CLOSE ELECTIONS AND TURNOUT

The margin of victory and horseshoes in comparative perspective

Master thesis

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ABSTRACT

More people decide to vote when the race is close than when it is not. This is arguably the most consistent finding in aggregate-level research on turnout. However, studies in recent years using multilevel analysis consistently tend not to find any such link at the individual-level. My motivation for this thesis was to uncover the reason for this puzzling inconsistency. I argue that closeness indeed can affects the decision to vote, but that this effect is contingent and indirect. The changes in aggregate turnout is the result of different mechanisms affecting different people in different situations. In this endeavor, instead of asking *if* closeness of elections affects the decision to vote, I therefore ask, *for whom might it do so?*

I use several theories from across the social sciences, and propose five different hypotheses for whom closeness might count and why that is. I hypothesize that (i) although closeness of election only affect those without a habit of voting; (ii) it fuels cognitive engagement for those that are interested in the election; and (iii) it fuels interest in the election for those who are educated or (iv) feel close to a particular party. In general, it should (v) affect the decision to vote indirectly *through* interest in the election. To test these hypotheses I use multilevel regression models and mediation analysis on cross-national survey data from the Comparative Study of Electoral System. I calculate closeness of election in a novel way so that it is comparable across different electoral systems. The complete data set includes 35,913 respondents from legislative elections in 26 countries between 2006 and 2011. Although too inconclusive for hypothesis *v*, the empirical analysis lend clear support for hypotheses *i-iv*. Indeed, the regression models indicate no statistically significant relationship when looking at the mean; only when controlling for the right things and looking in the right place do we see how closeness may help shape political behavior. The results clearly supports the notion that closeness of elections can affect the decision to vote, for some people in some situations. This contributes to an empirical foundation and understanding for the role of closeness of election in shaping turnout. It also highlights how combining different theoretical approaches can help uncover the more complex ways in which different factors interact in shaping political participation.

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1. INTRODUCTION

Electoral participation is a core subject of political science and the question of who votes and why have received unabated attention. This attention has been growing over the last few decades following the decline of turnout in established democracies (Franklin, 2004; Norris, 2004). Because of its centrality to the rational choice approach to voting, the perhaps most studied element in this enterprise has been *closeness of election* (Geys, 2006: 645). The closeness of an election is the uncertainty of the outcome when people have the opportunity to vote. It is expected that the closer the election the more probable it is that people will decide to vote. The literature have two general explanations for this relationship: In the rational choice framework of Downs (1957) and Riker and Ordeshook (1968), the instrumental benefit of voting is essentially the ability to affect the outcome. Since the probability of any single voter affecting the outcome is greater in a closer election, more people should decide to vote. This has been called the "Downsian Closeness Hypothesis" (Matsusaka and Palda, 1993) or simply the "decision hypothesis". The second is the "mobilization hypothesis". Following the work of Key (1949) and Denver and Hands (1974) stating that a closer election provoke more campaign efforts by political elites, Cox and Munger (1989) and Kirchgässner and Schulz (2005) argue that these mobilization effort in turn increase turnout (Shachar and Nalebuff, 1999). In other

words, closeness of election is expected to affect turnout either directly by altering the voters' probability of affecting the outcome or indirectly by increasing the mobilization efforts by elites.

Closeness of election matters because it is a forever moving electoral institution; the very competitiveness of elections; the dynamic temporal dimension of elections able to shape the decision to participate, whose nature varies by the characteristics of the electoral and party system. It has been a key component in explaining variation in voter turnout both empirically and theoretically. Aggregate-level research consistently find that turnout is higher in closer elections (Geys, 2006; see also Matsusaka and Palda, 1993; Blais and Dobrzynska, 1998; Franklin, 2004). This is generally regarded as the most robust finding in the voter turnout literature (Geys, 2006: 645). Some scholars have even stated that they "cannot see how this finding could be wrong" (Blais, 2006: 119). Franklin (2004: 206) goes as far as to argue that to understand the decline in turnout in established democracies, one must only look at the character of elections—putting emphasis on closeness of election—not the character of voters. However, although this aggregate-level evidence might seem compelling, it alone does not create enough leverage to make any reliable inference about individual-level behavior per se. We need individual-level analysis in order to map an empirical foundation for the relationship between close elections and turnout. This has been possible in recent years following the advent of multilevel statistical techniques within the social sciences (e.g., Bühlmann and Freitag, 2006; Fieldhouse et al., 2007; Hadjar and Beck, 2010; Hobolt et al., 2009; Kittilson and Anderson, 2011; Söderlund et al., 2011; Singh, 2011b, 2011a; Persson, 2012). This growing individual-level research, however, consistently tend *not* to find any link between the closeness of an election and the decision to vote (Smets and Ham, 2013: 12). As a result, there seems to be a rather disconcerting and puzzling inconsistency in the empirical literature. Although we know that more people tend to vote in a closer election, we lack an empirical foundation for why that might be. My motivation for this thesis was to uncover the reasons for this puzzling inconsistency.

At the outset, this inconsistency might be either one of two things. The theoretical arguments could simply be incorrect and the findings at the aggregate-level are due to some flaw in the method or the data. More likely, however, is that closeness of election do affect the decision to vote, but that this effect is contingent and indirect. If we assume that closeness of election only affects a certain group of people and only in some specific context. Then Individual-level

regression models not accounting for such dependencies will not find any relationship between closeness and voting when looking at the mean. However, aggregate-level regression models are able to notice the slight change in the total amount of people turning out in a close election as compared to one that is not. Indeed, Blais and Dobrzynska (1998), in a very renowned study of aggregate-level turnout, finds that a change of ten percentage points in the vote share between the winner of an election and the runner up only change the total amount of people turning out with about one or two points. So to uncover the reason for this aforementioned inconsistency, instead of asking *if* closeness of election do or do not affect the decision to vote, I ask:

> *For whom may the closeness of the election affect the decision to vote and why might that be?*

The multitude of theoretical explanations of political participation that have found empirical support highlight the fact there is no *single* causal mechanism or core explanation of electoral participation (Gallego, 2010; Arceneaux and Nickerson, 2009; Smets and Ham, 2013). Therefore, in a slight departure from much of this literature, I do not aim to compare contextual and individual variables in a race to predict turnout. Instead, I conceptualize the relationship between closeness of election and voting by distinguishing between supply- and demand-side factors of participation and focusing on how they work together (Kittilson and Anderson, 2011; Söderlund et al., 2011). To do this, I try to use theories from across the social sciences, including the developmental framework of voting, the resource model of participation and the heuristic-systematic model of information processing

To test these hypotheses I use multilevel regression models and mediation analysis on cross-national survey data from module 3 of the Comparative Study of Electoral System (CSES, 2013). The data set includes 35,913 respondents from legislative elections in 26 countries between 2006 and 2011. In order to measure closeness of election in way that is comparable across election systems I combine the district- and national-level margin of victory. The most common measure of district-level closeness in the literature is the margin between the two largest parties. This have no intuitive counterpart in multiparty systems using proportional representation where several electoral seats are contest simultaneously. For the district-level, I compute the margin of victory between the winner and runner up exclusively for *the marginal seat*. I then combine this with the national-level margin, measured as the margin between the

winning and losing bloc. The results of the analysis clearly supports the notion that closeness of election can affect the decision to vote, for some people in some situations.

1.1 Relevance and contribution

Understanding who votes and why is important. Elections are usually viewed as a key element of democracy, and phenomenon's like declining turnout rates (Franklin, 2004; Norris, 2004; Dalton, 2008) and unequal participation (Lijphart, 1997) are potential challenges to representative democracy. Furthermore, less competitive elections is often argued to be the source of the decline in turnout in advanced democracies (Franklin, 2004; Franklin et al., 2004; Johnston et al., 2006; see also Blais and Rubenson, 2013). It is then not surprising that trying to understand how closeness of election affects individual-level turnout is not a particularly novel endeavor. The recent advancements in multilevel techniques have allowed scholars to examine individual- and aggregate-level factors, as well as cross-level interactions, simultaneously, which have fostered an increasing amount of literature taking this approach (e.g. Birch, 2010; Bühlmann and Freitag, 2006; Söderlund et al., 2011; Kittilson and Anderson, 2011; Hadjar and Beck, 2010; Fieldhouse et al., 2007; Blais and Rubenson, 2013; Tawfik et al., 2012; Górecki, 2011; Hobolt et al., 2009; Persson, 2013; Gallego, 2010; Singh, 2011a, 2011b).

Nevertheless, this study contributes to the turnout literature both empirically and theoretically. This is a novel approach to the study of closeness and voting, as very few studies of voter turnout even discuss or account for these types of conditional effects empirically or theoretically (with exceptions of course, e.g., Arceneaux and Nickerson, 2009; Gallego, 2010; Kittilson and Anderson, 2011; Söderlund et al., 2011). In addition, this thesis emphasize the importance of measuring closeness of election correctly. Especially how closeness varies also in systems with proportional representation when combing the district- and national-level margin of victory. None (to my knowledge) has examined closeness of election and turnout at the individual-level at this scale while using an appropriate measure of closeness. The results contribute to an empirical foundation and understanding for the role of closeness of election in shaping turnout. In addition, much of the existing literature on voter turnout is often underspecified theoretically by only using a single theoretical framework at a time (Smets and Ham, 2013). I show that combining different theories can help uncover the more complex ways in which different factors interact in shaping political participation. In this case, it especially highlights the role of *habit* in shaping the role of context, and the role of context in shaping the role of *interest*.

2. THEORY

In this chapter, I present the theory and hypotheses. I have tried to organize this chapter so that it follows my main argument about the inconsistency in the literature and the conditional effect of closeness in a logical manner. Firstly, to lay the foundation, I define *the closeness of an election*. After that, I briefly elaborate on the empirical inconsistency between aggregate- and individual-level studies regarding close elections. My point is not conduct a literary review, but to provide the facts behind this assertion. Thirdly, I present the theoretical framework. This is to clarify my rational and motive in approaching this subject theoretically. Lastly, I present each hypothesis in turn. My first hypothesis is about the role of habit. I present this first because, I argue, this contingent relationship is applicable to my other hypotheses. The next hypothesis proposes how the closeness of election might affect the decision to vote for those with a lot of interest in the election directly. My next two hypothesis uses the mobilization hypothesis (a common hypothesis in the literature on close elections and turnout) as background, and propose two different ways in which close elections might increase interest in the election. The last hypothesis is the most general, proposing that closeness of election should affect the decision to vote indirectly via interest in the election.

2.1 The closeness of an election

The closeness of an election is *the uncertainty of the outcome of the election prior to the actual results being known*. In other words, a close election is one where there is no clear winner at the point(s) in time when people have the opportunity to vote. It is an intrinsic component of the electoral competition; many scholars even call it the competitiveness of the election. Conceptually, we could say that it ranges from a lower bound where the outcome is the most certain and an upper bound where the outcome is the most uncertain. This is not unambiguous, however. Firstly, "uncertainty" can be the actual uncertainty of the election, i.e. how many votes that determined the outcome, or it can be the uncertainty from the perspective of a particular person, i.e. whether he or she perceive it to be close. Secondly, "outcome" can mean both the outcome for a particular party, i.e. how many seats they win in parliament (or in some specific constituency), or the outcome of the election as a whole, i.e. which party or coalition wins the majority and hence the executive. Hence, closeness vary by both by perspective and outcome (see Figure 1).
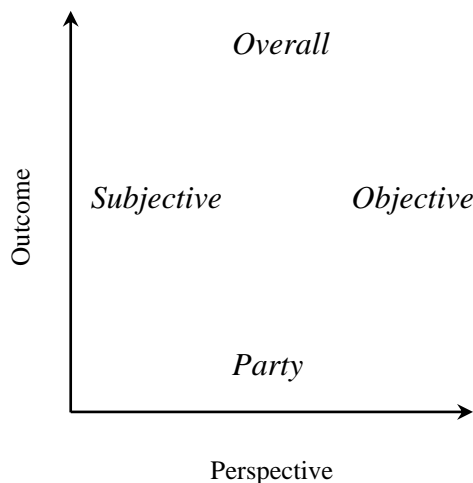
Figure 1: Different types of closeness of election

Perspective determines the scope of closeness. Objective closeness is the actual and numerically measurable closeness of election, while subjective closeness is how close voters perceive it to be. Earlier individual-level research do not distinguish between the two, and subsequently used the "self-reported" closeness (e.g. Riker and Ordeshook, 1968). I am interested in how the electoral context affects voter behavior. Closeness of election is a property of elections, which

is why it is of such interest: It is a property of the election that can vary a lot. The voter's perception of closeness—and how that perception alters their behavior—is only *a*, not *the* part of this. For example, the mobilization hypothesis assumes that closeness affects the decision to vote via indirect mechanisms (e.g. altering information costs and increasing social pressure) independent from ones individual perception of closeness. When using a subjective measure we assume closeness itself to be something affected by the context, instead of being a part of it. Even if that is of interest, an individual's perception of closeness is prone to be confounded by other variables of interest, like the level of information, social capital, perception of civic duty, and so on. Separating the effect of closeness from other thing would be difficult. In short, there are both theoretical and empirical reasons for taking an objective perspective when studying how closeness affects the decision to vote.

The "outcome" dimension specifies what part of the election that is actually close. The election can be very close for a specific party, e.g. because it is about to fall under or over the legal threshold of exclusion or lose or win a parliamentary seat, while the overall race for the majority (and hence executive) already is a foregone conclusion. The race for the marginal seat in a district can be very close between some parties, but not all. The unit of analysis in this case is voters, not parties. The intricate relationship between closeness of election and the decision to vote does not limit itself to the relationship between a voter and his/her favored party. On the contrary, it extends to several indirect effects imposed by the general context in which the decision is made. Even if the outcome of some voters favored party is very certain, the situation for other parties might be very different, both nationally and locally. Untangling and properly weighting the uncertainty for each party in relation to each other could yield an interesting measure, but it will come at the cost of complexity. A more parsimonious approach is to focus on the *overall objective* closeness of the election.

2.1.1   Measuring closeness across electoral systems

I have three general points on the measuring of electoral systems; (i) the data, (ii) the level of measurement and (iii) the operationalization. Firstly (i), somewhat paradoxically, measuring the actual objective uncertainty is impossible (because it is uncertain). The most intuitive proxy would be pre-election information like opinion polls, media coverage and previous election results. However, so-called *ex ante* information is difficult to attain and measure, especially at the district-level. There is virtually no alternative to using so-called *ex post* election information in comparative research. The virtue of post-election data is that closeness can be accurately

calculated. However, this measure will always be biased because it does not account for the changes in closeness caused by itself (or other things for that matter) right before the election. Still, for this analysis I use post-election data.

Secondly (ii), the overall outcome can be calculated either at the district- or national-level. It is most common to measure closeness at the national-level, but there have recently been an increase in studies arguing that district-level closeness is more suitable (e.g. Blais and Lago, 2009; Grofman and Selb, 2009; Selb, 2009; Franklin, 2004). Blais and Lago (2009), for example, argues that it is more appropriate to measure the race in the district because it is possible for the national race to be a foregone conclusion while parties are still fighting for a seat in the district. This is obviously correct. However, my argument is that closeness at the national-level *also* matters. The overall uncertainty of the election is a product of the level of closeness at both the district- *and* the national-level, no matter which outcome is in focus. An election is always the most close when both the district-level race and the national-level race is close at the same time. Conversely, an election is always the least close when both the district-level race and the national-level race is the most certain at the same time. I therefore measure closeness of election as the mean between the district- and national-level closeness.

Thirdly (iii), measuring closeness of election in a way that allows comparison across different types of electoral systems is no trivial endeavor, especially across different formulas. Franklin (2004), for example, simply code countries with proportional representation as "0", assuming they have perfect competition. The standard measure used in plurality election is the difference in votes (or vote share) between the winner and the runner-up. This have no self-evident equivalent in systems with proportional representation where several electoral seats are contested simultaneously. Recently, both Grofman and Selb (2009), Selb (2009) and Blais and Lago (2009) argue that closeness of election also varies within PR systems at the district-level, and that this is important. Grofman and Selb (2009) proposes an excellent measure of district competitiveness, but it is only applicable to d'Hondt systems. Blais and Lago (2009) proposes to measure district-level closeness as the minimal amount of votes required for any party to win one additional seat, but this would be very difficult to compute with the available data. Selb (2009), however, uses a compelling measure of the district-level margin in way that is comparable across different systems and relatively easy to compute: He uses the margin of victory between the winner and runner up exclusively for *the marginal seat*. In a plurality single member districts this is equivalent to the margin of victory between the leading and second

party, but it also have an intuitive counterpart in PR multi-member districts. Although he only applies it to election system using the highest fraction method (d'Hondt and Sainte-Laguë), I also adapt to systems using largest remainder (Droop and Hare quota) in order to have a wider sample. Although sophisticated measures of district-level closeness have been introduced in the literature over the recent years, there is no standard of cross-national measure of closeness at the national-level. The national-level margin is measured simply as the margin between the winning and losing bloc. That it is, the number of votes cast for the winning "bloc" minus the number cast for the loosing "bloc".

2.2 The inconsistency in empirical literature on close elections and turnout

Studying if closeness of election affects voter turnout is in no way a novel endeavor. Closeness is frequently regarded as the most studied element in the voter turnout literature (Geys, 2006: 647). In his 2006 meta-analysis of 83 empirical aggregate-level studies, Geys (2006) lists 52 that includes some kind of measure of closeness. In this section, I briefly discuss the inconsistency between individual- and aggregate-level studies on closeness of election.

The link between closeness and turnout is often considered as the most consistent finding in the empirical literature on voter turnout (Blais, 2006: 119; Geys, 2006: 647), to the point where some "cannot see how this finding could be wrong" (Blais, 2006: 119). Even some classic texts have emphasized this relationship. Key (1949), in his study of turnout in the Southern U.S during the early twentieth century, stress the role of closeness for increasing turnout. Several scholars since then have similarly attributed low turnout rates (e.g. Felchner, 2008; Kelley et al., 1967; Teixeira, 1992; Wattenberg, 2002), especially declining rates over time (e.g. Burnham, 1965; Franklin, 2004; see also Blais and Rubenson, 2013), to the lack of closeness. In their meta-analyses of aggregate-level research, Geys (2006) and Matsusaka and Palda (1993) finds that 69 and 70 percent of the studies they analyze, respectively, found a positive relationship between closeness and turnout (see Table 2). More recently, for example, several studies have used elections with a two-level ballot system to test closeness more accurately (Indridason, 2008; Garmann, 2014; Simonovits, 2012; Fauvelle-Aymar and François, 2006; De Paola and Scoppa, 2013). By using district-level data from the first round to measure the expect closeness in the second round, they all find this same tendency. In all aggregate-level studies, however, the magnitude of the effect is always found to be quite small. Blais and Dobrzynska (1998), in a very renowned study of aggregate-level turnout, suggests that a ten point change in the percentage points between the winner of the election and the runner up only increase turnout by one or two points.

However, the story is very different in individual-level research. Only 35 percent of the individual-level studies using a national-level measure of closeness in Smets and Ham (2013) meta-analysis finds the same relationship. *None* of the individual-level studies using district-level measures of closeness found any link. This tendency for individual-level studies to come up short on the matter have led many to question the validity of aggregate-level findings. Matsusaka and Palda (1993) suggests that the findings at the aggregate-level is simply due to

ecological fallacy (Cho et al., 2008; Robinson, 1950). Other scholars have pointed out problems related to how closeness of election often is measured. Cox (1988), for example, argues that when closeness is measured by percentage margins at the national-level, part of the turnout measure (i.e. dependent variable) appears in the closeness measure (i.e. independent variable), since both is partially calculated from the total number of cast votes. This, he argues, means that any correlation found might be spurious.

