.032)	0.169*** (0.015)
Political Interest	0.113*** (0.020)	0.041*** (0.003)	0.025*** (0.003)	0.112*** (0.023)	0.042*** (0.004)	0.029*** (0.003)	0.093*** (0.020)	0.045*** (0.005)	0.024*** (0.003)	0.085*** (0.022)	0.050*** (0.004)	0.029*** (0.003)
Female	0.009 (0.039)	0.056** (0.015)	0.037* (0.014)	0.041 (0.038)	0.089** (0.025)	0.020 (0.015)	0.155** (0.054)	0.030+ (0.016)	-0.013 (0.015)	0.012 (0.038)	0.003 (0.018)	0.012 (0.018)
Age	-0.004 (0.004)	-0.004*** (0.001)	-0.004*** (0.000)	-0.001 (0.005)	-0.004*** (0.001)	-0.004*** (0.000)	-0.005 (0.004)	-0.005*** (0.001)	-0.004*** (0.000)	0.003 (0.004)	-0.005*** (0.001)	-0.005*** (0.000)
Education	0.011 (0.055)	-0.017 (0.022)	0.042*** (0.010)	0.050 (0.050)	-0.002 (0.019)	0.055*** (0.011)	0.101* (0.048)	-0.014 (0.021)	0.037* (0.015)	0.024 (0.050)	0.013 (0.024)	0.069*** (0.011)
Religiosity	0.173*** (0.037)	0.083*** (0.018)	0.073*** (0.009)	0.199*** (0.054)	0.113*** (0.019)	0.068*** (0.010)	0.185*** (0.045)	0.089** (0.026)	0.062*** (0.013)	0.254*** (0.040)	0.108*** (0.019)	0.074*** (0.010)
Constant	-1.191*** (0.355)	2.968*** (0.078)	-1.218*** (0.340)	-1.629*** (0.362)	3.200*** (0.071)	-1.648*** (0.346)	-0.548* (0.218)	2.722*** (0.084)	-0.634* (0.258)	-1.445*** (0.360)	2.439*** (0.079)	-1.449*** (0.358)
Fixed Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	18,603	14,064	18,453	16,594	12,948	16,464	10,295	7,929	10,237	15,196	11,562	15,063

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. Sense of external efficacy (measured by “How much do you think the following care about what people like you think: National/Regional Government?”) is categorical: 1=none; 2=a little; 3=some; 4=a lot. Political disenchantment (measured by respondents’ agreement/disagreement with “Politicians make campaign promises they have no intention of keeping”) is also categorical: 1=strongly disagree; 2=somewhat disagree; 3=somewhat agree; 4=strongly agree. Trust – insignificant in the original civic duty models – was excluded as a control variable for comparative purposes. Results indicate that perceived corruption harms all three attitudes.

Appendix D.9. Association between perceived corruption and self-reported turnout with and without sense of civic duty (CD), external efficacy (EE), and political disenchantment (PD) in the model

	National						Regional					
	(CD1)	(CD1)	(EE1)	(EE1)	(PD1)	(PD1)	(CD2)	(CD2)	(EE2)	(EE2)	(PD2)	(PD2)
Corruption: National (Hardly any – A lot)	-0.246*** (0.070)	-0.204*** (0.076)	-0.215** (0.075)	-0.131+ (0.071)	-0.266*** (0.072)	-0.213** (0.066)						
Corruption: Regional (Hardly any – A lot)							-0.297*** (0.083)	-0.263** (0.092)	-0.263** (0.096)	-0.178+ (0.096)	-0.306*** (0.089)	-0.234** (0.081)
Corruption: European (Hardly any – A lot)												
Corruption: Municipal (Hardly any – A lot)												
Civic Duty		1.316*** (0.162)						1.478*** (0.153)				
External Efficacy				0.257*** (0.042)						0.221*** (0.038)		
Political Disench.						0.290*** (0.061)						0.340*** (0.085)
Constant	2.396*** (0.179)	1.937*** (0.207)	2.270*** (0.191)	1.476*** (0.142)	2.401*** (0.185)	1.726*** (0.178)	2.242*** (0.187)	1.831*** (0.221)	2.298*** (0.218)	1.558*** (0.234)	2.204*** (0.202)	1.384*** (0.208)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12,250	12,250	8,685	8,685	12,334	12,334	4,886	4,886	4,124	4,124	4,921	4,921

