UNPACKING SECOND-ORDER ELECTIONS THEORY: VOTE CHOICE AND TURNOUT
IN EUROPEAN ELECTIONS

by

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(Under the Direction of Cas Mudde)

ABSTRACT

Shortly after the first European elections were held in 1979, scholars noted that, relative to
national elections, governing and large parties tend to lose votes, smaller (and ideologically
"extreme") parties gain votes, and turnout is significantly lower (Reif and Schmitt 1980; Reif
1984). They therefore classified European elections as "second order national elections". Second
order elections (SOE) theory has since become one of the most important theories of European
comparative electoral behavior. The general aim of this thesis is to test and unpack the key
arguments of this 'grand' theory of European elections, which has become a sort of received
wisdom in the field of European comparative electoral behavior over time. In particular, I
concentrate on (1) the determinants of governments' performance in European elections; (2) the
determinants of performance of other, particularly extreme and radical, parties in European
elections; and (3) the determinants of variation in turnout in European elections. I put special
emphasis on the effects of election timing, party size, as well as some novel variables, such as
coalition composition.

INDEX WORDS: European elections, Second-order elections, electoral behavior, vote choice,
turnout, election timing, party size, political parties

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DEDICATION

To my grandmother, Helena (RIP) and my parents, Barbara and Michael

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INTRODUCTION

With the introduction of popular elections to the European Parliament in 1979, the citizens of the member states of the then European Economic Community (now European Union, EU) were given the possibility to vote in a brand-new electoral contest. The first scholars to analyze these elections observed that European elections are different from elections to national parliaments. Writing shortly after the first European elections, Reif and Schmitt (1980) noted that, relative to national elections, governing and large parties tend to lose votes, smaller (and ideologically "extreme"[1]) parties gain votes, and turnout is significantly lower (see also Reif 1984, 1997; Schmitt et al. 2020). They therefore classified European elections as "second order national elections".

Second order elections (SOE) theory has since become one of the most important theories of European comparative electoral behavior. Many scholars have attempted to revisit, test, and elaborate upon the theory. These works mainly observed similar general patterns in following European elections (e.g. van der Eijk and Franklin 1996; Marsh 1998; Hix and Marsh 2007, 2011), and although there have been challenges to some of the propositions of SOE theory (e.g. Curtice 1989; Carrubba and Timpone 2005; Hobolt et al. 2009; Clark and Rohrschneider 2009;

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[1] SOE theory uses the terms "radical" and "extreme" interchangeably, even though the empirical literature makes a distinction between the two (e.g. Ignazi 1992; Kitschelt and McGann 1995; Mudde 2007). In the rest of the text, for practical reasons, I will use solely the term "extreme", also given that the most recent article of one of the authors of the theory uses this term (Schmitt et al. 2020).

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de Vries et al. 2011; Hobolt and Spoon 2012), the model remains the dominant perspective on European elections (van der Brug and de Vreese 2016, 2).

While the importance and plausibility of SOE theory have led to an extensive amount of research in the years following its introduction, it has now almost become received wisdom, a truism that is mentioned "in passing" rather than assessed. This has led to a failure to address some of the theoretical and empirical weaknesses of the theory. Moreover, a plethora of its proposed   mechanisms, as well as many implications that stem from SOE theory, remain unexplored. These issues have become even more evident with the significant shifts in European politics that have occurred since the early 1980s, when was the theory formulated. Consequentially, the main aim of this study is the elaboration and refinement, but also the unpacking, of SOE theory. In addition, it aims to address some of the ramifications for other aspects of comparative electoral behavior.

Theoretical Issues

There are at least three broad theoretical issues with SOE theory's key general propositions. First, it remains somewhat unclear whether all government parties should experience losses in European elections, especially given other predictions of the theory and its elaborations (e.g. Curtice 1989; van der Eijk and Franklin 1996; van der Eijk et al. 1996; Ferrara and Weishaupt 2004; Kousser 2004; Hix and Marsh 2007, 2011; Weber 2011). SOE theory predicts that government and large parties suffer losses in European elections and small parties gains. This may be problematic given that there are many small government parties and large non-governmental (opposition) parties.

But SOE theory also predicts that the magnitude of government party losses is dependent upon the temporal position of European elections in the national election cycle (NEC), building mainly on the logic from midterm elections in the US (e.g. Goodhart and Bhansali 1970; Tufte 1975; Stimson 1976). Concretely, it argues that government parties should experience gains rather than losses when European elections are held at the very beginning (*honeymoon*) of the NEC, losses when they are held at the middle (*midterm*) of the NEC, yet regain support when a European election takes place towards the very end of the NEC (*late term*).

Finally, SOE theory does not clearly explain how the role of a party *within* a government coalition matters. Are all coalition members equally punished, irrespective of their size and position in the coalition (cf. Downs 1957; Lewis-Beck 1988; Powell and Whitten 1993; Whitten and Palmer 1999; Anderson 2000; Powell 2000; Dorussen and Taylor 2001; Fisher and Hobolt 2010; Hobolt et al. 2013)?