Table 1: Comparison of aggregate- and individual-level studies on close elections and turnout.

Study and scope	Level of analysis		Variable	Successes/ Failures/ Anomalies	Success rate (%)	Effect size (r_{av})	Modal category
	Turnout measure	Closeness measure					
Geys (2006), 1968-2004	Aggregate-level	Either	Tests (343)	206/137/19	56.91	0.58*	Success
			Studies (52)	36/16/-	69.23	0.69*	Success
Matsusaka and Palda (1993), 1973-1989			Tests (49)	35/9/3	71.43	-	Success
			Studies (23)	16/6/1	69.57	-	Success
Smets and Ham (2013), 2000-2010	Individual-level	National-level	Tests (51)	15/36/0	29.41	0.29***	Failure
			Studies (20)	7/13/0	35.00	0.36**	Failure
		District-level	Tests (13)	0/13/0	0.00	0.00	Failure
			Studies (3)	0/3/0	0.00	0.00	Failure

*** $p<0.001$; **$p<0.01$; *$p<0.05$.
Note: The tested hypothesis is a positive relationship between closeness and turnout (i.e. a closer election generates higher turnout). Success rate = (successes/number of tests)*100. A test is an anomaly if the coefficient is statistically significant but in the opposite direction of the hypothesis. Mean effect size (r_{av}) = sum of r_i /number of studies where r = (successes – anomalies)/number of tests. Modal category is "success" if the majority of tests are a success and "failure" otherwise. See sources for more details.
Source: Meta-analysis data of aggregate-level studies are from Geys (2006: 646) and Matsusaka and Palda (1993: 858-859), and meta-analysis data of individual-level studies are from Smets and Ham (2013: 13).

There was few individual-level studies examining the relationship between closeness and turnout prior to the introduction of multilevel regression models. Those that exists, e.g. Riker and Ordeshook (1968), Ashenfelter and Kelley Jr (1975) and Blais (2000), uses subjective measures based on survey questions. These studies typically find a statistically significant relationship between closeness and turnout, but this type of measure problematic because it does not measure the effect of closeness per se (see 2.1). District-level measures of closeness (with individual-level units of analysis) were introduced by Matsusaka and Palda (1993, 1999), but they use inappropriate statistical models.

2.3 Theoretical framework: Closeness and horseshoes

Almost 40 years ago, Ferejohn and Fiorina (1975: 920) declared that "closeness counts only in horseshoes and dancing". Indeed, this seems like a reasonable explanation for the aforementioned inconsistency.

When does closeness counts?   <   Horseshoes!

                                    <   Dancing!

A problem with much of aggregate-level literature on closeness of election and turnout is how it relates the context with the individual. The underlying model of behavior behind some of these studies implicitly assumes a direct macro-micro link (e.g., Franklin, 2004; Powell Jr, 1986; Jackman, 1987). For example, Franklin (2004; 1996: 321) explicitly argues that the institutional context constitute the boundaries within which individual-level characteristics can play a role. The presumption is that the context *directly* shapes the costs and incentives of participation. In some sense, this amounts to a linear and deterministic ontological view of political behavior. The supply- versus demand-side factor framework of Kittilson and Anderson (2011), on the other hand, seems like a more promising approach. When analyzing the relationship between efficacy and voting they argue that electoral institutions works as supply of *opportunity*, shaping the costs and benefits of participation (Kittilson and Anderson, 2011). They separate between the *contingent* and *indirect* effect of the electoral supply on political behavior. Instead of weighting macro- and micro-level factors against each other, one should examine how supply- and demand-side factors interact in shaping political behavior. In other words, instead of asking *if* closeness of election affects the decision to vote, we should ask *how* and *when*. Or in this case, I ask for *whom*. So to understand close elections and turnout, all we need to do is to find the horseshoes. This is my first point of departure. *For whom does closeness count*?

The theoretical literature on political participation is vast and takes on different philosophical approaches to human social behavior. Albeit the extensiveness of this literature, there still exists no core theory of voter turnout. In its stead, we have many different theoretical explanations, all with some degree of empirical support. As Smets and Ham (2013: 2) notes, this "[…] points to the possibility that multiple causal mechanisms explain turnout and that different causal mechanisms may be prominent for different voters or in different contexts." This is my second

point of departure. Instead of applying the "usual models" of turnout (i.e. some form of rational choice theory), or try to come up with an all-encompassing master explanation of this phenomena, I test five hypothesis for when closeness might count. More specifically, I lean on a social-psychological approach to political participation. I try to combine the developmental framework of voting, the heuristic-systematic model of information processing and the resource model of participation. I also rely on the mobilization hypothesis already proposed in the literature. Each hypothesis is designed so that they rely on different mechanisms, while the theories still overlap. My rationale is this: If closeness of election affects the decision to vote via different mechanisms for different people, then we should find empirical support for different mechanisms for different people. The central theme is how closeness of election interacts with *habit* and *interest*. In the rest of the chapter, I present the hypotheses I derived from taking this approach in turn.

2.4 Close elections and the habit of voting

Any effect of closeness of election on the *act* of voting is conditional on the habit of voting. The concept of habit can broadly be defined "as psychological dispositions to repeat past behavior" (Neal et al., 2012: 492). The important role of habit in the context of turnout have long been stressed (e.g. Milbrath, 1965; Brody and Sniderman, 1977). In a very famous article in the turnout literature, Plutzer (2002) outlined what he called a *developmental framework* for understanding turnout. He argued that much of the mixed results in the literature would make more sense when considering the development of the habit of voting. Key to this framework is the idea of *inertia*: the longer citizen's do or do not vote repeatedly, the higher propensity they have for settling into the habit of doing the same next time around. This idea of voting or not in the past itself increasing the probability of voting again in the future have been tested many times (Kanazawa, 1998, 2000; Green and Shachar, 2000; Gerber et al., 2003; Fowler, 2006; Cutts et al., 2009; Denny and Doyle, 2009; Meredith, 2009; Dinas, 2012; Fujiwara et al., 2013). It is also the backdrop of Franklin's (2004) argument about the importance of the electoral context. He argues that the electoral supply is one of the most important factors for young people that have not yet gained the habit of voting. Hence, for him it inadvertently affects *if* they get the habit of voting. The shifting temporal trend of aggregate-level turnout is then merely the sum of the proportion in each new generation that gained the habit of voting, which varies as the context varies.

However, I think this idea of inertia where also *not* participating increases the propensity to not vote next time, is somewhat misguided (Aldrich et al., 2011; Cravens, 2013). In social-psychology the idea of habit is more often described as the development of automaticity based on learned associations between context and responses (Ouellette and Wood, 1998; Verplanken et al., 1997; Verplanken and Aarts, 1999; Aarts and Dijksterhuis, 2000). The development of habit is a gradual process. It requires repetition over time within the same context for the association between context and response to form in memory (Wood and Neal, 2007, 2009; Neal et al., 2012). For rare events, like blood donation (Masser et al., 2008), habit has been linked to identity-related factors (Verplanken and Orbell, 2003). Voting becomes habitual over time as it is associated more and more with psychological rewards and becomes more central to one's identity (Verplanken and Orbell, 2003). For example, the increased content and pride from complying with social norms, and being able to call oneself "a voter". In addition, once someone overcomes their initial predisposition to stay home, the inertia increases substantially

initially because of the decreased cost. The more often someone vote the more familiar the process will be. For example, uncertainty around and effort to get information about the parties, how to register, where to meet, how the process at the voting booth works and so on, is lower. This means that the anxiety for going to the polls is much lower. However, *not* voting have no such mechanism. The cost remains the same and there is no association of psychological reward or identity that can be triggered in memory. In other words, the habit of voting is a gradual process *for those that do vote*.

For those that do have a (degree of) habit of voting, when the election comes, the response of voting is likely to be activated, and other responses deactivated (Mc Culloch et al., 2008). From this, people may act on the first response that comes to mind as an ideomotor effect (Bargh, 1999). In other words, a habitual voter may vote simply as a reflex, without considering the act and then make a decision. It is not that a habitual voter necessarily does it on pure reflex per se; they may consciously decide to override the idea and do something else. However, making novel decision always require more effort because the already established pattern of responses must be overwritten (Quinn et al., 2010; Neal et al., 2006). In other words, by having voted repeatedly in the past they may vote again, simply because it is easier than making a decision not to. The habit of voting therefore work as a "buffer" for decision-making. The higher the degree of habit, the less likely it is that the decision is a consciously evaluated one. This why the notion of *inertia* fits very well, once we only regard inertia for those that *do* vote. The propensity for these mechanisms to work in this way would always rise as the electoral experience rises, so it is difficult to set any definite dividing line for when someone is a habitual voter. Even Milbrath (1965: 31) in his seminal work regarded it is as concept of reinforcement, where *the strength of habit* characterize the habitual voting.

2.4.1 Hypothesis 1: The habit of voting as a buffer for the decision to vote

In order for habit to form, a stable context is need. In this sense the context is crucial to the developmental model of turnout. Closeness of election might play an import role in this regard, as Franklin (2004) argues. However, I am not interested in the habit-formation per se. I am interested in *when closeness matters*, or more specifically, for *who*. Because the habit of voting can levitate the decision to vote, this is a natural place to start. My central hypothesis, then, is that closeness only counts for people without the habit of voting. In other words, I do not regard the electoral supply as the one shaping the incentives and cost of participating, but rather the

psychological boundaries as the one shaping the role of the electoral supply. Again, this relationship is of course much more complicated. Habit-formation's most heavy dependence is the performance context, of which also closeness can play a role. However, whom closeness can make a difference for when it comes to voting, are primarily those without any predisposition do so regardless. Causally speaking, I expect the habit of voting to moderate the relationship between closeness of election and voting, i.e. a contingent effect. Hence, I formulate the hypothesis as:

Hypothesis 1 *The habit of voting moderates the effect of closeness of election on voting*

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           ┌──────────┐
           │  Habit   │
           └──────────┘
                │
                ▼
┌────────────┐      ┌──────────┐
│Closeness of│─────▶│  Voting  │
│  election  │      │          │
└────────────┘      └──────────┘
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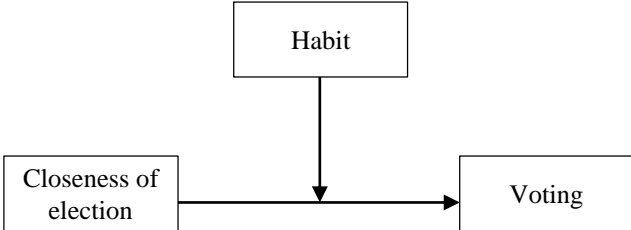
Figure 2: Hypothesis 1, the effect of closeness of election is moderated by habit

2.5 Close elections and cognitive engagement

Individuals with an interest in the election should invest more cognitive effort in a closer election because of accuracy-motivation induced by uncertainty. I would argue that the level of uncertainty in the election should affect the way citizen's process election-relevant information at a cognitive level. In short, they should be more cognitively engaged. Consider the heuristic-systematic model (HSM) of information processing in social psychology (Chaiken, 1980, 1987; Chaiken et al., 1989; Chen et al., 1996; Chen and Chaiken, 1999): The HSM model maintains that people develop their opinions and beliefs by using either or both *systematic* or *heuristic* ways of processing information. *Systematic* processing is the use of decision-relevant information. When using systematic processing, individuals are more *cognitively engaged* because they carefully analyze the available information in order to make up their mind about something. It involves the detailed analysis of structure and quality of an argument. Systematic processing therefore require a lot of cognitive effort. *Heuristic* processing, on the other hand, requires much less effort. When using heuristic processing, individuals process the information quickly based on so-called judgmental rules—the set of knowledge structures that are already stored in memory—and assign a judgment. These judgmental rules are predetermined based on the conclusions that have worked in past; like cognitive shortcuts developed from prior experience which easily can be used to evaluate new information (Smith, 1984). In this sense, heuristic processing requires little attention to the actual incoming contextual information. Judging the information—what party to vote for, which party will win, and so on—does not require any complex thought, because individuals simply assign it the same meaning as they had before without investing any effort.

What induces heuristic versus systematic processing? Firstly, all-else equal, individuals will try to exert the least amount of *effort* making a decision or forming an opinion (Shugan, 1980; Cacioppo et al., 1996; see also e.g., Lupia and McCubbins, 1998; Basinger and Lavine, 2005; Lavine et al., 2012). Secondly, individuals will want to be *sufficiently* confidence in that they have made the right decision (Fiske and Taylor, 2013: 15; see also e.g., Basinger and Lavine, 2005; Lavine et al., 2012). In other words, people will tend to use heuristic processing as much as possible, unless they for some reasons feel the need to be more certain. Why should closeness of election matter for people *with interest in the election*? Because the uncertainty of the election should alter the degree of confidence needed for the decision to be *sufficient*. This is the *accuracy-motivation*: Individuals will engage in the necessary cognitive processing in order to

be sure they made the right decision (or opinion) in an uncertain situation. The notion that individuals are drawn to resolve uncertainty is not a new one (e.g., Kagan, 1972), and uncertainty is linked to increased systematic processing in many ways (e.g., Weary and Jacobson, 1997; Loewenstein, 1994; Tiedens and Linton, 2001). In elections that are not close, most individuals that are interested in the election probably do not engage in much systematic processing, and simply rely on heuristics for most incoming information. When the race is close, however, because it is *uncertain*, it should have a tendency to fuel more systematic processing and hence more cognitive effort.

Consider a sport example, which is more intuitive: People consume sport for many reasons, including team affiliation, social facilitation, self-actualization and so on (Milne and McDonald, 1999: 23-26). For example, someone watching a football game with interest. Their interest is high and they indeed sought out information. However, the degree of *cognitive engagement* will still vary depending on the game. Crucially, how much cognitive effort that is exerted in analyzing and making up opinions during the game will vary depending on how exciting and uncertain it is. For example, most people will probably exert more cognitive effort in an exciting game on overtime, than in one that is a "blowout" from start.
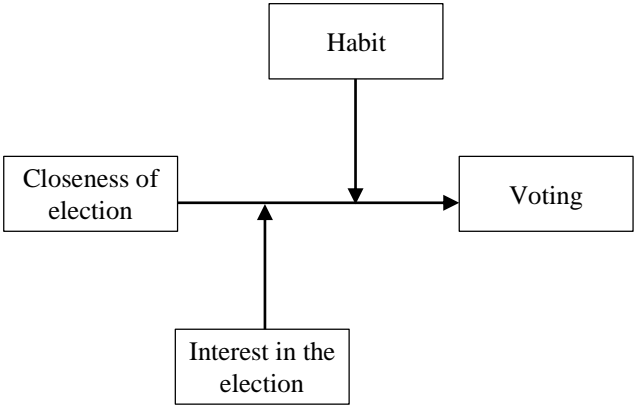
Figure 3: Hypothesis 2, closeness of election affects voters that are interested in the election and that does not have the habit of voting

2.5.1 Hypothesis 2: Uncertainty and those interested in the election

My second hypothesis builds on this framework in a simple way: Closeness of election should have an impact on those that already have shown an interest, e.g. sought information about the election. This is because, on average, they should have invested more cognitive effort in a closer

election. The assumption is that those who already have an interest in the election—for whatever reason—have a high propensity to vote in the first place (see e.g., Rubenson et al., 2004; Denny and Doyle, 2008). The extra effort and excitement because of the closer election should be enough to make those that otherwise would not vote overcome their initial inclination to stay home. In addition, the habit of voting should still act as moderator. Both regarding whether a closer election translates into more cognitive effort, and whether more cognitive effort translates into a higher probability of voting. I therefore expect the effect of closeness on turnout to be moderated by both information seeking and habit, i.e. a contingent effect (Figure 3). Hence, I formulate the hypothesis as:

Hypothesis 2 *Closeness of election affects the decision to vote for people that are interested in the election and do not have a habit of voting*

2.6 The mobilization hypothesis and interest in the election

The primary hypothesis proposed in the literature is the mobilization hypothesis[1]. It argues that closeness of election affects elite actors' incentive to mobilize, which in turn affect turnout. In other words, although it argues that the effect is indirect via mobilization, it also argues that there is a direct relationship from mobilization to the decision to vote. I will first present the mobilization hypothesis. This is important because in hypothesis 3 and 4 I assume it to be "true". After that, I present my hypothesis for *who* this should matter for. My first argument is that closeness of elections—in light of the mobilization hypothesis—should primarily matter for *interest in the election*. Therefore, much of the effect of closeness of election on turnout should be (more) indirect.

The mobilization hypothesis states that closeness of election increase mobilization, which in turn increase turnout (see e.g. Cox, 1999; Denver and Hands, 1974; Key, 1949; Kirchgässner and Schulz, 2005). In other words, closeness of election does not affect voters, but rather "pivotal elites" (Cox, 1999; Cox and Munger, 1989), "pivotal leaders" (Shachar and Nalebuff, 1999) and/or "strategic politicians" (Aldrich, 1993). My interpretation is as follows: Closeness of election can both *increase* and *focus* partisan mobilization efforts, which in turn increase turnout by potentially reducing information costs, promoting interest in the campaign and increasing social pressure. The first assumption of this is that *closeness affects mobilization*. This, in turn, reasonably assumes that elites (e.g. politicians) are inherently strategic in their mobilization efforts, and that they adapt to the nature of the competition (see e.g. Rosenstone and Hansen, 1993). One might therefore expect closeness of election to affect *elites*, because they will perceive mobilization efforts to have a higher probability of being important in a close election. This means that closeness can affect partisan mobilization efforts in two different ways, a distinction the existing literature usually fails to make. Firstly, close elections might drive parties to *increase* the pool of resources available in the campaign, i.e. increasing fund-

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[1] The "decision" hypothesis is the other standard hypothesis proposed in the literature. It simply states that people are more prone to vote in close elections because their chances of affecting the outcome is greater. In other words, it suggests a direct relationship between closeness and turnout. This notion comes from the rational choice approach to voting where the instrumental utility gained from voting is weighted by the probability to affect the outcome (Downs, 1957; Riker and Ordeshook, 1968). The inherent problem associated with this hypothesis is that the probability of affecting the outcome in a close election is null (Gelman et al., 1998; Mulligan and Hunter, 2003; Owen and Grofman, 1984), so people should not vote. However, an interesting approach is that this is true because people miscalculate their chances by being overly confident in their assessment. This conforms to the concept of "illusion of control" within social-psychology (Langer, 1975; Presson and Benassi, 1996).

raising efforts, borrowing and the use of politician's personal financial resources. For example, financial contributors might be persuaded to give more either because their chances of affecting outcome is greater or because parties and candidates are willing to promise more favors in return (Cox and Munger, 1989; see also Denzau and Munger, 1986). This overall increasing of resources would be driven by the overall level of closeness in the election, i.e. the closeness of the national (election-level) race. Secondly, close elections might influence where and how the existing pool of resources are being allocated, i.e. how the mobilization efforts are *focused* (Bartels, 1985; Shaw, 1999, 2008).
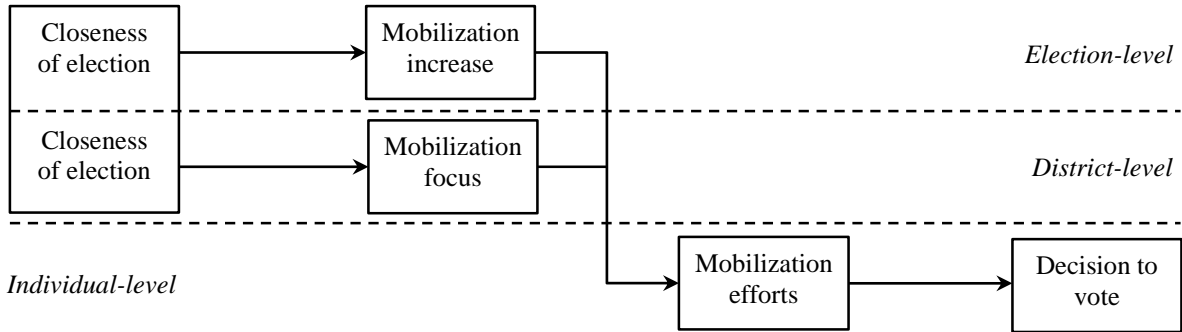
Figure 4: A multilevel interpretation of the mobilization hypothesis

The allocation of resources should be driven by the relative degree of closeness in different areas, i.e. district-level closeness, because parties seek to allocate more resource in areas where the outcome is uncertain (and hence mobilization can be more decisive). Generally, then, one would therefore expect an overall increase in mobilization efforts in a closer election. Indeed, several studies of U.S. elections have found that campaign activity (Patterson and Caldeira, 1983; Shachar and Nalebuff, 1999; Hill and McKee, 2005; Gimpel et al., 2007), campaign donations (Ansolabehere and Snyder Jr, 2000; Erikson and Palfrey, 2000), media coverage (Clarke and Evans, 1983; Jackson, 1996) and mobilization of elites (Cox and Munger, 1989) increases in a close election.