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. Sense of external efficacy (measured by the question “How much do you think the following care about what people like you thing: National/Regional Government?”) is categorical: 1=none; 2=a little; 3=some; 4=a lot. Political disenchantment (measured by respondents’ agreement/disagreement with “Politicians make campaign promises they have no intention of keeping”) is also categorical: 1=strongly disagree; 2=somewhat disagree; 3=somewhat agree; 4=strongly agree. All self-reported turnout items are dichotomous: 0=no; 1=yes. Adding sense of civic duty to the regressions lead to a 17 and 11; external efficacy, a 39 and 32; and political disenchantment, a 20 and 23 percent decline in the effect of the perceived corruption measures corresponding to national and regional elections. Hence, external efficacy and political disenchantment constitute stronger mediators than civic duty.

Appendix D.10. Association between perceived corruption and self-reported turnout with and without sense of civic duty (CD), external efficacy (EE), and political disenchantment (PD) in the model

	European						Municipal					
	(CD3)	(CD3)	(EE3)	(EE3)	(PD3)	(PD3)	(CD4)	(CD4)	(EE4)	(EE4)	(PD4)	(PD4)
Corruption: National (Hardly any – A lot)												
Corruption: Regional (Hardly any – A lot)												
Corruption: European (Hardly any – A lot)	-0.244*** (0.049)	-0.183*** (0.052)	-0.265*** (0.053)	-0.265*** (0.059)	-0.255*** (0.053)	-0.268*** (0.051)						
Corruption: Municipal (Hardly any – A lot)							-0.336*** (0.022)	-0.288*** (0.013)	-0.327*** (0.024)	-0.281*** (0.056)	-0.342*** (0.025)	-0.298*** (0.041)
Civic Duty		1.318*** (0.163)						1.268*** (0.004)				
External Efficacy				0.001 (0.070)						0.171** (0.053)		
Political Disench.						-0.086 (0.063)						0.222*** (0.027)
Constant	2.174*** (0.156)	1.400*** (0.160)	2.196*** (0.168)	2.193*** (0.264)	2.159*** (0.170)	2.314*** (0.186)	2.474*** (0.064)	1.648*** (0.036)	2.430*** (0.069)	1.998*** (0.253)	2.480*** (0.072)	2.038*** (0.155)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,983	2,983	3,076	3,076	3,077	3,077	1,247	1,247	1,255	1,255	1,259	1,259

Notes: ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. Sense of external efficacy (measured by the question “How much do you think the following care about what people like you thing: National/Regional Government?”) is categorical: 1=none; 2=a little; 3=some; 4=a lot. Political disenchantment (measured by respondents’ agreement/disagreement with “Politicians make campaign promises they have no intention of keeping”) is also categorical: 1=strongly disagree; 2=somewhat disagree; 3=somewhat agree; 4=strongly agree. All self-reported turnout items are dichotomous: 0=no; 1=yes. Adding sense of civic duty to the regressions lead to a 25 and 14; external efficacy, a 0 and 14; and political disenchantment, a 0 and 13 percent decline in the effect of the perceived corruption measures corresponding to European and municipal elections. From these results, we may conclude that civic duty plays a stronger mediation role only when it comes to European elections, as the estimated mediation effect of civic duty and external efficacy are equivalent regarding municipal elections.

Appendix D.11. Association between perceived corruption and benefit of voting

	DV: Benefit of Voting		
	National (1)	Regional (2)	Municipal (3)
Corruption: National (Hardly any – A lot)	-0.220*** (0.038)		
Corruption: Regional (Hardly any – A lot)		-0.252*** (0.049)	
Corruption: Municipal (Hardly any – A lot)			-0.308** (0.099)
Political Trust	0.340*** (0.043)	0.268*** (0.041)	0.273 (0.170)
Partisanship	0.729*** (0.083)	0.754*** (0.102)	0.926*** (0.182)
Political Interest	0.269*** (0.014)	0.246*** (0.014)	0.233*** (0.069)
Female	0.173* (0.081)	0.155+ (0.093)	0.197*** (0.054)
Age	-0.005 (0.003)	0.007** (0.003)	0.005+ (0.003)
Education	0.284*** (0.056)	0.401*** (0.044)	0.981*** (0.039)
Religiosity	-0.008 (0.056)	0.044 (0.065)	0.157*** (0.038)
Constant	-1.216*** (0.280)	-2.026*** (0.247)	-3.221*** (0.143)
Fixed-Effects: Election	Yes	Yes	Yes
N	10,270	9,301	919

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results from logistic regressions, with clustered standard errors (by election study) in parentheses. All benefit indicators are dichotomous: 0=no; 1=yes. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. Study fixed-effects coefficients are not reported. There is no question on benefit of voting in European elections.

Appendix D.12. Average marginal effect of perceived corruption on benefit of voting

	DV: Benefit of Voting		
	National	Regional	Municipal
Corruption: National (Hardly any – A lot)	-0.032*** (0.005)		
Corruption: Regional (Hardly any – A lot)		-0.035*** (0.007)	
Corruption: Municipal (Hardly any – A lot)			-0.054*** (0.016)
N	10,270	9,301	919

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to changes in predicted probabilities of reporting a benefit in voting. The corresponding log-odds coefficients can be seen on Appendix D.5.

Appendix D.13. Association between perceived corruption and self-reported turnout

	DV: Self-Reported Turnout					
	National		Regional		Municipal	
	(1)	(2)	(3)	(4)	(5)	(6)
Corruption: National (Hardly any – A lot)	-0.269*** (0.073)	-0.217** (0.082)				
Corruption: Regional (Hardly any – A lot)			-0.304*** (0.086)	-0.257** (0.082)		
Corruption: Municipal (Hardly any – A lot)					-0.329*** (0.022)	-0.226*** (0.039)
Benefit of Voting		0.980*** (0.111)		0.962*** (0.045)		0.948*** (0.078)
Constant	2.399*** (0.189)	1.552*** (0.246)	2.158*** (0.195)	1.369*** (0.206)	2.444*** (0.065)	1.533*** (0.159)
Fixed-Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes
N	12,479	12,479	4,959	4,959	1,269	1,269

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All perceived corruption measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All self-reported turnout items are dichotomous: 0=no; 1=yes. All benefit items are dichotomous: 0=no; 1=yes. Including benefit of voting in the turnout regressions yields a 19, 15, and 33 percent decline in the effect of perceived corruption measures regarding national, regional, and municipal elections, respectively. There is no question on benefit of voting in European elections.

Appendix D.14. Association between perceived corruption and cost of voting

	DV: Cost of Voting			
	National (1)	Regional (2)	European (3)	Municipal (4)
Corruption: National (Hardly any – A lot)	0.198*** (0.027)			
Corruption: Regional (Hardly any – A lot)		0.188*** (0.033)		
Corruption: European (Hardly any – A lot)			0.148*** (0.027)	
Corruption: Municipal (Hardly any – A lot)				0.153*** (0.021)
Political Trust	0.021 (0.034)	0.015 (0.028)	0.049 (0.033)	0.004 (0.035)
Partisanship	-0.803*** (0.082)	-0.793*** (0.080)	-1.028*** (0.105)	-0.776*** (0.090)
Political Interest	-0.116*** (0.018)	-0.117*** (0.018)	-0.089*** (0.022)	-0.129*** (0.022)
Female	0.588*** (0.055)	0.596*** (0.056)	0.605*** (0.064)	0.595*** (0.065)
Age	-0.015*** (0.003)	-0.015*** (0.003)	-0.011*** (0.002)	-0.014*** (0.003)
Education	-0.042 (0.053)	-0.047 (0.052)	-0.017 (0.064)	-0.077 (0.056)
Religiosity	-0.010 (0.057)	-0.004 (0.059)	0.035 (0.077)	-0.002 (0.065)
Constant	0.060 (0.256)	0.158 (0.187)	-1.017*** (0.258)	0.383+ (0.204)
Fixed-Effects: Election	Yes	Yes	Yes	Yes
N	14,766	14,667	8,627	12,308

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. The cost variable is dichotomous: 0=no; 1=yes. Study fixed-effects coefficients are not reported.

Appendix D.15. Average marginal effect of perceived corruption on cost of voting

	DV: Cost of Voting			
	National	Regional	European	Municipal
Corruption: National (Hardly any – A lot)	0.023*** (0.003)			
Corruption: Regional (Hardly any – A lot)		0.022*** (0.004)		
Corruption: European (Hardly any – A lot)			0.017*** (0.003)	
Corruption: Municipal (Hardly any – A lot)				0.018*** (0.002)
N	14,766	14,667	8,627	12,308

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to changes in predicted probabilities of considering voting costly. The corresponding log-odds coefficients can be seen on Appendix D.8.