The second assumption of this hypothesis is that *mobilization affects turnout*, an area of study that have received considerable scholarly attention. Increased mobilization efforts is expected to increase participation for several reasons (Rosenstone and Hansen, 1993). Firstly, more information about the parties and the election becomes available, and people are more likely to

be (intentionally or unintentionally) exposed to the campaign, e.g. by increased media coverage or direct contacting by parties, which decrease the effort and time needed to attain information about the election or the parties. Secondly, this increase in exposure to the election is similarly likely to generate higher interest in the election in general. Thirdly, parties are likely to target their mobilization strategically, e.g. using existing networks (like unions and churches) or people centrally positioned in social networks (like business leaders or the wealthy and well educated). This, in turn, can increase the social pressure to vote. Although the research primarily focus on U.S.-elections, the link between mobilization and turnout have found wide support in the literature. Studies on the effect of campaign spending (Caldeira and Patterson, 1982; Caldeira et al., 1985; Cann and Cole, 2011; Cox and Munger, 1989; Grier and Munger, 1991, 1993; Jackson, 1997, 2002; Patterson and Caldeira, 1983), direct contacting (Abramson and Claggett, 2001; Gerber and Green, 2000; Goldstein and Ridout, 2002; Huckfeldt and Sprague, 1992; Kramer, 1970; McClurg, 2004; Rosenstone and Hansen, 1993; Wielhouwer, 1999, 2003; Wielhouwer and Lockerbie, 1994), political advertising (Ansolabehere and Iyengar, 1995; Franz et al., 2008; Krasno and Green, 2008; Nagler and Leighley, 1992; Sigelman and Kugler, 2003; Wattenberg and Brians, 1999) and "demobilization" campaigning (Berelson, 1954; Converse, 1962; Zipp, 1985) generally tend to find that increased mobilization efforts increase participation.

2.6.1 Hypothesis 3: Mobilizing the interest of educated individuals

Here I build on the resource model of participation. The resource model—also called the civic voluntarism model—centers on a socio-economic account of participating, where different types of resources determine the likelihood of participating in elections (Brady et al., 1995; Nie and Verba, 1987; Verba et al., 1995). As Brady et al. (1995: 271) famously argued, people *don't* vote because they can't, because they don't want to, or because nobody asked. In other words, people need the right amount of resources, interest and mobilization in order to vote. High-resource actors already have a high propensity for voting. However, even if people are rich in resources, they will not participate if they are not aware of the importance of their involvement or "no one asked" them to do so (Brady et al., 1995: 271).

If we use the resource model and ask, "for whom does closeness count?" with the mobilization hypothesis in mind, the most intuitive answer is *educated individuals*. It counts because it fuels

their interest in the election. Education is important because it is considered a key indicator of both access to information and civic skills, and an ability for processing political information (Dalton, 2008; Brady et al., 1995). Following the mobilization argument above, we assume that politicians and parties target their mobilization (Rosenstone and Hansen, 1993). Specifically, they strategically mobilize those that have the highest probability of providing benefit. Educated individuals are a key group because they are the most likely to respond to mobilization (Brady et al., 1995; Rosenstone and Hansen, 1993). In addition, if we sum the indirect consequence of increased mobilization in a close election, it also includes increased exposure to the election via media coverage and social network effects. For example, informational contagion (Burt, 2000) and behavioral contagion (Kenny, 1992), where those already mobilized by a party or candidate influence those around them. Informational contagion is influence by those already mobilized by them increasing the level of politically-centered conversation and alter its content, while behavioral contagion is their influence by increasing their political involvement (McClurg, 2004). These factors should affect educated individuals more because of their larger recruitment network, increased civic skills and ability to process political information (Verba et al., 1995: 376; Brady et al., 1995).
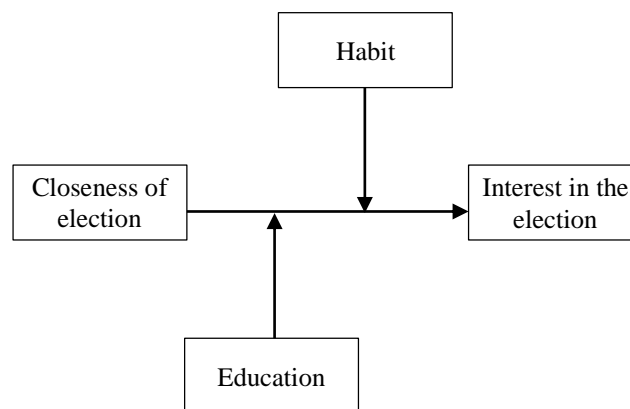
Figure 5: Hypothesis 3, closeness of elections affects the interest in the election for educated individuals without a habit of voting

In addition, this should only matter for those without a habit of voting. Once the election comes, the response of voting is likely to be activated and other responses deactivated. This automaticity and response should hold for most aspects of the election. It is not that a habitual voter will or will not be interested in the election, but a closer election—despite the increase in mobilization and election coverage—should not change the already established pattern for most

people. I therefore hypothesize that the effect of closeness of election is moderated by both education and habit:

Hypothesis 3 *Closeness of election affects the interest in the election for people with higher education that does not have a habit of voting*

2.6.2 Hypothesis 4: Cueing partisan loyalty

Closeness of election should also affect the interest in the election for people that feel close to particular party. Not because of direct or indirect mobilization per se, but because of the increased exposure to the election and media coverage it creates, close election triggers group-based loyalty. Kam and Utych (2011: 1252) argues that "[c]lose elections may trigger group-based loyalties and thus cognitively engage citizens because they want to "root for their team"". I reiterate the same argument. This also fits into the heuristic and systematic information-processing model. Rather than processing the election information through accuracy-motivated heuristic or systematic processing, these voters may be driven by impression-motivated processing (Chen et al., 1996: 46; Chen et al., 1999; Kam and Utych, 2011). This type od information processing are focused more on determining what decisions and opinions will satisfy the current *social goal*. Also, for same reasons as in the last section, it should be conditional on the habit of voting. My hypothesis is therefore that closeness counts for those close to a party and without the habit of voting:

Hypothesis 4 *Closeness of election affects the interest in the election for people that feel close to a political party and that do not have a habit of voting*
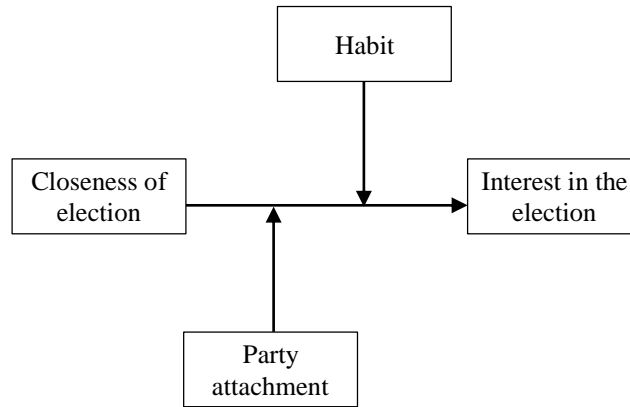
Figure 6: Hypothesis 4, closeness of elections affects the interest in election for people that feel close to a party and does not have a habit of voting

2.6.3   Hypothesis 5: Close elections and voting through interest

Interest in the election should mediate the relationship between closeness of elections and voting. Of course, this relationship is more complicated underneath (as discussed above). However, closeness of election should affect both the level of interest in the election, and the propensity to vote for those that already are interested. I therefore also expect that—on average—closeness of election should affect the decision to vote indirectly through interest in the election.

Hypothesis 5 *Closeness of election affects voting through interest in the election*
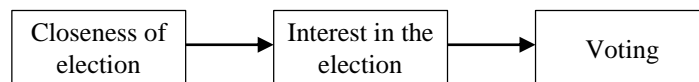
Figure 7: Hypothesis 5, closeness of election affects voting indirectly through interest in the election

3. DATA

Because of the scope and comparative nature of the data applied in the analysis, a comprehensive overview of the data is important. This chapter presents an overview of the data and measurements used in the analysis. I first provide an overview of data and then introduce the variables. In section 3.3 I describe in more detail how I calculated closeness of election.

3.1 Overview of data

The unit of analysis is voting-aged individuals. However, the aim of this study is to infer on how closeness of election affects the decision to vote comparatively across different types of political systems. In order to examine both supply- and demand-side factors simultaneously, I base the analysis on both micro- and macro-level data. I use data from Module 3 of the Comparative Study of Electoral Systems (CSES), which is excellent for this purpose. The CSES is a collaborative project that provides post-election survey data in addition to relevant contextual data about the elections and the political system. This includes the district-level data necessary to calculate the margin of victory. CSES Module 3 tracked elections in 41 countries between 2006 and 2011, and subsequently includes data from 50 elections. After removing missing data—including districts without enough election-data to calculate the margin of victory)—I am left with 35,913 (eligible) respondents in 1444 district-elections in 26 countries. This covers a total of 31 elections. I restrict the analysis the legislative (lower house) elections. I provide a descriptive overview in Table 2. The sample of countries spans several types of electoral- and party-systems. The CSES is a unique resource for the comparative study of electoral behavior in this way, because of the wide scope and inclusion of both micro- and macro-level data. However, it have been criticized for not having standardized rules on translation and not insisting on random sampling (Curtice, 2007: 902).

My measure of closeness of election is a combination of both district- and national-level margin. Hence, it varies by district. Because I for some countries have more than one election (Mexico, Finland, Iceland, Norway and the Netherlands), the second level in the multilevel models are *district-election*. Meaning that for these countries, some districts are parted in two clusters (because there are data from two separate elections for the same district). A sufficient number of clusters are necessary for the estimation of the variance component of multilevel models to be accurate, especially when several levels are included. Rabe-Hesketh and Skrondal (2008: 62) recommends the use of at least 10 to 20 clusters for a multilevel random effects model. The multilevel regression models used in this analysis includes 1444 districts-elections at the second level within 26 countries at the third level, which should meet this requirement.

Table 2

Overview of the elections included in the analysis

Electoral System			Country	Year of election	*N* of Resp./ Districts	Median/SD of Margin of victory	National-level Margin	National-level Turnout
Type	Allocation Method							
Majoritarian and mixed systems	Plurality	Single Majority Districts	Canada	2008	1251/209	15.9/7.89	11.39	58.8
			Germany	2009	1,836/208	16.75/4.09	25.4	70.78
			Japan	2007	945/35	7.35/4.19	7.8	58.64
			Mexico	2006	1,420/83	5.96/5.81	.58	63.67
				2009	2,255/122	14.31/4.97	18.8	44.8
			New Zealand	2008	914/70	16.35/6.67	13.82	79.46
			United States	2008	1,364/94	15.7/10.87	10.6	74.4
	Majority	Run-off	France	2007	1,838/187	10.83/5.47	10.02	64.42
		Alt. Vote	Australia[a]	2007	1,164/147	10.31/4.47	5.4	94.76
Proportional Representation	Highest Quotient	D'Hondt	Chile	2009	26/2	1.48/.06	.90	87.67
			Croatia	2007	533/6	1.26/1.15	2.2	63.41
			Czech Rep.	2006	1,680/14	2.38/1.45	3.89	64.42
			Denmark	2007	1,216/10	0.76/2.6	1	81.77
			Finland	2007	564/7	10.00/1.02	18.8	65.02
				2011	1,244/14	1.98/0.45	3.3	67.4
			Iceland	2007	1,377/6	18.82/1.77	32	83.62
				2009	1,226/6	3.1/1.10	3.72	85.12
			Poland	2007	1,337/36	5.46/1.47	9.4	53.88
			Slovakia	2010	1,125/1	4.72/0	9.41	58.97
			Turkey[a]	2011	581/40	15.13/4.33	23.85	83.16
		Modified Sainte-Laguë	Latvia	2010	544/5	5.08/.64	9.74	63.12
			Norway	2005	1,844/18	1.06/0.76	1.1	77.78
				2009	1,701/19	2.65/2.22	3.1	76.4
			Sweden	2006	1,037/28	1.48/1.97	2.2	81.99
	Largest Remainder	Droop Quota	Slovenia	2008	838/8	1.47/2.07	1.19	63.1
		Hare Quota	Brazil[a]	2010	187/5	15.05/0.06	30.10	82
			Estonia	2011	621/9	3.38/2.53	1.9	70.92
			Greece	2009	916/52	3.79/1.78	4.81	70.92
			Hong Kong	2008	105/1	14.37/0	23.34	45.2
			Netherlands	2006	2,153/1	1.22/0	2.44	80.35
				2010	1,971/1	0.43/0	.86	75.4

a. Compulsory voting (enforced)

Source: Module 3 of The Comparative Study of Electoral Systems

Note: All data are for elections to the lower house of the legislator. The "margin of victory" is the combined measure (see 3.3).

3.2 Dependent variable: Voting

The main dependent variable for this thesis is whether the respondent cast a ballot. The variable is coded as binary variable with 1 representing that the respondent did cast a ballot and 0 representing that the respondent did not. It is constructed from item Q21 of the CSES survey module (CSES Module 3). They do not have any standardized wording, so the phrasing of the question vary from country to country. Of the 35,913 respondents included in main analysis, 4,447 reported that they did vote (12.4 percent).
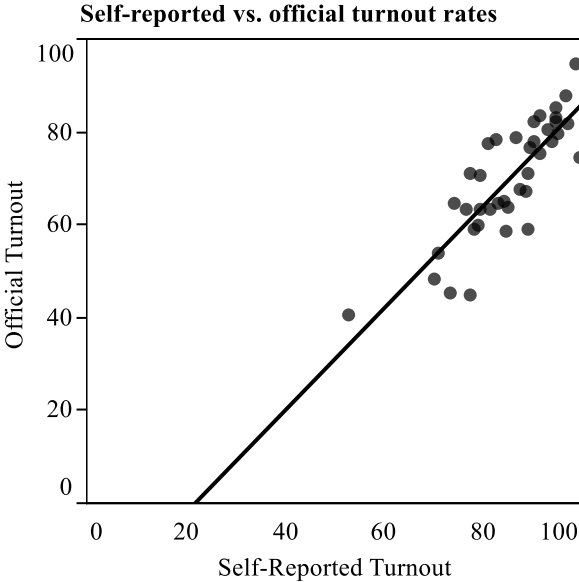
Figure 8: Actual vs. self-reported turnout for the data used in the analyses

Because it is only based on a survey question it is *self-reported*—rather than actual—turnout. Using self-reported turnout is not without caveat. There are notable problems related to using self-reported voting because of non-response bias (none-voters are less likely to respond to a questionnaire) and misreporting (respondents claiming to vote when they didn't) (Fieldhouse et al., 2007: 801). Karp and Brockington (2005: 825) estimate that, during the 1990s, the gap between self-reported turnout and official turnout in the American National Election Studies (ANES) was over 20 percentage points. Using self-reported turnout can therefore quickly lead to validity problems. While most studies of over-reporting focus on individual-level problems of social desirability bias (e.g. Bernstein et al., 2001; Silver et al., 1986; Granberg and

Holmberg, 1991; Holbrook and Krosnick, 2012), recent studies have emphasized contextual factors (Górecki, 2011; Karp and Brockington, 2005). More specifically, elections that are high-salience may generate higher over-reporting rates (Górecki, 2011: 544). This might be a particular problem for this analysis because the institutional setting that facilitates a high-salience (or "high-stake") election is the primary focus of the analysis (see Franklin, 2004: 44). I plot the aggregated self-reported turnout against actual turnout rates in Figure 8 for the data used in the analysis. Each dot represents a country and the black line is a regression line. There is very little deviation from the trend (R^2=.69; std. error=7.2; p<.001). The intercept, however, is at 24.1. In other words, although the relationship between self-reported and actual turnout rates are stable across the sample, it is stable at about 20 percentage points above actual turnout rate. This is about the "usual" rate of turnout misreporting in such surveys. It does emphasize the inherit problem with analyzing voter turnout at the individual-level, albeit there is little to do to correct it. An alternative to survey data is validated voting data, but they are very hard to come by and never includes as much information. The problem of over-reporting bias is a chronic feature of the individual-level turnout research from which I can do nothing to exempt from.