Appendix D.16. Association between perceived corruption and self-reported turnout

	DV: Self-Reported Turnout							
	National		Regional		European		Municipal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption: National (Hardly any – A lot)	-0.210** (0.073)	-0.148* (0.074)						
Corruption: Regional (Hardly any – A lot)			-0.295*** (0.087)	-0.243** (0.081)				
Corruption: European (Hardly any – A lot)					-0.247*** (0.050)	-0.212*** (0.055)		
Corruption: Municipal (Hardly any – A lot)							-0.332*** (0.019)	-0.303*** (0.005)
Cost of Voting		-1.213*** (0.161)		-1.036*** (0.116)		-0.638*** (0.097)		-0.534* (0.254)
Constant	2.272*** (0.185)	2.535*** (0.184)	2.177*** (0.196)	2.313*** (0.177)	2.142*** (0.159)	2.196*** (0.169)	2.465*** (0.054)	2.473*** (0.035)
Fixed-Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	8,758	8,758	4,909	4,909	3,043	3,043	1,255	1,255

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All self-reported turnout items are dichotomous: 0=no; 1=yes. The cost variable is dichotomous: 0=no; 1=yes. Adding cost of voting in the turnout regressions yields a 29, 18, 14, and 8 percent decline in the effect of perceived corruption measures regarding national, regional, European, and municipal elections, respectively.

Appendix D.17. Association between perceived corruption and civic duty to vote with perceived corruption-age interactions

	DV: Civic Duty to Vote			
	National (1)	Regional (2)	European (3)	Municipal (4)
Corruption: National (Hardly any – A lot)	-0.186*** (0.046)			
Corruption: National*Age	0.002+ (0.001)			
Corruption: Regional (Hardly any – A lot)		-0.163*** (0.047)		
Corruption: Regional*Age		0.001 (0.001)		
Corruption: European (Hardly any – A lot)			0.037 (0.086)	
Corruption: European*Age			-0.004+ (0.002)	
Corruption: Municipal (Hardly any – A lot)				-0.245*** (0.039)
Corruption: Municipal*Age				0.003** (0.001)
Age	-0.010* (0.004)	-0.005 (0.004)	0.006 (0.008)	-0.003 (0.005)
Political Trust	0.019 (0.032)	0.020 (0.035)	0.056+ (0.033)	0.007 (0.028)
Partisanship	0.334*** (0.036)	0.298*** (0.031)	0.265*** (0.046)	0.260*** (0.052)
Political Interest	0.112*** (0.020)	0.111*** (0.024)	0.091*** (0.020)	0.087*** (0.023)
Female	0.006 (0.039)	0.033 (0.038)	0.158** (0.054)	0.008 (0.039)
Education	0.007 (0.054)	0.044 (0.049)	0.094* (0.045)	0.020 (0.051)
Religiosity	0.174*** (0.037)	0.201*** (0.054)	0.175*** (0.048)	0.257*** (0.040)
Constant	-0.959** (0.310)	-1.469*** (0.300)	-1.144** (0.427)	-1.170** (0.383)
Fixed-Effects: Election Study	Yes	Yes	Yes	Yes
N	18,453	16,464	10,237	15,063

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results from logistic regressions with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. Study fixed-effects coefficients are not reported. Only in the regression of duty to vote in municipal elections the interaction term is statistically significant and in the expected (positive) direction.

Appendix D.18. Turnout rates accounting for nonresponse bias and overreporting in the MEDW data

Country	Region/ Municipality	Election Level	Actual Turnout Values	Original Turnout Values	Weighted MEDW Data	Face-Saving Turnout Question	Weighted MEDW Data & Face-Saving Turnout Question
Canada	Ontario	Regional	.49	.85	.53	.84	.52
	Quebec	Regional	.75	.92	.80	.88	.71
	Ontario	National	.68	.91	.75	.88	.69
	British Columbia	National	.70	.89	.75	.88	.75
France	Quebec	National	.66	.89	.73	.87	.67
	Île-de-France	National	.54	.78	.67	.77	.64
	Provence	National	.56	.82	.66	.79	.63
	Provence	European	.43	.78	.40	.71	.30
	Île-de-France	European	.43	.78	.54	.70	.44
	Paris	Municipal	.55	.82	.71	.78	.66
Germany	Marseille	Municipal	.50	.78	.75	.76	.72
	Lower Saxony	National	.73	.92	.85	n.a.	n.a.
	Lower Saxony	Regional	.59	.87	.72	n.a.	n.a.
	Bavaria	National	.70	.94	.76	n.a.	n.a.
Spain	Lower Saxony	European	.49	.78	.64	n.a.	n.a.
	Catalonia	National	.66	.89	.74	.87	.72
	Catalonia	Regional	.70	.91	.73	.87	.66
	Madrid	National	.73	.93	.83	.90	.76
	Catalonia	European	.46	.79	.58	.71	.48
	Madrid	European	.46	.79	.60	.70	.50
Switzerland	Madrid	Regional	.66	.91	.74	.86	.64
	Lucerne	National	.53	.82	.54	.77	.48
	Lucerne	Regional	.50	.78	.47	.74	.40
	Zurich	National	.43	.86	.58	.85	.54
	Zurich	Regional	.35	.82	.38	.79	.33

Notes: Weights correspond to age, gender, education, turnout and election results. The face-saving turnout question reads “Which statement represents your voting behavior in this election? I did not vote in the election; I thought about voting this time but didn’t; I usually vote but didn’t this time; I am sure I voted in the election”. Average difference between actual turnout values and corrected values is .09, .24 and .02 when it comes to weighted MEDW data, face-saving question, and weighted MEDW data & face-saving turnout question, respectively.

Appendix D.19. Association between perceived corruption and civic duty to vote with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data & face-saving turnout question (WDFQ)

	DV: Civic Duty to Vote							
	(OD1)	(WD1)	(FQ1)	(WDFQ1)	(OD2)	(WD2)	(FQ2)	(WDFQ2)
Corruption: National (Hardly any – A lot)	-0.094*** (0.021)	-0.078* (0.034)	-0.028 (0.035)	0.003 (0.051)				
Corruption: Regional (Hardly any – A lot)					-0.094*** (0.021)	-0.087** (0.034)	-0.111** (0.041)	-0.060 (0.055)
Corruption: European (Hardly any – A lot)								
Corruption: Municipal (Hardly any – A lot)								
Political Trust	0.019 (0.032)	0.031 (0.034)	0.035 (0.044)	0.081 (0.052)	0.020 (0.035)	0.033 (0.037)	-0.013 (0.037)	0.060 (0.049)
Partisanship	0.333*** (0.035)	0.383*** (0.046)	0.368*** (0.081)	0.385*** (0.089)	0.297*** (0.031)	0.369*** (0.057)	0.352*** (0.087)	0.399*** (0.099)
Political Interest	0.112*** (0.020)	0.135*** (0.019)	0.133*** (0.012)	0.148*** (0.022)	0.111*** (0.024)	0.139*** (0.024)	0.143*** (0.012)	0.154*** (0.022)
Female	0.006 (0.039)	-0.047 (0.049)	0.013 (0.059)	-0.042 (0.073)	0.033 (0.038)	-0.008 (0.041)	0.080 (0.057)	0.011 (0.080)
Age	-0.004 (0.004)	-0.003 (0.005)	0.001 (0.003)	0.009** (0.003)	-0.001 (0.005)	-0.001 (0.004)	0.006 (0.004)	0.015*** (0.003)
Education	0.007 (0.054)	0.021 (0.058)	0.005 (0.106)	-0.006 (0.118)	0.043 (0.049)	0.067 (0.049)	-0.035 (0.114)	0.015 (0.123)
Religiosity	0.174*** (0.037)	0.156*** (0.043)	0.099 (0.077)	0.104 (0.099)	0.200*** (0.054)	0.172** (0.054)	0.087 (0.093)	0.087 (0.111)
Constant	-1.218*** (0.340)	-1.595*** (0.373)	-1.826*** (0.403)	-2.491*** (0.399)	-1.648*** (0.346)	-2.007*** (0.368)	-1.943*** (0.403)	-2.795*** (0.403)
Fixed Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	18,453	18,453	4,855	4,855	16,464	16,464	4,088	4,088

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. In the analysis with weighted MEDW data & a face-saving turnout question, the correlation between corruption and civic duty fails to achieve statistical significance.