3.2.1 Information seeking (as a measure of interest in the election)

In order to measure the degree of interest in the election I use *information seeking*. Although interest can take many forms, the amount information the respondent sought should be a perfect measure. This is because it measures how much actual *attention* was given to campaign, and at the same time, it separates between the general notion of interest in politics and the specific interest in the election. I created the *information seeking* variable from a CSES survey question phrased the following way: "How closely did you follow the election campaign? Very closely, fairly closely, not very closely, or not closely at all?". It is therefore a four-valued scale from one to four. About 9 percent reported they followed the campaign "not closely at all", 34 percent "not very closely", 42 percent "fairly closely" and 15 percent "very closely". Some consider it problematic to use measure of interest in the election

3.3 Calculating the closeness of an election as the margin of victory

I use post-election data in order to calculate closeness of election. It is comprised of the percentage point *vote share* margin of victory at both the district- and national level. The measure of closeness I use for the regression analyses is thus a combined measure: The closeness of an election is the mean of the national- and district-level margins. Because it varies by district it is a district-level variable. For the regression analyses, *I reversed the scale so that a higher number means it is a closer election.* In general terms, I refer to the theoretical concept—i.e. the uncertainty of an election—as "closeness of election", and the measure I use for this as the "margin of victory". My combined measure can in theory range from near zero (most close) to one hundred (least close). The least close election in my sample is fifty. Hence, the scale ranges from (near) zero to fifty. I have plotted the distribution density for the scale in Figure 9. In the regression analysis—where the measure is reversed—the closest elections (with a margin near zero) is about fifty.
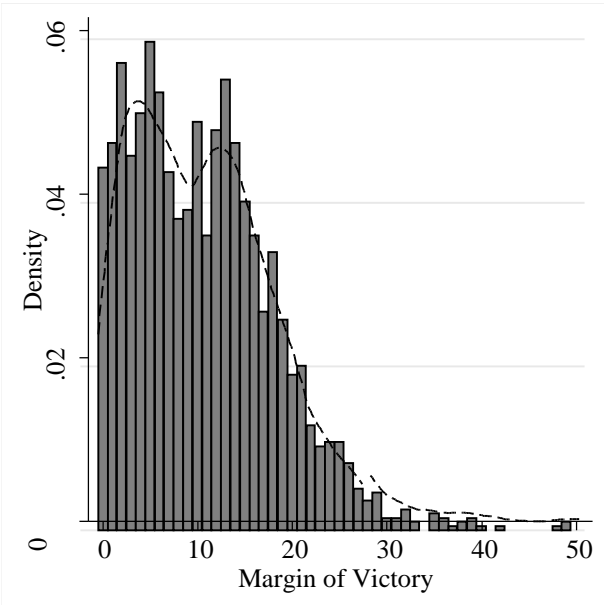
Figure 9: Histogram (density plot) of the combined margin of victory

3.3.1 National-level margin of victory

Although sophisticated measures of district-level closeness have been introduced in the literature over the recent years, there is no standard of cross-national measure of closeness at the national-level. The national-level margin is measured simply as the margin between the winning and losing bloc, similarly to van Egmond (2003). That it is, the number of votes cast for the winning "bloc" minus the number cast for the loosing "bloc". What defines each bloc where a qualitative judgment. For elections with electoral alliances, I used the margin between the winning and losing alliance. Where a single party won the majority (e.g. the US), the margin is between the winning party and the runner up. I used official election data for each estimates. I present all the margins I estimated in Table 2 for scrutiny.

3.3.2 District-level margin of victory

As noted, I use the margin between the winner and the runner up for *the marginal seat* to measure the district-level margin victory. CSES only provides contextual data for the primary electoral district (i.e. the lowest tier), however. I have only calculated the district-level margin at this tier. Many countries allocates seats at several tiers. Germany, for example, allocates different seats at the district- and regional-level using (very) different formulas. In addition, the CSES only provides data for up to nine parties per district. Districts with more than nine parties have incomplete data. I have discarded districts where information on more than ten percent of vote share is missing. Otherwise, I summed the missing vote share into one "party" for the computation of the district margin. Another thing to note is that these methods operates with the number of votes when allocating seats. CSES only provide data for the vote share (percent) each party received. The principal computation remains the same, however, but with the end result being the vote share margin as opposed to the actual vote margin.

For plurality single-majority the race for marginal seat is simply between the two largest parties. Hence, the district-level margin of victory is calculated as the (percentage point) margin between the party with most votes and the party with the second most votes, based on the CSES data. Note that both France and Australia have majority systems where candidates need an absolute majority in order to win a seat. France have run-off elections if no candidates gets enough votes. Australia uses an alternative vote system where the candidate with the least amount of votes get removed—and its votes distributed on the remaining candidates based its voters preferences—until one party have an absolute majority. CSES only provides data from

the first round in both these cases. The district-level margin in France and Australia is therefore also the (percentage point) margin between the two parties with the most votes.

However, for systems using proportional representation the calculation is bit more complicated. I provide two hypothetical examples below to better illustrate. Table 3 shows the allocation of seats based on the Hare quota (largest remainder) and Table 4 the same using D'Hondt (highest fraction) for a hypothetical district with three parties competing over five seats (the same as Selb, 2009). I first go through the Hare quota example to illustrate how I computed the margin in largest remainder systems, and then the D'Hondt example to illustrate how I computed the margin in systems using highest fraction. For an overview of electoral systems and formulas, see e.g. Lijphart and Grofman (1984), Gallagher (1992, 1991) and Farrell and McAllister (2003).

Methods using largest remainder first divide the vote shares for each party by a quota. The two quotas used by countries in the analyses are the Hare and Droop quota. The Hare quota is given by taking $\dfrac{\text{total votes}}{\text{total seats}}$, and the Droop quota by the integer of $1+\dfrac{\text{total votes}}{1+\text{total seats}}$. The integer left after dividing (i.e. how many times each party can fill the quota) is the number of seats allocated automatically to each party. The ranking of the fractional remainders determine the allocation of the remaining seats, which are allocated one by one until all seats are filled. In the hypothetical example with five seats (Table 3), the Hare quota becomes 20 (100/5). After dividing the vote share of each party by the quota, we see that party A gets two automatic seats (with an integer of 2) and party B one (with an integer of 1). Party C did not make the quota (with only a fraction .85). Three (of the total five) seats have then been allocated automatically, so there are two seats left to be allocated by the highest fractional remainder. The first of the two remaining seats are giving to party C (with a fractional remainder of .85) and the second to party A (.65). The marginal seat is the last seat allocated of the remaining seats. In this case, the race for the marginal seat is fought between party A (.65) and party B (.5). The district-level margin is then calculated as $20 \times (.65 - .5) = 3$, where 20 is the devisor that produced the fraction (i.e. the quota). In other words, party B would need another proportion of 3 of the vote to win the final seat.

Table 3: Distribution of seats using the Hare quote in a hypothetical district with five seats and three competing parties

Party	A	B	C	Total
Vote share	53	30	17	100
Total seats:				5
Quota				20
Votes/Quota	2.65	1.5	.85	
Automatic seats	2	1	0	3
Remainder	.65	.5	.85	
Highest remainder seats	1	0	1	2
Total seats	3	1	1	5

Table 4: Distribution of seats using the D'Hondt in a hypothetical district with five seats and three competing parties

Divisor	Party A	Party B	Party C
1	53 (1)	30 (2)	17 (5)
2	27 (3)	15	8
3	18 (4)		
4	13		
Total seats:	3	1	1

The other type of proportional allocation method used by countries in the sample is the highest average methods. This method divides the vote share of each party by a series of devisors and allocate seats one by one to the party with the highest fraction. D'Hondt formula uses 1,2,3… as divisors and Modified Sainte-Laguë uses 1.4,3,5… The hypothetical example in Table 4 shows the allocation of seats according to the D'Hondt formula. The hypothetical district— including vote share—is the same as in the Hare quota example above. We see that D'Hondt would allocate party A the first seat (53), party B the second (30), party A the third (27) and fourth (18) and party C the fifth (17). The marginal seat is the last allocated seat. Party B have the highest fraction (at 15) after the other seats have been allocated, and is therefore the contender for this seat. In this example, I would calculate the district-level margin as $2 \times (17 - 15) = 4$. In other words, the increase in the proportion of votes that party B needs in order to secure the marginal seat is 4. If we compare the two example, we see that both formula

allocates the same amount of seats for the different parties. However, who competes for the marginal seat is different. Under the Hare quota it is party A and B that competes for marginal seat with a margin of 3, while under D'Hondt it is C and B with a margin of 4.

National-level Elections (N=38)

District-level Elections (N=1,603)
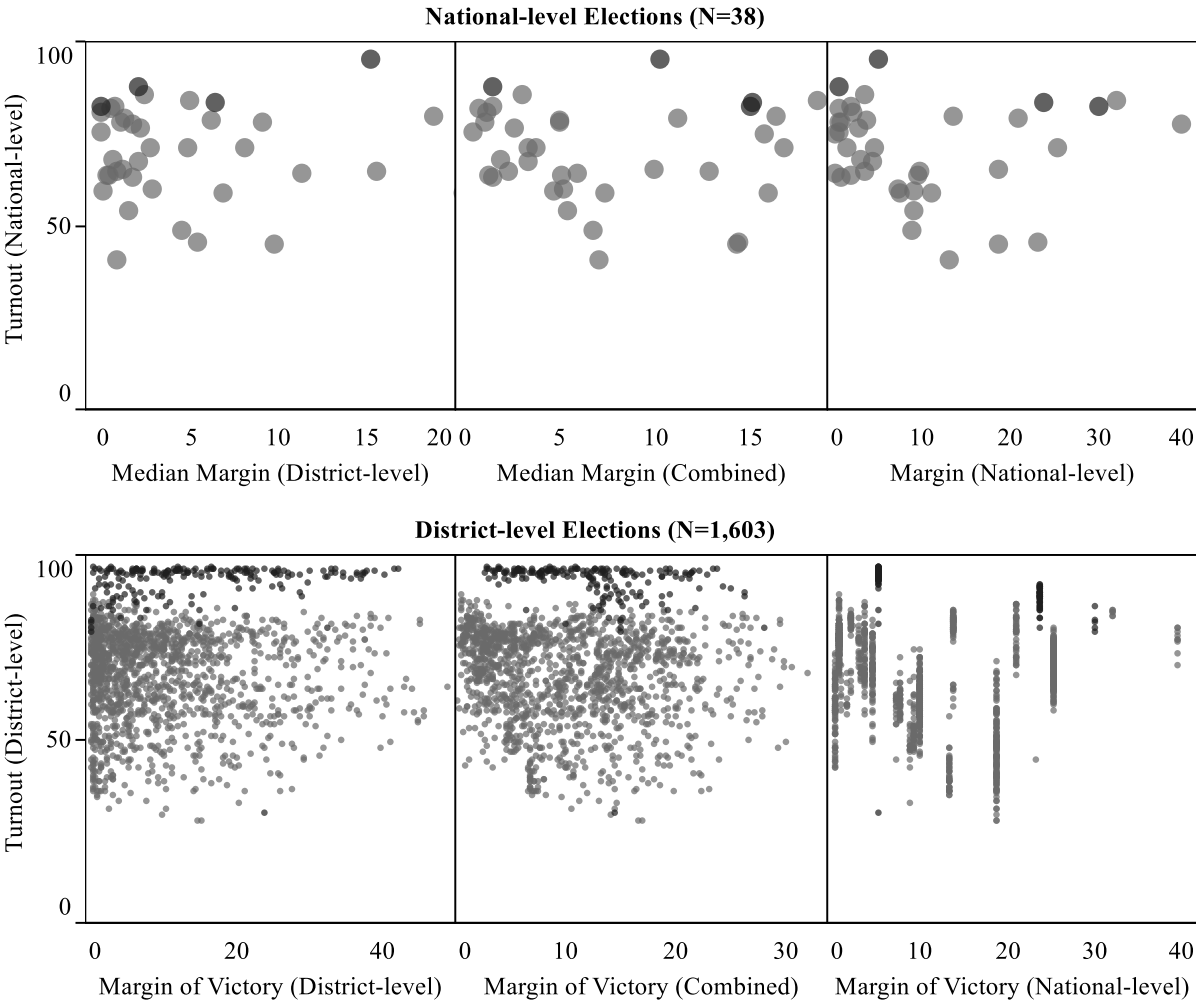
Figure 10: Comparison of a combined versus district- and national-level measures of closeness
Dark dots represent systems with compulsory voting.

There are a few notable exceptions, however. Brazil uses the Hare quota but allocates the remaining seats using the D'Hondt formula. I calculated the margin of victory in Brazil using the competition for the marginal seat allocated at the D'Hondt stage. Greece also allocates

remaining seats in a non-standard way. Greece allocates the remaining seats in two stages using both national- and district-level vote count in a rather complex system. The CSES does not provide enough data to calculate this correctly. I therefore calculated the district-level margin in Greece as in a "normal" largest-remainder system. Furthermore, Ireland have a peculiar (proportional) single-transferable-voting system where the remaining seats are allocated by transferring the surplus votes of the candidates that have already made the quota (see Bowler and Grofman, 2000). Because CSES only provides data for the voters first preference—and since the formula works similarly to normal largest-remainder methods in practice—the district-level margin in Ireland is calculated the same way as a "normal" Hare-quota country.

3.3.3 The combined measure

As noted, I argue that both national- and district-level margins should be combined in order to measure the uncertainty of an election. I combined the computed district-level margin with my estimated national-level margin so that the margin of election k for individual i living in district j is

$$MARGIN_{ijk} = \frac{MARGIN_j + MARGIN_k}{2}.$$

For the analysis, the measure is simply reversed as

$$CLOSENESS_{ijk} = (\max[MARGIN] + \min[MARGIN]) - MARGIN_{ijk},$$

so that a higher number equals a closer election. I have plotted the combined compared to the district- and national-level measures in Figure 10. Black dots represents districts with compulsory voting. Note that this is the actual margin, so dots near zero on the x-axis are district-election that were dead heat. It can be somewhat misleading, because the amount of districts varies from country to country. The Netherlands, for example, is one big district. However, the first notable thing is that districts with compulsory voting clearly separates from the rest, and show no structural pattern with regard to the margin of victory. The second noticeable thing is that the combined measure show how the variation in closeness of election actually is much larger once we account for both margins.
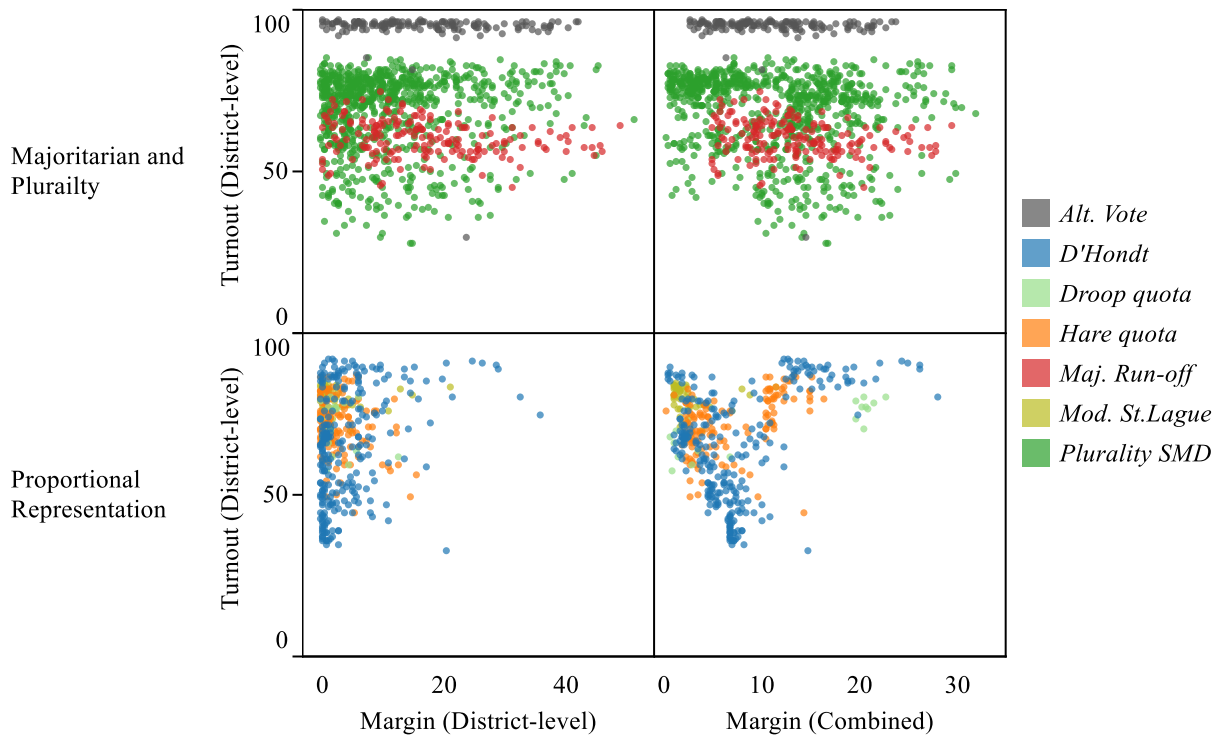
Figure 11: Comparison of closeness of election for different electoral systems and formulas

This is especially noticeable for systems with proportional representation. I have plotted the district- and combined measure by electoral system in Figure 11, and color-coded the different formulas. For majoritarian systems, the increase in variation is not that dramatic, although the tight clustering in the upper left corner is more spread out. There is however a clear separation between systems with compulsory voting and the rest: The line of dots at the top—with a clear margin between them and the rest—is the district-elections in Australia. For PR systems, however, we see that combining national- and district-level closeness changes the picture completely. While the district-level margins are all clustered tight around the zero, when including the national-level margin a clear linear pattern between turnout and closeness appears. Note that most of the dots breaking of that structure are districts in Turkey and Brazil, which have compulsory voting. This emphasize the point that the national-level margin is especially important in PR systems.

3.4 Individual-level variables

My main focus independent variable—aside from information seeking and closeness of election—is *electoral experience*, which I use to measure the degree of habit. In the analysis, I also control for the individual-level variables education, party attachment, political efficacy, age and gender. I present them here in turn.

3.4.1 Electoral experience (as a measure of habit)

I measure electoral experience as the amount of elections the respondent have experienced *if they voted in the last election* based on the respondents age. Hence, electoral experience is coded 0 if they reported that they did not vote in the last election. The measure goes from 0 to 22. About 18 percent are coded 0, with rest being distributed rather like a (slightly left-skewed) normal distribution between 1 to 22. A key thing I am interested in is *habit*. As discussed in the theory section, building on the developmental model participation and cognitive psychology, I expect habit to work as "buffer" for decision to vote. In other words, the key condition for any mechanism to make any difference for the actual act of voting for an individual, is that they do not do not "already" vote because they simply have a habit of doing so. However, to measure habit directly is probably impossible. In order to capture this concept as accurately as possible I use a measure of *electoral experience*. As argued in the theory section, although some assume *not* voting to also be habit forming (e.g., Górecki, 2011), this is of course not the case (see e.g. Aldrich et al., 2011). In addition, it should typically take at least three elections before someone vote as habit (Plutzer, 2002). So if someone have an *electoral experience* of less than three, we can reasonably assume they did not vote (if they did) as an act of habit. Likewise, if someone reported they voted in the last election and have lived through more than twenty, it is *very probable* it was a habit. Hence, I try to measure the "degree of habit" by assuming that the higher the electoral experience, the more likely someone is to have a habit of voting.

3.4.2 Higher education

Education is of course a key variable accounting for the degree of resources (Brady et al., 1995; see also e.g., Gallego, 2010). *Higher education* is a dummy variable coded 1 if the respondent attended (finished or not) education at the university level, and 0 otherwise. It is based from item D3 which ranges from 1 (no education) to 8 (finished university degree). Although the original variable contains more information in this regard, because I do not consider the

difference between the levels of the scale to be equal enough I recoded it into a more sensible dummy.

3.4.3 Attachment to party

The variable for party attachment is comprised from item Q20C, "Do you feel very close to this party, somewhat close or not very close?", where respondent answered either "not very close", "somewhat close" and "very close". I have coded "not very close" as 1 and "very close" as 3. *Because it includes a lot of missing variables, I filled in values from item Q20A*, "Do you feel close to one party?". This item only have two responses (yes or no), so I coded those who answered "no" as "not very close", and those who answered "yes" as "somewhat close". Fitting the models only using Q20C (N ~ 18,000)—that is, without combining the two measures— produces the same results, including quantities of interest, as those presented here.

3.4.4 Political efficacy

Political efficacy was created by combining Q4, "Who is in power can make a big difference?" and Q5, "Who people vote for makes a big difference". These questions measure how much influence respondents think they have on the political system. The two items are combined to one variable ranging from 2 to 10. I use this variable as a proxy for measuring the degree to which the respondents think their vote will make a difference. It measure this indirectly. While the term "efficacy" within psychology usually refers to the ability of bringing about change directly by one's self (see e.g. Bandura, 1977), this variable measures to what degree the respondents think they're vote will affect the political system. That is, if the individual act of voting makes a difference on the results *and* if the results makes a difference for the political system. The findings for political efficacy at the individual-level is somewhat mixed, however. Smets and Ham (2013) finds that most test (they include) do find a significant and positive effect of efficacy on turnout, but also that most studies do not. I include this variable because of the decision hypothesis. I want to control for what would be a second (also interesting) alternative mechanisms: If close elections make more people feel their vote matters and are more inclined to vote because of it.

3.4.5 Age, age squared and gender

The resource model of political participation consistently link variables like age and education to higher turnout (Brady et al., 1995). Being older increase information and lower the cost of seeking information because it increases access to resources (Moyser and Day, 1992). Although

the variable for electoral experience may be very correlated with *age*, age is an important predictor also for other reasons. I include *age* and *age squared* as control variables to cover other non-habit related factors. In other words, with the risk of confounding, I wish to separate the effect of electoral experience from other age-related turnout-boosting factors. In addition, I included a demographic control variable for gender as *female* coded 1 if the respondent was female.

3.5 District- and national-level control variables

I also include variables for *district magnitude, compulsory voting*, *Proportional representation* and *Freedom House rating*. District magnitude (along with closeness of election) is at the district, i.e. second, level. Compulsory voting, proportional representation and Freedom House rating is at country, i.e. third, level. I present them here in turn.

3.5.1   District magnitude (district-level)

The *district magnitude* is the amount of seats in each district, i.e. a district-level variable. It varies from 42 to 1, with the exception of the Netherlands which have a district magnitude of 150. Blais and Lago (2009: 98) argues that the impact of closeness of election decreases as the district magnitude increases. However, in the way I measure closeness, I remain somewhat skeptical: Although there are more seats contest, the crucial thing determining the closeness of an election is the marginal (pivotal) seat. Even if a district have a very large number of seats, it can still be pivotal if the last seat is closely contested. I include it as a control because the closeness measure I use is the vote share margin, and not the actual vote margin. It is possible to imagine that the one seats in a SMD election has a higher value than one of the 150 of the Netherlands.

3.5.2   Compulsory voting (country-level)

In include three country-level variables. Firstly, I include a dummy variable for c*ompulsory voting*, coded 1 if there is enforced compulsory voting and 0 otherwise. Both Australia Turkey and Brazil have compulsory voting in my sample. The positive effect of compulsory voting on turnout is among the most robust findings in the literature (see Geys, 2006: for a review; see also e.g. Blais and Dobrzynska, 1998; Hadjar and Beck, 2010; Jackman and Miller, 1995). As indicated by the graphical representation of the margin in section 3.2.1, countries with compulsory voting have an equally high turnout out rate regardless of the closeness of the election.

3.5.3   Proportional representation (country-level)

In addition, I include a dummy variable for *proportional representation*, coded 1 if the electoral system is proportional and 0 if it is majoritarian or mixed. 18 of the 26 countries in the analyses

have PR (see Table 2). Turnout tend to be much higher in proportional electoral systems (Blais and Dobrzynska, 1998; Franklin, 2002; Jackman, 1987; Jackman and Miller, 1995; Selb, 2009). Because the nature of the competition is so different, this is of course a key control variable.

3.5.4   Freedom House (country-level)

I also include a variable for the *Freedom House rating* of the respective country. The FH rating is a measure of the degree of freedoms and civil liberties. The ranking goes from 1 to 17, with countries below 3 are considered free, and countries between 3 and 5 are considered "partly free". Most countries in my analysis have a rating of one. However, Turkey and Hong Kong have a rating of 3 and 3.5, making them only "partly free". Eight countries in the sample have a rating above 1 but below 3. Although the Freedom House score is often criticized (e.g., Erdmann and Kneuer, 2011: 105), different levels of civil liberties can make for different dynamics with regard to how closeness affects turnout. This is key because I do wish a comparable sample, albeit with a big scope in order to infer more generally.

3.6 Summary

This chapter described the data, measurement and operationalization used in the analysis. The data set includes 35,913 respondents as unit of analysis, nested within 1444 district-election again nested in 26 countries. The data is therefore structured in three hierarchical levels, with district-elections as second and country as third. It is "district-election" because the same district in two elections in the same country are considered separate clusters (to ease computation). The data is comprised from the CSES and covers 31 elections between 2006 and 2011. In order to measure closeness of election I computed the vote share margin between the winner and looser of the *marginal seat*, and combined that with the national-level two-bloc margin.

4. METHOD

The following chapter deals with the methods and methodologies that are used in the analysis. I take a quantitative approach and use multilevel regression models—both linear and logistic—as well as average causal mediation analysis. In general, I adopt a comparative perspective within this framework. I begin the chapter by introducing the general research design. I then discuss the multilevel models. Lastly, I explain the estimation procedure of the average causal mediation analysis.

4.1 Research design

The implicit aim of this thesis is to make some inferences about the complex relationship between closeness of election, several moderating factors, and the decision to vote. I regard it as comparative because, as Przeworski and Teune (1970: 74) argues, comparative politics deals with "the influence of larger systems upon the characteristics of units within them". To be able to deal with the large amount of individual-level data and at the same time make as accurate inferences as possible—and keeping within the tradition of the turnout literature—I use quantitative methods. Because my unit of analysis are individuals at the micro-level and my focus independent variable is at the macro-level, my theory implies a relationship between several layers of data (Steenbergen and Jones, 2002). More accurately, my data have three levels, with (i) individuals nested within (ii) district-elections, which in turn are nested within (iii) countries. For hypotheses 1 and 2 I use multilevel logistic regression models with *voting* (i.e. did or did not vote) as dependent variable. For hypotheses 3 and 4 I use multilevel linear models with *information seeking* as dependent variable. In order to test the conditional effects I use interaction terms and calculate marginal effects. For hypothesis testing, I primarily rely on this rather than the main effects. Lastly, for hypothesis 5 I use both and estimate the average causal mediation effect.

4.2 Multilevel regression models

All regression models included in the analysis are multilevel models. Both linear and logistic regression models rest on the assumption that there is no autocorrelation, i.e. that the observations are independent from one another (Rabe-Hesketh and Skrondal, 2008: 323). This is not the case here, because my data have a natural multilevel structure. We can reasonably assume that individuals within the same districts and individuals within the same countries are more similar to one another compared to individuals in other districts and countries. If such inter-individual dependencies are not accounted for, the standard errors are usually underestimated and effects seems more statistically significant than they actually are (Hox, 2010: 6). Multilevel analysis is a statistical technique that allows for the analysis of independent variables operating at different levels, i.e. where the units of analysis are hierarchically nested within groups (Hox, 2010), as here. The fundamental idea of multilevel regression modeling is to account for a hierarchically structured data by implementing random effects at the various hierarchical levels. For the first model—where I test hypothesis 1 and 2—I use a logistic regression model. This is because the dependent variable is binary (did vote /did not vote). In the second model—where I test hypothesis 3 and 4—I use a linear regression model. More accurately, I use linear mixed models and generalized linear mixed models. In this chapter I focus on the estimation procedure, modeling choices, assumptions and interpretation of the models. An in-depth statistical account of multilevel regression models are available by e.g., Bryk and Raudenbush (1992), Goldstein (2011), Snijder and Bosker (2004) and Hox (2010).

4.2.1   Model estimation

When using multilevel models, it is generally recommended to start with an empty model (only including the random intercept for the dependent variable) and build stepwise up to the finished model with all variables and interaction terms (Luke, 2004). This is called the "bottom-up" approach (Hox, 2010). The idea is to continuously watch the standard errors and residual variance at the distinct levels while gradually adding variables. The alternative is to start with all variables and gradually strip down (Hox, 2010: 56). However, the bottom-up approach is usually considered more parsimonious (Luke, 2004). In my estimation procedure, I used the bottom-up approach for both the linear models and the logistic models: First, I estimated an empty model only including the dependent variable and the random intercept at the district-election- and country-level. Second, I included the individual-level variables. Third, I added the district-level level variables, which includes *closeness of election*. Fourth, I added the

country-level control variables. These four models then serve as a reference point. Fifth, I added the interaction terms, with one model for each hypothesis. I evaluate the goodness-of-fit for each model using information criteria and looking at the variance at the higher levels. All models in the analysis include the same 35,913 respondents, nested in 144 district-election and 26 countries. Because I have the same data for all models they easier to compare (Hox, 2010: 49).

All models are *fixed slope random intercept* models, i.e. I only let the intercepts vary between district-elections and countries. An alternative approach would be to let the slope of *closeness of election* to vary between countries as well, but I have not done so here. The estimation is done using maximum likelihood (ML). ML uses the mean and variance as parameters and estimates the values with the highest likelihood of generating the observed sample, given that the assumptions of the model are true (Hox, 2010). For the logistic regression models, the log-likelihood is approximated using adaptive Gauss-Hermite with 25 quadrature (integration) points. All "main" models are estimated in Stata 13.1 using the *mixed* command for the linear multilevel models and the *meqrlogit* command for the logistic multilevel models. The regression models estimated for the average causal mediation analysis—that are modelled to "mimic" the main models—are estimated using the *lme4* package in R. The control parameters for these models are set so that they are as equivalent as possible to the models estimated in Stata.

4.2.2   *Information criterion and model comparison*

To evaluate the model performance I use Akaike Information Criterion (AIC) (Akaike, 1998), Bayesian Information Criterion (BIC) (Schwarz, 1978) and the deviance (-2*log-likelihood). For each measure, a lower value should constitute a better fit. Often, evaluating regression models include evaluating R2-values. However, especially for multilevel models, the R2 is not considered very useful, because it cannot be interpreted as the proportion of explained variance as is done in a standard linear regression (Luke, 2004). Instead, you would get an R2 value for each of the three levels which, although possible (Rabe-Hesketh and Skrondal, 2008: 103), is not done here. In pretext of a multilevel model, information criterions like the AIC and BIC as relativistic measures are more useful. The deviance is not an information criterion per say, it measures the goodness-of-fit only by looking at the log-likelihood (LL). Neither deviance, AIC or BIC are particularly useful by themselves (see e.g., Trivedi, 2010: 359). However, because I will estimate several models, they are used to compare each model relative to each other. The

deviance statistic will generally decrease as the parameter of the model increases (i.e. with the complexity of the model). This will make it automatically favor the more complex models, making it less useful when comparing the models including interaction terms with those that don't, for example. For this reason

I also include AIC and BIC. They are also based in the LL, but includes penalties when adding more variables. The AIC penalize based on number of variables in model and the BIC also penalize based on the sample size. I have estimated both AIC, BIC and deviance for each model. I will also report the intra-class correlation (ICC). I calculate the ICC by dividing the residual variance in the dependent variable at the higher levels by its total variance. It can be interpreted as the proportion of variance in the dependent variable that happens between—rather than within—the different levels. In logistic models the residual variance at the first level is fixed at 3.29, so the ICC is primarily affected by changes in the variance term at the higher levels (Hox, 2010: 59). Because of the large sample size (with N almost 36,000) the probability of getting statistically significant results is higher, so the α-value is set to five percent. However, the actual threshold is often somewhat arbitrary (as in .049 vs. .051). I do not reject the null hypotheses unless the threshold is met, but I still provide full *p*-values for the discussion. All significant tests are two-tailed.

4.2.3   Interaction terms, conditional hypotheses and marginal effects

All hypotheses, with the exception of hypothesis 5, are *conditional* hypotheses. That is, they mean to describe the relationship between closeness of election and voting, or closeness of election and engagement, where the effect is hypothesized to be contingent on a third and fourth variable (Franzese and Kam, 2009). The way to analysis this in a regression model is to include an interaction term between conditional variables (Brambor et al., 2006: 64). In the case of logistic regression, however, some methodologists debate whether it is necessary to include the product term (Berry and Berry, 1991; Berry et al., 2010; Nagler, 1991; see also Rainey, 2014). This is based on the work of Wolfinger and Rosenstone (1980) who argued that registration requirements have a smaller effect on individuals with more education (Rainey, 2014: 2). They do not include any product term in the model when testing this by arguing that the logistic model accounts for this naturally because the S-shaped response curve creates a "compression effect" (Rainey, 2014: 2). However, Rainey (2014), using simulations, shows that excluding the product term bias the researcher towards finding an interaction. Therefore I also include interaction terms to test my conditional hypotheses for the logistic models. My hypotheses

primarily deals with the conditional relationship between three variables, meaning that I include an interaction between all three. In addition, it is important to include all constitutive variables, including all underlying two-way interaction (in the case of three-way interaction terms)(Brambor et al., 2006). When the interaction terms are included in the model, the main effect of the constitutive variables is not really interpretable as unconditional effects (Franzese and Kam, 2009: 20). The coefficients of the constitutive variables is the slope for one unit change in the dependent variable when the other variables included in the interaction term are zero. In other words, when including an interaction term, the coefficient of its constitutive terms have no meaningful theoretical value. When I test the unconditional relationship between these variables, it is by running a model without the interaction term. Some would argue, however, that if any variable is also part of an interaction effect, it makes little sense to analyze its unconditional effect (Brambor et al., 2006: 73). Although I would argue that this is not necessarily the case, my theoretical focus is more or less exclusively the conditional relationships. To analyze this in a meaningful way, I calculate the marginal effect of closeness of election across the different values of the variables I suspect it is dependent on. Only looking at the main effect of the interaction term make little sense because the it does not show the conditional uncertainties they are calculated on (Brambor et al., 2006). I present the marginal effects with confidence intervals graphically by plotting them for each interaction term.

4.2.4   Hypotheses 1 and 2: Logistic regression

The dependent variable for hypotheses 1 and 2 is a binary indicator of whether the respondent voted or not. Because it is a simple yes/no outcome it has two natural bounds and only two values, so a coefficient calculated using linear regression would be nonsensical, e.g. predicting negative values. In addition, having a binary dependent variable in a simple linear regression will violate several assumptions. Firstly, the residual variance would not be constant across the different values of the independent variables, i.e. the assumption of homoscedasticity would be violated (Menard, 2002: 7). Second, the residuals would not be normally distributed. I therefore use a generalized linear model instead, which can treat the dependent variable as the outcome of a Bernoulli trail rather than as a continuous outcome. The regression is simply done with the natural logarithm of the odds of someone voting (i.e. logit) as the dependent variable instead, hence "logistic regression". The results from the logistic regression models are presented as OR. For a continuous independent variable, the OR is the mean increase in odds of voting for a unit increase in that variable. For a dummy variable, the OR is the mean increase in odds of

voting if the dummy equals 1. An OR less than 1 means the odds is lower, and an OR higher than 1 mean that the odds is higher.

To test hypothesis 1 I include an interaction term between *electoral experience* and *closeness of election*. Because I expect closeness of election to only have any effect for those without habit, I predict that the marginal effect of closeness should increase as electoral experience decreases. I fo this in two stages, however. My first model with this interaction term (2.4) does not include the motivational factors (information seeking, party attachment and efficacy). The second (2.5) does. Because I expect closeness of election to primarily affect voting indirectly through these variables, including them should reduce the statistically significant effect of closeness. My rationale for this expectation is that those variables are better predictor by themselves, and the regression model should not be able to see the difference between, e.g., those who are very interested in the election and those who got very interest in the election because the race was close. To test hypothesis 2 I include a three-way interaction term between *closeness of election*, *electoral experience* and *information seeking*. Because *closeness of election* should have an impact on those who are interested in the election and does not have a habit of voting, I predict that the marginal effect of closeness should be higher for this group. Conversely, that it should not be statistically significant those without any interest in the election and with very little electoral experience.

4.2.5   Hypotheses 3 and 4: Linear regression

For hypotheses 3 and 4 my dependent variable is the level of interest in election, measure by *information seeking*. To test hypothesis 3 I include a three-way interaction term between *closeness of election*, *electoral experience* and *higher education*. Because a closer election should fuel more interest in educated individuals without a habit of voting, I predict that closeness of election only should have a statistically significant effect for those with higher education and little electoral experience. For hypothesis 3 I include a three-way interaction term between *closeness of election*, *electoral experience* and *party attachment*. Similarly, because closeness should affect those who feel close to a particular party without a habit of voting, I predict that closeness should have a statistically significant effect only on this particular group, and not otherwise.

4.2.6 Assumptions and limitations

The most important thing I must emphasize is that *the dependent variable for the linear regression model only have four different possible values*. The variable for *information seeking* is scale from 1 to 4. The problems is that the residuals then can't really by normally distributed. I have provided diagnostic plots in the appendix. This includes a density plot of the distribution of residuals (also by country), a scatter plot between the residuals and fitted values and a Q-Q plot. The residuals seems normally distributed, but when plotting them against the fitted values there are clear patterns for each value. A second thing to note is that the linear regression assumes the relationship between values on the scale to be equally distant. I originally intended to use a multilevel ordered logistic regression, but the available out-of-the-box procedures either used days to estimate or failed to converge with the more complex interaction models. I have rerun the model for hypothesis 3 and 4 with multilevel logistic regression after recoding *information seeking* into a binary variable (with those who followed the election "fairly closely" and "very closely" were coded as one), which yields the same results as those presented in the analysis (including quantities of interest).

Both models assumes that the error terms are independent. The CSES sample selection are random within districts (in some cases random also across districts), so when I account for the district- and country-level dependencies, this assumption should be reasonable. Lastly, all regression models assume that the conditional probability distribution between the dependent and independent variables are not caused by any other underlying factor. The robustness of the theoretical assumptions the statistical models is the only test for this. I would of course argue these assumptions are reasonable. I have tried to explicitly state all important choices made both regarding modelling and data measurement, so that the analyses can be evaluated with that in mind.

4.3 Average causal mediation analysis

Hypothesis 5 proposes that closeness of election also may have an indirect effect on voting via interest in the election (which I measure as *information seeking*). To test this I combine the "full" linear and logistic models above and estimate how closeness of election might affect voting *through* information seeking (as per Imai et al., 2010a; Imai et al., 2010b; Imai et al., 2011). Identifying whether there is a *causal* mechanism is of course no trivial matter. The traditional approach to these types of questions have been to use structural equation models (Baron and Kenny, 1986; see also e.g., MacKinnon, 2008; Shadish et al., 2002). However, mediation analysis is increasingly criticized for various (very legitimate) reasons (Green et al., 2010: 203; Bullock and Ha, 2011; Heckman and Smith, 1995; Deaton, 2010; Brady and Collier, 2010). Still, uncovering causal mechanisms is a fundamental goal of political science. The inherit difficulty with doing so quantitatively have led some to argue that process tracing, for example, is the best approach (Collier et al., 2004). Although I recognize this, I intend to test my hypothesis with the data at hand to the best extent possible. For this purpose I use the relatively new estimation procedure developed by Imai et al. (2010b; 2011; 2010a; 2013), called average causal mediation effect or ACME. By definition it still rests on certain assumptions, but less so than traditional approaches using structural equation modeling. Here I will outline the method and methodology.