Appendix D.20. Association between perceived corruption and civic duty to vote with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data joint with face-saving turnout question (WDFQ)

	DV: Civic Duty to Vote							
	(OD3)	(WD3)	(FQ3)	(WDFQ3)	(OD4)	(WD4)	(FQ4)	(WDFQ4)
Corruption: National (Hardly any – A lot)								
Corruption: Regional (Hardly any – A lot)								
Corruption: European (Hardly any – A lot)	-0.130*** (0.025)	-0.137*** (0.031)	-0.181*** (0.040)	-0.212*** (0.053)				
Corruption: Municipal (Hardly any – A lot)					-0.116*** (0.020)	-0.102*** (0.030)	-0.157*** (0.039)	-0.112+ (0.064)
Political Trust	0.059+ (0.033)	0.103* (0.041)	0.115+ (0.070)	0.270* (0.115)	0.007 (0.028)	0.011 (0.030)	-0.033 (0.054)	-0.019 (0.067)
Partisanship	0.265*** (0.047)	0.322*** (0.068)	0.228* (0.108)	0.345* (0.161)	0.260*** (0.052)	0.310*** (0.062)	0.325*** (0.078)	0.311** (0.103)
Political Interest	0.091*** (0.020)	0.120*** (0.016)	0.116*** (0.013)	0.142*** (0.011)	0.086*** (0.023)	0.113*** (0.022)	0.106*** (0.015)	0.130*** (0.021)
Female	0.155** (0.055)	0.154+ (0.091)	-0.017 (0.099)	-0.087 (0.164)	0.008 (0.039)	-0.025 (0.049)	0.014 (0.073)	-0.000 (0.085)
Age	-0.005 (0.004)	-0.005 (0.004)	0.004 (0.003)	0.005 (0.004)	0.003 (0.004)	0.005 (0.005)	0.005+ (0.003)	0.013*** (0.003)
Education	0.095* (0.044)	0.051 (0.044)	0.016 (0.075)	-0.015 (0.085)	0.019 (0.051)	0.021 (0.062)	0.011 (0.112)	-0.060 (0.137)
Religiosity	0.177*** (0.048)	0.135* (0.059)	0.065 (0.102)	-0.101 (0.142)	0.255*** (0.040)	0.265*** (0.048)	0.154* (0.075)	0.101 (0.099)
Constant	-0.634* (0.258)	-0.912*** (0.178)	-0.841*** (0.245)	-1.079*** (0.305)	-1.449*** (0.358)	-1.798*** (0.377)	-1.545*** (0.335)	-2.055*** (0.437)
Fixed Effects: Election Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10,237	10,237	1,955	1,955	15,063	15,063	3,866	3,866

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses. All corruption variables are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All civic duty indicators are dichotomous: 0=choice; 1=duty. In the analysis with weighted MEDW data & a face-saving turnout question, the correlation between corruption and civic duty is significant only when it comes to European elections.

Appendix D.21. Association between perceived corruption and self-reported turnout in national elections with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data & face-saving turnout question (WDFQ)

	National							
	(OD1)	(OD1)	(WD1)	(WD1)	(FQ1)	(FQ1)	(WDFQ1)	(WDFQ1)
Corruption: National (Hardly any – A lot)	-0.246*** (0.070)	-0.204*** (0.076)	-0.316*** (0.081)	-0.279** (0.085)	-0.189* (0.080)	-0.148+ (0.082)	-0.218*** (0.066)	-0.176* (0.073)
Corruption: Regional (Hardly any – A lot)								
Corruption: European (Hardly any – A lot)								
Corruption: Municipal (Hardly any – A lot)								
Civic Duty		1.316*** (0.162)		1.342*** (0.175)		1.519*** (0.147)		1.597*** (0.171)
Constant	2.396*** (0.179)	1.937*** (0.207)	1.231*** (0.208)	0.753** (0.247)	1.863*** (0.195)	1.379*** (0.206)	0.684*** (0.164)	0.130 (0.200)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12,250	12,250	12,250	12,250	4,116	4,116	4,116	4,116

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. Weights correspond to age, gender, education, turnout and actual election results. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All turnout items are dichotomous: 0=no/ I did not vote in the election/I thought about voting this time but didn't/I usually vote but didn't this time; 1=yes/I am sure I voted in the election.