4.3.1   Hypothesis 5: ACME estimation

For this purpose I define hypothesis 5 as suggesting a *process* in which closeness of election influence voting by using *information seeking* as the causal pathway to do so. Within the potential outcome framework we can exemplify like this (Imai et al., 2011: 768; see also Imai et al., 2010b; Pearl, 2000; Rubin, 2004, 1974; Splawa-Neyman et al., 1990): Assume that each respondent were treated with two scenarios, one in which the election was close and one in which it was not. For each respondent i, let T_i be an indicator taking the value 1 if the election was close and 0 otherwise. Then let $M_i(t)$ denote the potential value of *information seeking* for respondent i in election with closeness $T_i = t$. The potential outcome Y_i, i.e. did or did not vote, in an election with closeness t and information seeking m for respondent i is then defined as $Y_i(t,m)$. For example, for hypothesis 5, $Y_i(1,1)$ is the potential outcome for respondent i if he/she is in a close election and sought out information (assuming we separate between seeking and not seeking). The outcome we observed, Y_i, can then be written as $Y_i(T_i, M_i(T_i))$, meaning that

it is dependent on both $M_i(t)$ and T_i. That means that the *total effect* of closeness of election for respondent i is

$$\tau_i(t) = Y_i(1, M_i(1)) - Yi(0, M_i(0)),$$ (1.1)

which means that the *mediation effect* for closeness $t = 0, 1$ (assuming it is either close or not) is (Imai et al., 2011: 769)

$$\delta_i(t) = Y_i(t, M_i(1)) - Y_i(t, M_i(0)).$$ (1.2)

In other words, hypothesis 5 can be tested by measuring the potential outcome when the respondents is in a close election minus the potential outcome when the respondent is in not-close election, which is dependent on $M_i(t)$. That is, the mediation effect is the change in outcome equivalent to the change of *information seeking* that would be realized in a close election, i.e. $M_i(0)$, compared to a not-close election, i.e. $M_i(1)$. If *information seeking* would stay the same in a close election as in one that is not, all else equal, the mediation effect would be zero. The average causal mediation effect (ACME) is then the average $\delta(t)$, i.e. $\bar{\delta}(t)$. The last possible mechanism, the *direct effect* of closeness, is

$$\zeta_i(t) = Y_i(1, M_i(t)) - Y_i(0, M_i(t)).$$ (1.3)

The problem this highlights is that we cannot observe different outcomes in difference scenarios for the same respondent. Instead, I use regression models to predict the different potential outcomes for each part of the equation. I did the ACME estimation in two steps. First, I fitted regression models for the mediator and the outcome. The mediator model is a multilevel linear regression model with *information seeking* as dependent variable, and the outcome model is a multilevel logistic regression model with *voting* as dependent variable. Based on the mediator model, the ACME procedure generate two sets of predictions for *information seeking*, one for when election is close and one for when it is not. I have coded a close election as one with a vote share margin of 0, and a not-close election as one with a vote share margin of 20. Second, based on the outcome model, the ACME procedure predicts the outcome based on these values. For a close election, it first predicts the outcome (i.e. odds of voting) using the value for *information seeking* that it were predicted to have if the election was close. It then predicts the

outcome using the value for *information seeking* that it were predicted to have if the election were not close. The ACME is then computed as the average difference between these two predictions. The estimation is done using quasi-Bayesian Monte Carlo method with a 1000 simulations (King et al., 2000; Imai et al., 2010a).

However, there are two limitations to this approach. The first limitation is the *sequential ignorability assumption (Imai et al., 2011: 770)*. For inference to be valid in the framework outlined above, the distribution of close election—given the other variables—must be "ignorable". That is, the distribution must be independent of the potential outcomes and potential mediators. Similarly, *information seeking* must be assumed "ignorable" given the closeness of election and the other variables. This might be difficult even in an experimental setting, which highlights the difficulty with studying causal relationship with observation data. Although this assumption is untestable, the strength of the ACME procedure is that it comes with a sensitivity analysis designed to quantify the degree of possibility of violation. However, this test is not yet available for multilevel models. This assumption, then, only rests on the theoretical framework. The second limitation is that the ACME procedure cannot, as of yet, handle three-level regression models. This means that the respondents in the outcome and mediator models are only nested at the country-level. Therefore, in the analysis section, I first compare the two-level outcome and mediator models with the three-level models they are based on. The mediation analysis—including the outcome and mediation regression models—are with the lme4-package (Bates et al., 2008) and the mediation-package (Tingley et al., 2013) in R.

4.4 Summary

I use both linear and logistic multilevel regression models for the empirical analysis. The logistic models are for testing hypotheses 1 and 2 where the dependent variable is a binary indicator for whether the respondent voted. The linear model are for testing hypotheses 3 and 4 where the dependent variable is information seeking. For hypothesis 5 I combine both the linear and logistic model to estimate the average causal mediation effect, or ACME.

5. RESULTS

In this chapter I present the results from the analysis. I start with the results from the multilevel logistic regression model with *voting* as dependent variable. This is to test the effect of closeness conditionally on other variables. The second section have *information seeking* as dependent variable in a linear multilevel regression model in order to test how closeness of election might affect information seeking directly. In the last section I present the results from the mediation analysis. I will go through the analysis in the following way: I first go through "the reference models". They are reference models because I only use them for diagnostics and not for inference per se. I then go through the regression models including interaction terms one at a time, which all are estimated with a specific hypothesis in mind.

5.1 Closeness of election and voting

In this section I test the relationship between closeness of election and voting using individual-level turnout as dependent variable. These are multilevel logistic regression models with a binary dependent variable indicating whether the respondent cast a ballot or not. As discussed earlier in the theory chapter, I am interested in whether the effect of closeness might by conditional on a series of demand-side factors. I expect that the effect of closeness of election is conditional on habit and information seeking. I use electoral experience as a proxy for measuring habit, and expect the marginal effect of closeness to be negligible for individuals that have a high degree of experience (and hence vote because of habit). I also hypothesize about two types of links between the closeness of an election and information seeking: Either people that are more engaged in an election should be more influenced by a close election (i.e. a contingent effect), or a close election should increase engagement which in turn affects the decision to vote (i.e. an indirect effect). I address the direct relationship between closeness and information seeking in the next section. Here I estimate a model to test whether the effect of closeness is contingent on information seeking behavior, conditionally on habit.

5.1.1   Reference models (2.0-2.3)

The reference models are presented in Table 5. The first model (2.0) is the null model, and it only contains the intercept and variance between and within districts and countries (Rabe-Hesketh and Skrondal, 2008). AIC and BIC is 25094 and 25119. The deviance is 25088. These numbers will serve as reference for the rest of model. As explained in the methods chapter, the intra-class correlation is an estimation of the proportion of variance between (rather than within) district-elections and between countries. The null model have an intra-class correlation of 23.5 percent at the country-level and 26.2 percent at the district-level. This indicates that there is great deal of variation is between different district-elections and countries, and that a multilevel model indeed is warranted (see e.g. Hox, 2010: 47-50). In model 2.1 I introduce the level-1 variables *attachment to party, information seeking, efficacy, experience, age, age squared, higher education* and *female*. This made the AIC and BIC decreased to 19,849 and 19,942, respectively. The deviance decreased to 19,827. The intra-class correlation also decreased from 23.5 percent to 17.7 percent at the country-level and from 26.2 to 20.3 at the district-level. In addition, the random intercept for countries decreased from 1.049 to .751 and from .117 to .093

for districts. This means that the individual-level variables are able to account for a chunk of the variance at the district- and country-level. All variables are statistically significant and have coefficients in the expected direction.

Table 5: Multilevel logistic regression models of closeness of election and individual-level voting (odds ratio)

	Model (2.0)	Model (2.1)	Model (2.2)	Model (2.3)
	OR (p)	OR (p)	OR (p)	OR (p)
Fixed effects				
Closeness of election			1.011 (.037)*	1.007 (.186)
Attachment to party		1.554 (.000)***	1.556 (.000)***	1.556 (.000)***
Information-seeking		2.067 (.000)***	2.062 (.000)***	2.057 (.000)***
Efficacy		1.174 (.000)***	1.175 (.000)***	1.175 (.000)***
Experience		1.245 (.000)***	1.245 (.000)***	1.245 (.000)***
Age		1.041 (.000)***	1.040 (.000)***	1.040 (.000)***
Age (squared)		.999 (.000)***	.999 (.000)***	.999 (.000)***
Higher education		1.668 (.000)***	1.667 (.000)***	1.668 (.000)***
Female		1.078 (.045)*	1.070 (.045)*	1.079 (.044)*
District magnitude			1.002 (.584)	1.003 (.421)
Compulsory voting				5.860 (.000)**
Proportional Repr.				.732 (.343)
FH rating				.613 (.004)*
Random effects				
var (u_{0j}): intercept (cntry)	1.049	.751	.791	.484
var (u_{0jk}): intercept (dist.)	.117	.093	.0.87	.084
Model summary				
ICC (rho)=cntry. lvl.	.235	.177	.190	.126
ICC (rho)= dist. lvl.	.262	.203	.211	.147
$-2LL$	25088.359	19827.032	19822.075	19802.932
AIC	25094.36	19849.03	19848.07	19834.93
BIC	25119.83	19942.41	19958.43	19970.75
n=resp./dist./countries.	35913/1444/26	35913/1444/26	35913/1444/26	35913/1444/26

***p<0.001; **p<0.01; *p<0.05, two-tailed. Dependent variable is 1/0 indicating if resp. voted in last election.

Model 2.2 also includes the district-level variables, *closeness of election* and *district magnitude*. The model does not perform particularly better than the model with only individual-level variables. Deviance only decreased slightly from 19827 in the previous model to 19822. Although AIC stayed about the same, BIC actually increased from 19942.41 to 19958.4. In addition, the intra-class correlation increased from 17.7 percent to 19 percent at the country-level and from 20.3 percent to 21.1 percent at the district-level. In other words, including *closeness of election* in the regression model does not exactly increase the performance compared to the previous model. The coefficient of *closeness of election* is positive and statistical significant with a p-value of .37. However, this is probably because I have not

controlled for different types of electoral systems. *District magnitude*, on the other hand, is not statistically significant.

In model 2.3 I introduce the country-level variables, *proportional representation, compulsory voting* and *Freedom House rating*. This is model includes all variables used in the analysis. The AIC decreased from 19848 to 19834 and the deviance from 19822 to 19802, although BIC increased. In this sense the model performs about the same as the ones not including national-level variables. Intra-class correlation, however, went down from 19 to 12.6 percent at the country-level and from 21.1 to 14.7 percent at the district-level. The variance of the random intercept for the country-level went from .791 to .484 and the district-level from .211 to .084. The large decrease in both intra-class correlation and variance of the intercept shows that these variables are able to bite of a significant portion of the variance at the higher level. All in all, the variables performs as expected. Both *compulsory voting* and *Freedom House Rating* are statistically significant. The dummy variable for *proportional representation*, however, is not. Notably, the slightly statistically significant effect of *closeness of election* found in model 2.2 is now gone.

5.1.2   Interaction models (2.4-2.7)

If we were to conclude only the basis of the regression models above, we would probably argue that the closeness of an election seems unrelated to the decision to vote (at least on average). Now I turn to the main analysis in which *closeness of election* is the focus explanatory variable and *electoral experience, information seeking* and *party attachment*, in turn, are moderator variables. The multilevel models with interaction terms are presented in Table 6. I have four model with interactions terms. Model 2.4 and 2.5 includes an interaction term between *closeness of election* and *electoral experience*. I expect that the habit of voting moderates the decision-making process so that it constrains the role any supply- and demand side mechanisms may have on the actual act (hypothesis 1). More concretely, I expect the marginal effect of *closeness of election* to decrease as the *electoral experience* increase. I test this by including said interaction term and calculating the marginal effect of closeness over different values for *electoral experience*. In model 2.4 I exclude the demand-side factors, i.e. *attachment to party*, *information seeking* and *efficacy*. If the relationship between supply- and demand-side factors are as I hypothesize, including them should distort the picture because the relationship between

electoral experience and *closeness of election* is dependent on the demand-side factors. Because the impact of *closeness of election* actually depends on the values of these other variables, and because they are (by definition) the primary predictors of voting, they should account better for the variance preciously explained by *closeness of election*. The demand-side factors are put back in in model 2.5 to test this assumption. In model 2.6 I include a three-way interaction term between *information seeking, closeness of election* and *electoral experience.* This is to test whether the effect of closeness of election is contingent on information seeking, while still assuming they are both moderated by habit (hypothesis 2).

Table 6: Multilevel logistic regression models of closeness of election and individual-level voting, with interaction terms (odds ratio)

	Model (2.4)	Model (2.5)	Model (2.6)
	OR (*p*)	OR (*p*)	OR (*p*)
Fixed effects			
Closeness of election	1.001 (.924)	1.000 (.972)	.969 (.011)*
Attachment to party		1.557 (.000)***	1.556 (.000)***
Information seeking		2.056 (.000)***	1.227 (.304)
Efficacy		1.175 (.000)***	1.175 (.000)***
Experience	1.231 (.000)***	1.161 (.000)***	1.127 (.108)
Age	1.028 (.000)***	1.040 (.000)***	1.040 (.000)***
Age (squared)	.999 (.000)***	.999 (.000)***	.999 (.000)***
Higher education	2.038 (.000)***	1.661 (.000)***	1.656 (.000)***
Female	.953 (.180)	1.080 (.043)*	1.077 (.051)
Closeness * Exp.	1.001 (.024)	1.002 (.008)**	1.003 (.068)
Closeness * Inf. Seeking			1.014 (.003)**
Clsnss. * Inf.Skn. * Exp.			.999 (.248)
Exp. * Inf. Seeking			1.019 (.533)
District magnitude	1.003 (.441)	1.003 (.424)	1.003 (.416)
Compulsory voting	8.459 (.000)***	5.893 (.000)***	5.729 (.000)***
Proportional Repr.	.593 (.184)	.732 (.341)	.740 (.359)
FH rating	.461 (.000)***	.600 (.003)**	.605 (.003)**
Random effects			
var (u_{0j}): intercept (cntry)	.719	.479	.483
var (u_{0jk}): intercept (dist.)	.100	.083	.083
Model summary			
ICC (*rho*)=cntry. lvl.	.175	.124	.125
ICC (*rho*)= dist. lvl.	.199	.146	.147
$-2LL$	21960.102	19796.068	19776.122
AIC	21988.1	19830.07	19816.12
BIC	22106.95	19974.38	19985.9
n=resp./dist./countries.	35913/1444/26	35913/1444/26	35913/1444/26

***p<0.001; **p<0.01; *p<0.05, two-tailed. Dependent variable is 1/0 indicating if resp. voted in the last election.

As expected, when excluding the demand-side factors and including the interaction term, the performance of the model decrease substantially (model 2.5 in Table 6). The AIC and BIC went up from 19834.9 and 19970.7 in model 2.3 to 21988.1 and 22107, respectively, in model 2.4. Deviance goes from 19802.3 to 21960.1. In addition, variance at both upper levels increased substantially. The variance of the intercept goes from .484 to .719 for the country-level and from .084 to .100 for the district-level. The intra-class correlation increased from 12.6 and 14.7 for the country-level and district-level, respectively, to 17.2 and 19.9. This really emphasizes the importance of the demand-side factors for the performance of the model. The coefficient for *Closeness of election* remains not statistically significant. The coefficient for the interaction term between closeness and experience is also not statistically significant, and neither is the variable for gender. The other variables perform similarly as before. However, I am interested in the quantities of interest and not the mean odds ratio. Figure 12 plots the estimated marginal effect of *closeness of election* (the y-axis) over different values of *electoral experience* (the x-axis). The marginal effect is the approximation of how much the dependent variable is expected to increase or decrease for a unit change in the independent variable. The vertical bands represents the 95 percent confidence intervals. The effect is regarded as statistically significant (at alpha=.05) as long as these bands does not overlap the x-axis. In line with my hypothesis, the figure shows that the marginal effect of closeness increases substantially as electoral experience decreases. Consequently, the plot seems to provide prima facie evidence that the impact of closeness is stronger the less likely someone is to have a habit of voting.

The second model (2.5 in Table 6) have the same interaction-term but includes the demand-side factors: *Information seeking, efficacy* and *party attachment*. If we assume that the effect of closeness of election is conditional on *electoral experience* as well as *information seeking* and *party attachment,* the marginal predictions should change. The regression model does not account for the difference between, e.g., someone very interested in the election, and someone very interested in a close election (and interest is always a good predictor)[2]. However, in terms of goodness-of-fit, the model performs better than model 2.4, but is comparable to model 2.3 (same but without the interaction term). AIC decreased slightly from 19835 to 19830, but BIC increased from 19971 to 19975. The random intercept and the intra-class correlation also remains essentially the same. In terms of sheer "performance numbers", this model is equal to model 2.3. The coefficient for *closeness of election* remains non-statistically significant, but the

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[2] Although it have been debated how well logistic regression models account for interactions naturally when excluding interaction terms (see Rainey, 2014).

interaction term is now statistically significant with a (at p=.008). The coefficient for gender is now statistically significant again. All other variables have the same direction of effect and statistical significant levels as before. When I estimate the marginal effect again (Figure 13), we see that the distinctive relationship between *closeness of election* and *electoral experience* found in model 2.4 now is gone. This time there seems to be no relationship between the margin of victory and voting. The marginal effect is not statistically significant for individuals with *electoral experience* less than 8. Although the marginal effect is statistically significant when electoral experience is between 10 and 20, the effect size is negligible.
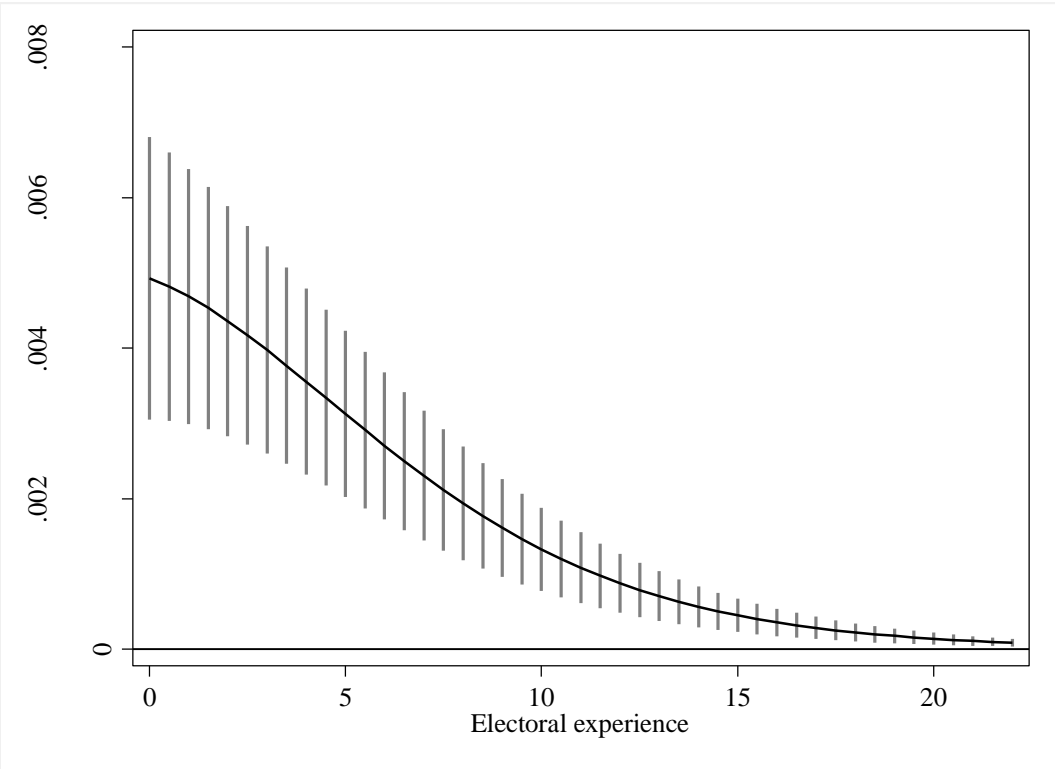
Figure 12: The marginal effect of closeness by electoral experience when *not including* motivational factors in the regression model (model 2.4)
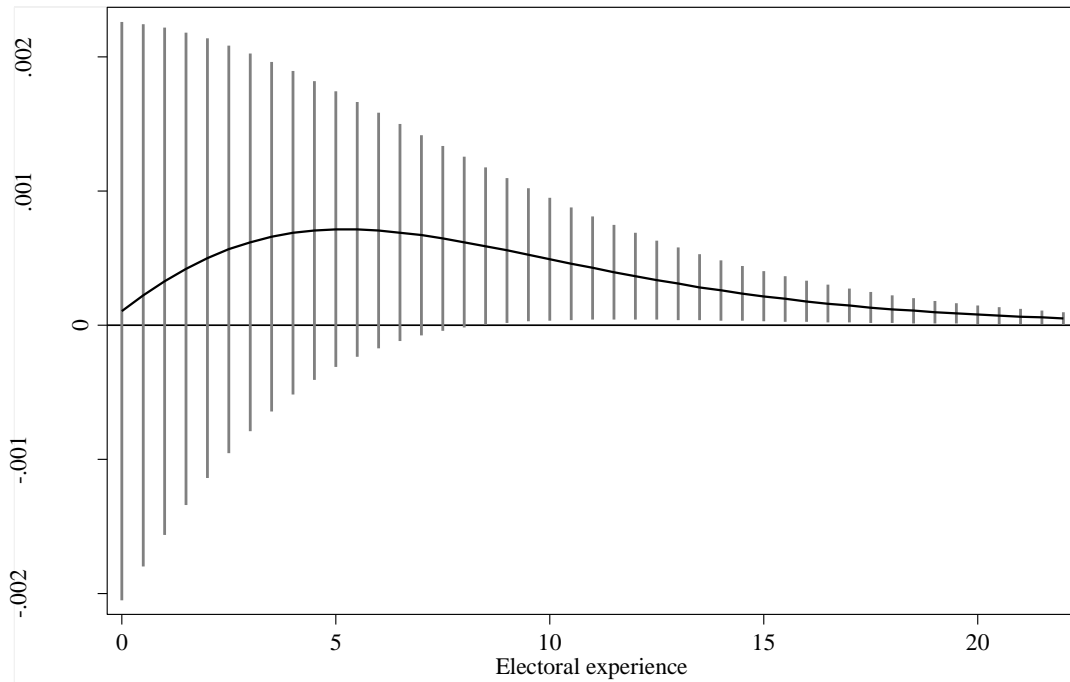
Figure 13: The marginal effect of closeness by electoral experience when *including* motivational factors in the regression model (model 2.5)

Next, we turn to model 2.6, where I include a three-way interaction term between *closeness of election*, *information seeking* and *electoral experience* (Table 6). Here I am interested in testing my hypothesis that the effect of closeness is conditional on information seeking. In other words, whether *information seeking* moderates the effect of closeness of election, still assuming that it is conditional on *electoral experience*. This means that I expect the dependency between closeness of election and electoral experience we saw in Figure 12 to be true for voters that are more than average engaged in campaign, but not for others. This model performs similarly to 2.5 in terms of goodness-of-fit. Although deviance is reduced from 19796 to 19776 and AIC from 19830 to 19816, BIC increases from 1974 to 19986. The variance of the country- and district-level intercept is virtually the same, as is the intra-class correlation. Including the interaction terms, in other words, does not explain any more or less of higher-level variance. The variable for gender is now not statistically significant. The coefficient for *closeness of election* is now statistically significant with a p value of .011, but in the opposite direction (odds ratio of .994). Interestingly, the coefficients for *information seeking* and *experience* is no longer statically significant, but the interaction term between closeness and *information seeking* is.

How closely did you follow the election?
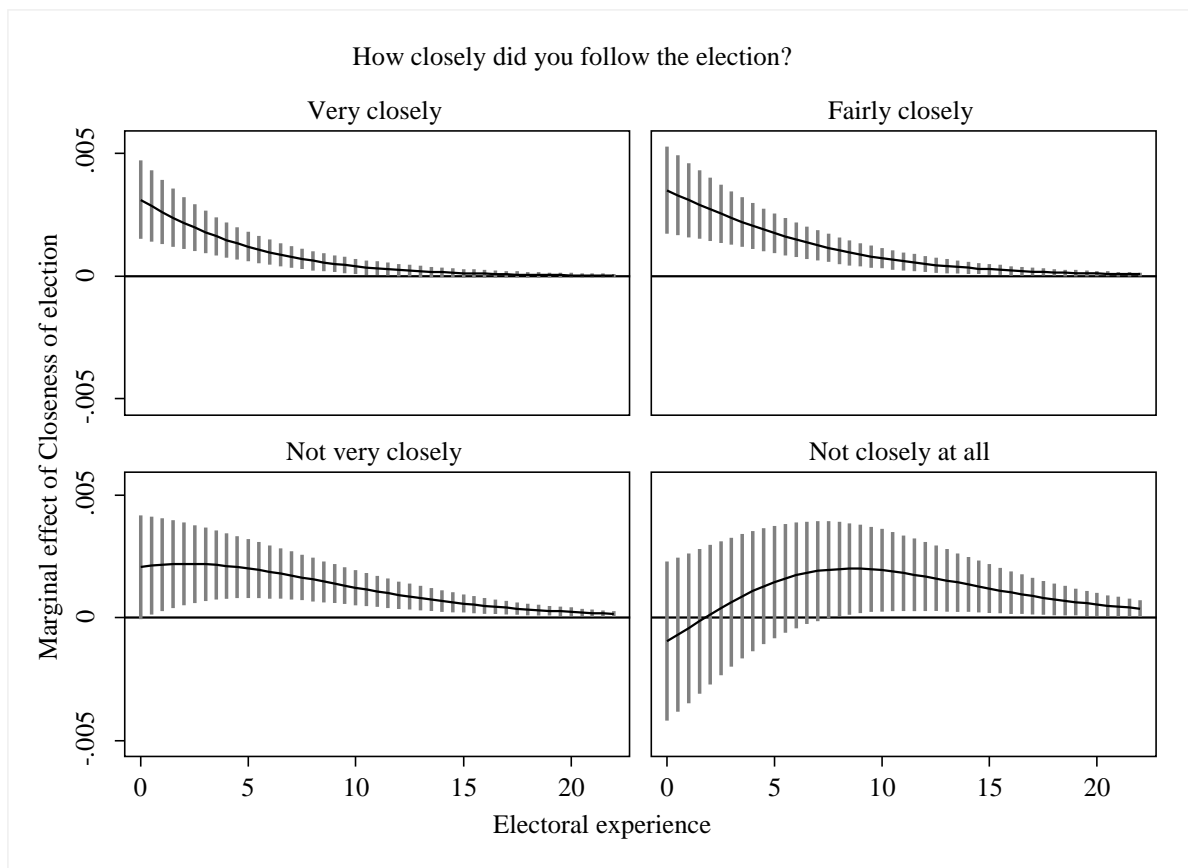
Figure 14: The marginal effect of closeness by electoral experience and information seeking (model 2.6)

I have plotted the marginal effect of closeness by *electoral experience* over different values of *information seeking* in Figure 14. The x-axis is again the level of electoral experience and the y-axis is the estimated mean marginal effect. Each of the four subplots represent one of the four levels of *information seeking*. The plot support my hypothesis that the effect of closeness is conditional on habit and information seeking: The average marginal effect of closeness is statistically significant and increasing as the electoral experience decreases for voters that followed the campaign, in contrast to those that did not. The effect is either not statistically significant or negligible for individuals that did not follow the campaign at all. The effect size, however, is quite small. The average adjusted predictions from model 2.4 are presented in Table 7. A "close elections" are elections with a margin of victory of less than five percent. Individuals with "no experience" are those that did not vote in the previous election. We would expect the probability for someone with no experience that followed the campaign very closely would be marginally higher in a close election. The predicted probability of voting for this group only increase by 2.1 percentage points from 88.6 percent to 90.7 percent when the election is close.

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In other words, the analysis support the hypothesis that there is a conditional relationship between closeness of election, information seeking and electoral experience.

Table 7: Predicted probability of voting by electoral experience, closeness of election and information seeking based on model 2.5 (SD in parenthesis)

		How closely did you follow the campaign?	
		Not closely at all	Very closely
No experience	Close	.292 (.041)	.907 (.016)
No experience	Not close	.386 (.043)	.886 (.020)
Experienced	Close	.742 (.035)	.979 (.004)
Experienced	Not close	.777 (.031)	.974 (.005)

Average adjusted prediction estimated from model 2.5; Standard Errors in parentheses.
Note: "Close" and "not close" refers to closeness of election. An election is
"close" if the margin of victory is less than five and "not close" otherwise.
"Experienced" voters are voters that reported they voted in the last election,
and people with "no experience" are those that did not.

5.2 Closeness of election and information seeking

The regression models above supports the proposition that the effect of closeness is contingent on habit and interest in the election. However, I also expect the relationship between closeness of election and information seeking to be more complex. In this section I examine the direct relationship between closeness of election and information seeking proposed in hypotheses 3 and 4. I use a multilevel linear regression models with level of information seeking as dependent variable. To recall, the *information seeking* variable is a four point scale measuring to what extent the respondent followed the campaign[3]. I expect closeness of election to increase information seeking more generally for high-resource individuals (hypothesis 3), following the resource model of participation. In addition, I expect that voters that are close to a particular party may seek out more information about the campaign in a close election because of group-based loyalties (hypothesis 4). This is an iteration of the mobilization hypothesis more rooted in cognitive psychology than sociology: A closer election increase the outcome at stake for political parties, triggering "root for the team"-like behavior for people that feel attached to some particular party.

5.2.1 Reference models (3.0, 3.1 and 3.2)

All reference models are shown in Table 8. Model 3.0 is the null model, which only contains the variance within and between countries. The deviance is 85914.9, the AIC 85922.9 and BIC 85956.9. These numbers serve as baseline reference for the models 3.2-3.3. The intra-class correlation is 9.8 percent at the country-level and 14 percent at the district-level. As expected, this indicates that the proportion of variance at the country- and district-level is more than enough to warrant a multilevel model. The variance within districts is .032 and the variance within countries is .072. In model 3.1 I introduce the first-level variables *attachment to party*, *efficacy*, *electoral experience*, *age*, *age squared, higher education* and *female*. I use the same

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[3] As noted in the method section, having a four valued scale as dependent variable is often problematic when using a linear model because the residual are not normally distributed. I originally intended to employ a multilevel ordered logistic regression, but the available out-of-the-box procedures either used days to estimate or failed to converge with the more complex interaction models. I have rerun the analysis with multilevel logistic regression after recoding *information seeking* into a binary variable, which yields the exactly the same results (including quantities of interest).

explanatory variables as in the voter model. AIC and BIC in this model decreases from 85923 and 85957 to 79879 and 79897, respectively. Deviance decreases from 85915 to 79878. This all confirms that model 3.1 is a better fit. The within-district variance is about same, down from .30 to .32. The within-country variance, however, decreased a bit more from .072 to .054. In accordance with our expectations, Intra-class correlation went from 98 percent to 88 percent at the country-level and from .143 to .137 at the district-level. All these numbers indicate that model 3.1 is a better model than the null model. Although my focus here is on closeness of election, the individual-level variables that also were statistically significant predictors in the turnout model is also statistically significant here: *Attachment to party, efficacy, electoral experience, age* and *higher education* have positive and statistically significant coefficients. This indicates that the effects of these variables on the decision to vote also can be indirect.

Table 8: Multilevel linear regression models of closeness of election and information seeking

Dep. Var.: Information seeking	Model (3.0)	Model (3.1)	Model (3.2)
	Coef. (p)	Coef. (p)	Coef. (p)
Fixed effects			
Closeness of election			.0024 (.061)
Attachment to party		.2369 (.000)***	.2366 (.000)***
Efficacy		.0729 (.000)***	.0730 (.000)***
Experience		.0286 (.000)***	.0286 (.000)***
Age		.0034 (.000)***	.0033 (.012)*
Age (squared)		-.0000 (.009)**	-.0000 (.000)***
Higher education		.1720 (.000)***	.1717 (.000)***
Female		-.1577 (.000)***	-.1574 (.000)***
District magnitude			-.0016 (.207)
Compulsory voting			.2235 (.114)
Proportional Repr.			-.2752 (.011)*
FH rating			-.3544 (.000)***
Random effects			
var (u_{0j}): intercept (cntry)	.072	.054	.056
var (u_{0jk}): intercept (dist.)	.032	.030	.026
Model summary			
ICC (rho)=cntry. lvl.	.098	.088	.093
ICC (rho)= dist. lvl.	.143	.137	.136
$-2LL$	85914.92	79877.60	79811.78
AIC	85922.92	79899.6	79843.78
BIC	85956.87	79992.98	79979.6
n=resp./dist./countries.	35913/1444/26	35913/1444/26	35913/1444/26

***p<0,001; **p<0,01; *p<0,05, two-tailed.

Model 3.2 includes both the district- and national-level variables. I added *closeness of election, district magnitude, compulsory voting, PR* and *Freedom House* rating. These variables—with the exception of closeness of election—are not included as predictors of interest per se. This makes the AIC decrease from 79878 to 19812 and BIC decrease from 19993 to 19980. The deviance also went down from 79878 to 79812. The within-district variance decreased from .030 to .026, while within-country variance actually increase slightly from .054 to .056. The proportion of variance at the district level is about the same as in model 3.1. Overall, introducing the higher-level variables increases the goodness-of-fit. *Proportional representation* and *Freedom House rating* have negative and statistically significant slope coefficients. Interestingly, this indicates that individuals in PR countries on average pay much less attention to the election than those in majoritarian and mixed systems.

Table 9: Multilevel linear regression on information seeking, with interaction terms

Dep. Var.: Information seeking	Model (3.3)	Model (3.4)
	Coef. (p)	Coef. (p)
Fixed effects		
Closeness of election	.0018 (.235)	-.0009 (.733)
Attachment to party	.2367 (.000)***	.1561 (.003)**
Efficacy	.0723 (.000)***	.0728 (.000)***
Experience	.0275 (.000)***	.0029 (.826)
Age	.0031 (.019)**	.0030 (.021)*
Age (squared)	.0000 (.000)***	-.0000 (.000)***
Higher education	-.0195 (.830)	.1709 (.000)***
Female	-.1569 (.000)***	-.1569 (.000)***
Closeness * Exp.	.0000 (.902)	.0007 (.015)*
Closeness * Higher Edu.	.0041 (.051)	
Close. * Exp. * High. Edu.	-.0005 (.100)	
Exp. * Higher Edu.	.0223 (.064)	
Closeness * Att.		.0024 (.047)*
Closeness * Att. * Exp.		-.0005 (.002)**
Att. * Exp.		.0171 (.011)*
District magnitude	-.0015 (.212)	-.0016 (.193)
Compulsory voting	.2254 (.116)	.2198 (.111)
Proportional Repr.	-.2729 (.014)*	-.2770 (.009)**
FH rating	-.3606 (.000)***	-.3439 (.000)***
Random effects		
var (u_{0j}): intercept (cntry)	.058	.053
var (u_{0jk}): intercept (dist.)	.026	.026
Model summary		
ICC (rho)=cntry. lvl.	.095	.088
ICC (rho)= dist. lvl.	.138	.131
$-2LL$	79805.49	79791.64
AIC	79845.49	79831.64
BIC	80015.27	80001.42
n=resp./dist./countries.	35913/1444/26	35913/1444/26

***p<0,001; **p<0,01; *p<0,05, two-tailed.

5.2.2 Interaction models (3.3 and 3.4)

Now we turn to model 3.3. This model includes an interaction term between *closeness of election*, *electoral experience*, and *higher education*. I hypothesized that high-resource individuals without the habit of voting should pay more attention to the election when then race is close (hypothesis 4). In terms of goodness-of-fit, introducing the interaction term does not make much difference if we compare it to model 3.2. The AIC and BIC goes from 79844 and 79980 to 79845 and 80015, respectively. The deviance decreases slightly from 79812 to 79805. Intra-class correlation goes from 9.3 percent at country-level and 13.6 percent at the district-level to 5.8 percent and 13.8 percent, respectively. In short, the higher-level variance measures stays the same. Regarding the covariates, the slope coefficient for the interaction term is statistically significant, while not *higher education*. Note that this is normal in a linear model with interactions terms because the coefficient now refers to slope when all the variables it interact with are zero, so the statistical significance test is only based on the regression surface in this peculiar region of the predictor space. To interpret the interaction term I have plotted point estimates of the marginal effect of closeness by electoral experience and higher education in Figure 15. The marginal effect in a linear regression is equivalent to the relevant conditional slope coefficient. In line with my hypothesis, we see that the marginal effect closeness of election only is statistically significant for individuals with higher education and electoral experience lower than 7. In addition, for this group we see that the effect size is clearly increasing as the electoral experience decreases.