Appendix D.22. Association between perceived corruption and self-reported turnout in regional elections with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data & face-saving turnout question (WDFQ)

	Regional							
	(OD2)	(OD2)	(WD2)	(WD2)	(FQ2)	(FQ2)	(WDFQ2)	(WDFQ2)
Corruption: National (Hardly any – A lot)								
Corruption: Regional (Hardly any – A lot)	-0.297*** (0.083)	-0.263** (0.092)	-0.323*** (0.081)	-0.290*** (0.086)	-0.313*** (0.094)	-0.286** (0.098)	-0.304** (0.093)	-0.288** (0.101)
Corruption: European (Hardly any – A lot)								
Corruption: Municipal (Hardly any – A lot)								
Civic Duty		1.478*** (0.153)		1.463*** (0.175)		1.529*** (0.186)		1.558*** (0.243)
Constant	2.242*** (0.187)	1.831*** (0.221)	0.908*** (0.178)	0.490* (0.212)	2.012*** (0.214)	1.655*** (0.239)	0.565** (0.211)	0.192 (0.263)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4,886	4,886	4,886	4,886	2,238	2,238	2,238	2,238

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. Weights correspond to age, gender, education, turnout and actual election results. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All turnout items are dichotomous: 0=no/ I did not vote in the election/I thought about voting this time but didn't/I usually vote but didn't this time; 1=yes/I am sure I voted in the election.

Appendix D.23. Association between perceived corruption and self-reported turnout in European elections with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data & face-saving turnout question (WDFQ)

	European							
	(OD3)	(OD3)	(WD3)	(WD3)	(FQ3)	(FQ3)	(WDFQ3)	(WDFQ3)
Corruption: National (Hardly any – A lot)								
Corruption: Regional (Hardly any – A lot)								
Corruption: European (Hardly any – A lot)	-0.244*** (0.049)	-0.183*** (0.052)	-0.293* (0.115)	-0.208+ (0.123)	-0.208** (0.069)	-0.155* (0.069)	-0.322** (0.099)	-0.253* (0.102)
Corruption: Municipal (Hardly any – A lot)								
Civic Duty		1.318*** (0.163)		1.369*** (0.131)		1.538*** (0.113)		1.481*** (0.064)
Constant	2.174*** (0.156)	1.400*** (0.160)	0.719* (0.361)	-0.152 (0.350)	1.668*** (0.214)	0.796*** (0.195)	0.301 (0.301)	-0.516+ (0.299)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,983	2,983	2,983	2,983	1,329	1,329	1,329	1,329

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. Weights correspond to age, gender, education, turnout and actual election results. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All turnout items are dichotomous: 0=no/ I did not vote in the election/I thought about voting this time but didn't/I usually vote but didn't this time; 1=yes/I am sure I voted in the election. Adding civic duty in the model with weighted MEDW data & a face-saving turnout question leads to a 21 percent decline in the effect of the perceived corruption measure corresponding to European elections. This is 4 percentage points less than in the analysis with the original MEDW data.

Appendix D.24. Association between perceived corruption and self-reported turnout in municipal elections with original MEDW data (OD), weighted MEDW data (WD), face-saving turnout question (FQ), and weighted MEDW data & face-saving turnout question (WDFQ)

	Municipal							
	(OD4)	(OD4)	(WD4)	(WD4)	(FQ4)	(FQ4)	(WDFQ4)	(WDFQ4)
Corruption: National (Hardly any – A lot)								
Corruption: Regional (Hardly any – A lot)								
Corruption: European (Hardly any – A lot)								
Corruption: Municipal (Hardly any – A lot)	-0.336*** (0.022)	-0.288*** (0.013)	-0.421*** (0.035)	-0.375*** (0.087)	-0.313*** (0.010)	-0.263*** (0.062)	-0.388*** (0.118)	-0.327 (0.207)
Civic Duty		1.268*** (0.004)		1.318*** (0.058)		1.238*** (0.006)		1.240*** (0.050)
Constant	2.474*** (0.064)	1.648*** (0.036)	2.108*** (0.099)	1.288*** (0.275)	2.106*** (0.029)	1.264*** (0.177)	1.718*** (0.333)	0.845 (0.609)
Fixed Effects: Election	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,247	1,247	1,247	1,247	613	613	613	613

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Values correspond to log-odds from logistic regressions, with clustered standard errors (by election study) in parentheses and without individual-level control variables. Weights correspond to age, gender, education, turnout and actual election results. All corruption perceptions measures are categorical: 1=hardly any corruption; 2=a little corruption; 3=some corruption; 4=a lot of corruption. All duty items are dichotomous: 0=choice; 1=duty. All turnout items are dichotomous: 0=no/ I did not vote in the election/I thought about voting this time but didn't/I usually vote but didn't this time; 1=yes/I am sure I voted in the election.