Now we turn to model 3.4 (Table 4), where I include an interaction term between *closeness of election*, *electoral experience*, and *party attachment*. I expect that closeness of election have a higher marginal effect on information seeking for individuals that feel close to a particular party and that have not gained the habit of voting (hypothesis 4). The goodness-of-fit is better than 3.3, but more or less the same as the reference model (3.2). The AIC and BIC are now 79832 and 80001 compared to 79844 and 79980 in 3.2. The deviance decreased from 79812 to 79792. There was a slight decrease in the proportion of variance at the higher levels. The intra-class correlation is now 13.1 percent for the district-level and 8.8 percent for country-level down from 13.6 and 9.3 before. The variance of the country-level varying intercept went from .056 to .053 and stayed the same for the district-level. Now the coefficient for electoral experience
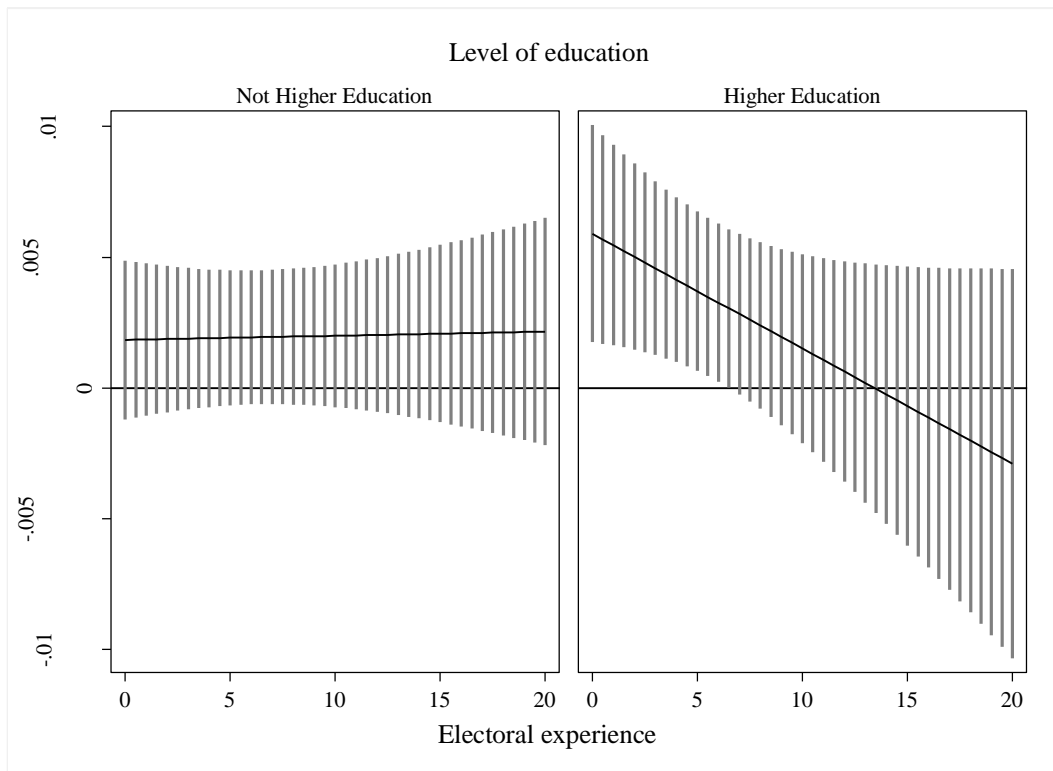
Figure 15: The marginal effect of closeness of election on information seeking by electoral experience and education (model 3.3)
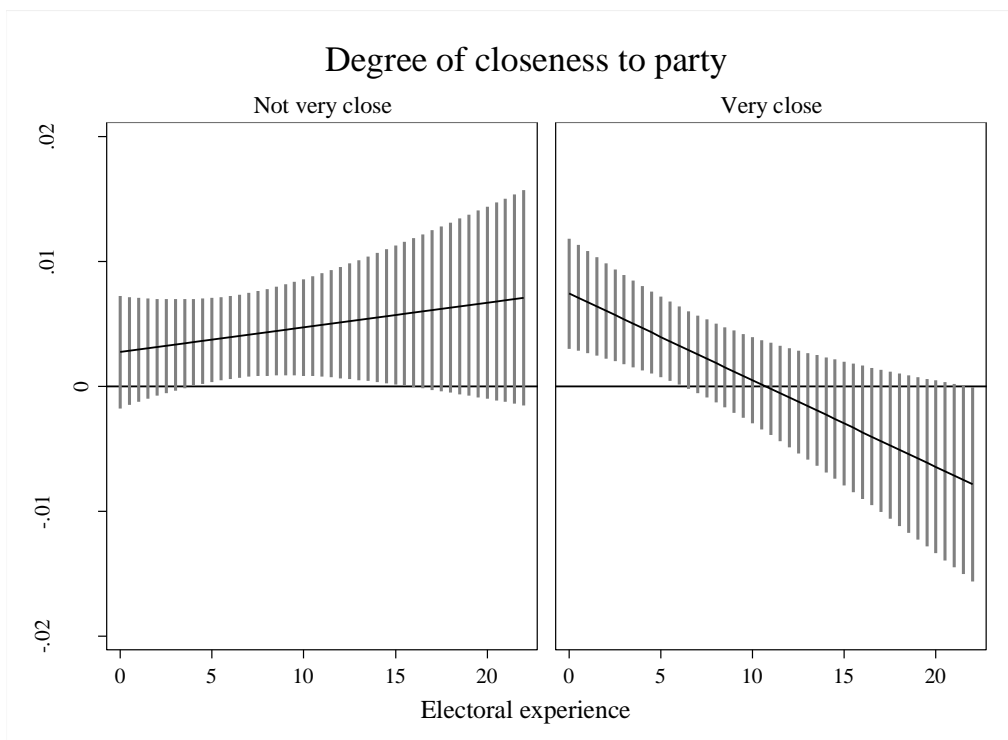
Figure 16: The marginal effect of closeness of election on information seeking by degree of closeness to party and electoral experience (model 3.3)

is not statistically significant, but all interaction terms are. Otherwise the main effects are the same as before. I have plotted the marginal effect of closeness of election by *electoral experience* and *party attachment* in Figure 16. The variable for *party attachment* have tree values, so here I leave out the middle value (the marginal effect was not statistically significant, i.e. all bands overlapped the y-axis). We see the exact same pattern as expected: The effect of closeness is primarily statistically significant only for individuals with that feels very close to a party with little electoral experience. The effect is also statistically significant for those with an electoral experience between 5 and 15, however with a smaller magnitude of effect. This is in line with my hypothesis that those with close attachment to a party—and with little electoral experience—will pay more attention to the campaign in close election.

5.3 Causal Mediation Analysis

Identifying whether there is a *causal* mechanism as proposed in hypothesis five is no easy trivial matter. My conjecture is that closeness of election also affects the decision to vote *through* information seeking (H5). The results from the regression analysis above suggests that the closeness of an election affects information seeking directly. The crucial step from the previous section to here is that I ask whether effect of closeness on information seeking also translates into a higher chance of someone casting a ballot. In other words, I have estimated the effect of *closeness of election* and *information seeking* on voting, and the effect of *closeness of election* on *information seeking*. By joining the two parts of this causal chain, I will now test if *information seeking* mediates the relationship between closeness of election and voting. As outlined in the method section, I estimated the average causal mediation effect or "ACME" (Imai et al., 2010a; Imai et al., 2010b; Imai et al., 2011). The ACME can be interpreted as the difference in the probability of voting when *information seeking* takes the value it would realize under a close election as opposed to one that is not, while other control variables are held constant. The direct effect of closeness on voting (average direct effect, or ADE) is the expected difference in probability of voting when the closeness of an election is changed but *information seeking* is held constant. The sum of the two is the average treatment effect (ATE).

There are two cautions I must repeat once more before proceeding with the analysis. First, the mediation package in R which implement the ACME estimation does not yet support multilevel models with more than two levels (Tingley et al., 2013). This means that the outcome and mediator regression models used for this estimation, although they are equal to model 2.6 and 3.4 otherwise, are treated as two-level models only accounting for country as the second level (and hence does not have a varying intercept at the district-level as well). Second, and more important, because I evidently did not observe whether the same respondent's voted in both or either a close and non-close election, I rely on the assumption of sequential ignorability. Assuming sequential ignorability means assuming (i) that the effect of closeness of election— conditional on the other covariates—is independent of the *information seeking* and *voting* outcome, and (ii) that *information seeking* is independent of *voting* given the other covariates. In other words, that *information seeking* and *voting* is distributed in the sample as they would be if they were randomly assigned across close and none-close elections (given the control variables). The advantage of the ACME procedure by (Imai et al. (2010b)) is that it includes sensitivity analyses which can test the robustness of this assumption. However, such sensitivity

analyses are not available when using multilevel or logistic regression models as of yet. My ACME results must therefore be reviewed with this in mind.

Table 10: Two-level regression models used as outcome and mediator model in the mediation analysis compared with three-level regression models

	Mediator model	Model 3.2	Outcome model	Model 2.3
	Only district-level	District- *and* country-level	Only district-level	District- *and* country-level
	Multilevel linear model Dep.var.= *Information seeking*		Multilevel logistic model Dep.var.= *Voting*	
	Coef. (*p*)	Coef. (*p*)	OR (*p*)	OR (*p*)
Fixed effects				
Closeness of election	.0030 (.023)*	.0024 (.061)	1.008 (.074)	1.007 (.186)
Attachment to party	.0280 (.000)***	.2366 (.000)***	1.547 (.000)***	1.556 (.000)***
Information seeking			2.046 (.000)***	2.057 (.000)***
Efficacy	.0717 (.000)***	.0730 (.000)***	1.173 (.000)***	1.175 (.000)***
Experience	.0281 (.000)***	.0286 (.000)***	1.243 (.000)***	1.245 (.000)***
Age	.0032 (.001)***	.0033 (.012)*	1.040 (.000)***	1.040 (.000)***
Age (squared)	-.0000 (.025)*	-.0000 (.000)***	.999 (.000)***	.999 (.000)***
Higher education	.1771 (.001)***	.1717 (.000)***	1.673 (.000)***	1.668 (.000)***
Female	-.1568 (.002)**	-.1574 (.000)***	1.076 (.050)	1.079 (.044)*
District magnitude	.0014 (.156)	-.0016 (.207)	1.004 (.185)	1.003 (.421)
Compulsory voting	.3026 (.936)	.2235 (.114)	5.857 (.000)***	5.860 (.000)**
Proportional Repr.	-.3028 (.161)	-.2752 (.011)*	.699 (.276)	.732 (.343)
FH rating	-.5186 (.006)**	-.3544 (.000)***	.621 (.003)**	.613 (.004)*
Random effects				
var (u_{0j}): intercept (cntry)	.211	(.056)	.498	(.484)
Model summary				
ICC (rho)=cntry. lvl.	.280	(.093)		(.126)
$-2LL$	80068.99	79811.78	19824.2	19802.932
AIC	80214.18	79843.78	19854.2	19834.93
BIC	80341.52	79979.6	19981.5	19970.75
n=resp./dist./countries.	35913/1444/26	35913/1444/26	35913/1444/26	35913/1444/26

***p<0,001; **p<0,01; *p<0,05, two-tailed.

The mediator and outcome model are presented in Table 10. The mediator model is the model with information seeking as dependent variable (based on model 3.2), and the outcome model is the model with voting as dependent variable (based on model 2.3). Because the ACME estimation does not support more than two levels, I compare them to the "correct" model with three levels to see if there is any sign major discrepancies. The first notable thing about the mediator model is that *closeness of election* is statistically significant contrary to the three-level model, while *proportional representation* is not. This is probably due to underestimated

standard errors, because the model do not account for the fact within-district cluster are correlated (Hox, 2010: 5; Stevens, 2012), which was what I feared. This means that the ACME estimates probably are biased in the same way and hence much less reliable. The second notable thing is the increase in proportion of variance at the country-level. This is of course not that unexpected, because the correlation between closeness and information seeking measured on respondents from the same district are not accounted for in the same way. Otherwise, the model is more or less completely the same. The outcome model, however, does not seem to suffer (on the outset, at least) from the same problem, although the *p*-value of closeness indeed is lower.
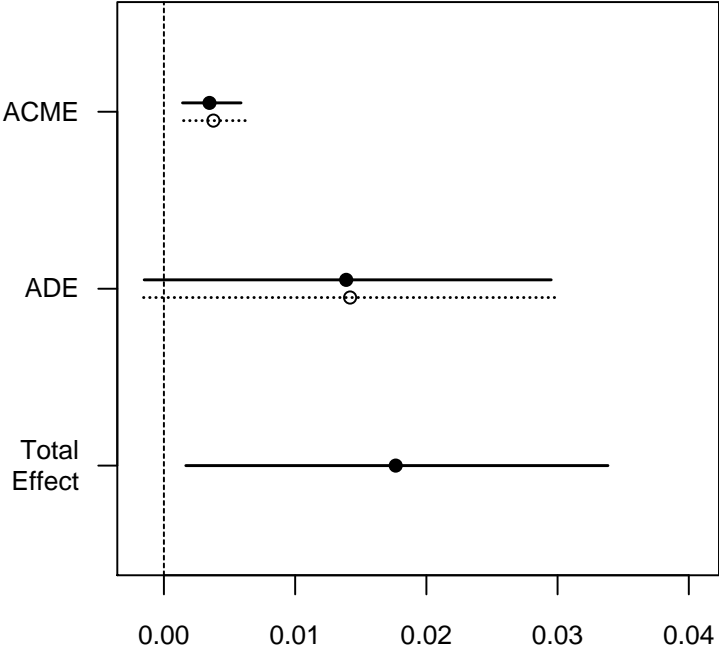
Figure 17: ACME point estimates of mediation analysis of closeness of election and information seeking
Black bars represent 95 percent confidence intervals. The ACME is the estimated indirect effect of closeness on voting *through* information seeking, the ADE is the estimated direct effect, and the sum of the two is the total effect.

Table 11 shows the results from the mediation analysis. The point estimates are also displayed graphically in Figure 17. They report the direct effect of closeness of election on the probability of voting (ADE), its indirect effect via information seeking (ACME), and the total effect. Because the "treatment" variable is continuous, the ACME point estimates are split between a

"control" and a "treatment" value, and the average between two are estimates plotted. "Treatment" is set at 50, i.e. a margin victory near zero, and "control" at 30, i.e. a margin of about 20 percentage points. In line with my hypothesis, closeness of election also seem to exercise a statistically significant effect through information seeking, while it does not seem to have any direct effect. Although this is in favor of the theory presented earlier, it can merely be suggestive evidence, if anything, because of the many pitfalls.

Table 11: Causal Mediation Analysis of closeness of election and information seeking

Outcome: *Voting* , Treatment: *Closeness*	Model (4)
Mediator: *Information seeking*	Coef. (*p*-value)
Point Estimates	
ACME (not close election)	.00377 (0.00)***
ACME (close election)	.00347 (0.00)***
ADE (not close election)	.01419 (0.09)
ADE (close election)	.01389 (0.09)
Total Effect	.01766 (0.04)
Prop. Mediated (not close election)	.20726 (0.04)
Prop. Mediated (close election)	.18872 (0.04)
ACME (average)	.00362 (0.00)***
ADE (average)	.01404 (0.09)
Prop. Mediated (average)	.19799 (0.04)
Model summary	
Simulations	1000
n=respondents/countries	35913/26

***p<0,001; **p<0,01; *p<0,05.

Note: Outcome and mediator model are specified in Table 10. "Close election" = 50 (i.e. zero margin), "not close election" = 30 (i.e. a margin of 20 percentage points).

6. DISCUSSION AND CONCLUSION

In this chapter, I summarize the results from the analysis and present the conclusion. I start by evaluating the results for each hypothesis. After that I provide a conclusion.

6.1 Summarizing the results

In short, the analysis found support for all hypotheses except one. Table 12 provides an overview of the hypotheses and their subsequent faring in the analysis. In other words, closeness of election can have an impact by different mechanisms for different people. Here I briefly discuss the results for each hypothesis in turn.

My first hypothesis was on the role of habit, and how it should affect the way closeness of election can have an impact on behavior. I built on the developmental framework of voting, and argued that *closeness of election should not affect those with the habit of voting* (H1). This hypothesis was applicable to all other test as well, all which lend support. Throughout the analysis, the role of habit seems clear: The more electoral experience the respondent had, the less likely it was for the closeness of election to have an impact. This highlights the important contribution of the developmental framework of voting. *Inertia* shapes the role of the context.

Table 12: Hypotheses and results

	Hypothesis	Theory	Variables	Evaluation
1	Closeness of election does not affect the decision to vote for those with the habit of voting	DM	*Electoral experience*	Supported
2	Closeness of election affects the decision to vote for people that are interested in the election and do not have a habit of voting	HSM	*Information seeking*, *electoral experience*	Supported
3	Closeness of election affects the interest in the election for people with higher education that does not have a habit of voting	MH, RM, DM	*Education*, *electoral experience*	Supported
4	Closeness of election affects the interest in the election for people that feel close to a political party and that do not have a habit of voting	MH, HSM	*Party attachment*, *electoral experience*	Supported
5	Closeness of election affects voting through interest in the election	(combined)	*Information seeking*	Inconclusive

Note: DM = the developmental model of voting, HSM = heuristic-systematic model of information processing, RM = the resource model of participation, MH = the mobilization hypothesis

My next hypothesis was that closeness of election affects the decision to vote for people that are interested in the election and do not have a habit of voting (H2). This also found support. There was clear difference between those who reported that they followed the election "very closely" or "fairly closely", and those followed it "not very closely" or "not closely at all". For those least interested closeness clearly did not matter. For those who followed it "very closely" and "fairly closely" the role of electoral experience was underscored. The less electoral experience, the more does closeness of election affect the decision to vote.

I also expected that *people with higher education that does not have a habit of voting should be more interested in election when the race is close* (H3). The results clearly show a fundamental difference between those with higher education and those without. For those with little electoral experience and higher education, a closer election seems to fuel more interest. This is key because of the importance of *interest* for participation. Even Brady et al. (1995: 283) recognizes its importance over other variables when it comes to voting.

The third hypothesis was that closeness of election should affect the interest in the election for people that feel close to a political party and that do not have a habit of voting (H4). When calculating the marginal effect and contrasting those who feel "not very close" with those that "feel "very close", we see this exact thing. Those that feel very close to a party and have little electoral experience clearly seems to get more interested in a closer election. Noticeable, however, closeness of election also have statistically significant effect on those who are not close to a party with a medium degree of electoral experience. The effect size is very small, also relative to how much it seems to affect those who are very close to a party, but in the bigger picture, both are quite small anyway.

My last hypothesis was that *closeness of election should affects voting through interest in the election* (H5). Although the results from the mediation analysis lends support, the limitations make it too difficult to reject the null hypothesis. The two-level regression models used for the estimation seemed to influence by the intra-district dependencies. Contrary to the three-level regression model that accounted for this, the two-level model had a statistically significant coefficient for closeness of election. This is probably because the model underestimates the standard errors by not accounting for the intra-group dependencies. Although the ACME procedure uses robust standard errors, the end result is probably very biased because of this. This in addition to that I cannot test sensitivity for the sequential ignorability assumption, means that I regard it as inconclusive.

6.2 Conclusion

That close elections increase turnout at the aggregate-level is arguably the most consistent finding in the turnout literature. However, individual-level studies rather consistently tend not to find any connection between the closeness of an election and someone's decision to vote. Although we know that more people tend to vote in a closer election, we lack an empirical foundation for why that might be. The aim of this thesis was to uncover the reason for this seemingly puzzling inconsistency. Ferejohn and Fiorina (1975: 920) declared that "closeness counts only in horseshoes and dancing". Instead of asking if closeness of election affects the decision to vote, I therefore went searching for horseshoes: When does closeness count? More specifically, I asked for *whom*. My rationale was that closeness of election might affect the decision to vote by different mechanisms for different people. In other words, that closeness only counts for some. Using the developmental framework of voting, the resources model of participation and the heuristic-systematic model of information processing, I argue that *interest* and *habit* are two integral parts of the equation. I proposed five different hypotheses for whom closeness might count and why that is. I hypothesize that (i) although closeness of election only can affect those without a habit of voting; (ii) it fuels cognitive engagement for those that are interested in the election; and (iii) it fuels interested in the election for those who are educated or (iv) feel close to a particular party. It should also (v) affect the decision to vote indirectly through interest in the election. To test these hypotheses I used multilevel regression models and mediation analysis on cross-national survey data from the CSES. I also calculated closeness of election in a way so that it is comparable across different electoral systems by combining district- and national-level margins. The results lend clear support for hypotheses i-iv. The results indicate an intricate relationship between the closeness of election, interest and habit. While closeness may help shape participation by altering the role of interest, habit shapes the role of closeness of election. Closeness of election can work as electoral supply of opportunity, but only in the right circumstances. The results clearly supports this notion that closeness of elections can affect the decision to vote, sometimes. Indeed, the regression models indicate no statistically significant relationship when looking at the mean, only when examining the conditional relationships. The reason most individual-level studies do not find any link, is that they do not look.

My thesis makes two general contribution to the turnout literature. Firstly by proposing to measure district- and national-level closeness simultaneously. There have been a push towards

the use of district-level as opposed to national-level measures, but for some reason this proposition of combining them have not been discussed in the literature. Secondly by giving an empirical foundation for the role of closeness of election in shaping participation. Although the scope tested here is narrow, the results support a broader idea: Closeness of election affects participation by different mechanisms for different people. Arguing that some aggregate-level change is due to a change in the universal decision calculus really says very little about very little.

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8. APPENDIX

Plots of residuals and fitted values for the linear regression model (Model 3.2).

Graphs by Country