To sum up, it is questionable that the first key general hypothesis of SOE theory, that government parties lose in European elections, truly holds up when we break it down (cf. Geddes 2003). Moreover, it is fairly evident that the theory contradicts itself in some ways.

The second theoretical problem is that we still don't know much about the reasons why some political parties do better in European elections. SOE theory, and the following research, has been mostly rather limited on this point. Particularly, it remains unexplained what role ideological extremity of a party, party size, and election timing have in this respect (cf. Oppenhuis et al. 1996). As noted, the authors of SOE theory posited that European elections should offer new opportunity structures for "extreme" parties (Reif 1984, 1997; see also Schmitt et al. 2020), but barely provided any theoretical reasoning for this. Moreover, it seems that the

theory assumes that smaller parties are more extreme (van der Eijk et al. 1996, 151), or that (all) extreme parties are small, an assumption that is in itself questionable.

Oppenhuis et al. (1996, 297) suggested a relation between election timing and the performance of extreme parties in European elections, proposing that European elections in the *late term* of the NEC set a new "marker" of public opinion and, consequently, are used by voters to signal their discontent with the government performance through voting for small extreme parties, which they referred to as "voting *with the boot*." But their study was based on survey data of only one European election (1989) and the mechanism has since remained unexplored and untested. Moreover, they also seem to assume that (all) extreme parties are smaller than mainstream parties, which is problematic in light of the developments in European party politics of the last decades. To sum up, links between ideological extremity, election timing, and party size and vote choice in European elections require further study.

Finally, the third theoretical issue concerns participation in European elections. Specifically, it is still largely unclear why turnout levels in European elections vary between countries and in time (see e.g. Marsh and Franklin 1996; Franklin 2001; Wessel and Franklin 2009). While SOE theory generally predicts that turnout will be lower in European than national elections, given the lower stakes in the former, it also suggests some variation in turnout in European elections across cases. One proposed determinant of this variation is election timing. However, except for the somewhat obvious proposition that turnout will be higher in the case of same-day elections, SOE theory does not explicitly state whether turnout changes in other stages of the NEC (i.e., *midterm* and *late term*).

Considering findings in research on voter turnout in national elections (e.g. Franklin 2004; Anzia 2014), theories of voter fatigue effects on turnout (e.g. van Egmond et al. 1998;

Rallings et al. 2003), and theoretical propositions of later assessments of SOE theory (Marsh and Franklin 1996), it is reasonable to expect that turnout will vary across these stages. Moreover, SOE theory itself proposes that European elections with a "referendum" aspect will draw more attention of voters (Reif and Schmitt 1980, 18). Given the theory's logic in terms of election timing, only elections held at the *midterm* (and potentially *late term*) should have this quality. Lastly, SOE theory suggests that different electoral procedures in national and European elections as well as the newness of European elections affect turnout, but this remains largely unexplored as well. To sum up, there are many questions around the effects on turnout of election timing, procedural differences, and contextual factors such as the newness of European elections in a country that need to be further addressed and explained.

Empirical Issues

From an empirical point of view, research on (European) SOE has long been limited by a small number of elections and countries. This made it difficult to observe systemic patterns of electoral behavior and (counter)evidence for many propositions of the SOE model remains therefore limited. This has become even more problematic in light of the significant changes that both the EU and the European party systems have undergone since 1980 (see e.g. Wolinetz 1991; Katz and Mair 1995; Ignazi 1996; Mair and van Biezen 2001; Mair 2013; Schmitt et al. 2015; Hobolt and de Vries 2016). The effects of these changes on the SOE theory are often noted but rarely analyzed systematically (cf. Schmitt et al. 2015; Hobolt and de Vries 2016; Hernandez and Kriesi 2016). Even the most recent comprehensive research on the theory is two decades old (cf. Hix and Marsh 2011).

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`Consequentially, I aim to test the propositions drawn from SOE theory and follow-up studies based on the largest possible set of elections and countries (1979-2019). While this extensive timespan gives me the possibility to observe trends across time and to evaluate the theory with greater leverage, it also forces me to limit the analyses to the aggregate level. We simply do not have reliable individual-level data for such a large set of elections and countries (see also Schmitt et al. 2020). Particularly in terms of voter motivations, aggregate-level analyses cannot fully explain the various mechanisms at play. However, it can serve as a first step, providing initial and partial (lack of) support for these mechanisms. These can then later be evaluated through smaller-n studies that can test the micro-level processes with more reliable individual-level data.

Maybe because of its broadness, SOE theory has also suffered from somewhat unclear definitions of some of the important concepts and, connected, measurement issues. This is mainly manifested in the party categorization, especially in terms of *winning* parties. The theory uses terms like 'small', 'extreme', 'radical', 'populist', and 'protest' almost interchangeably, without truly defining them or providing examples or lists of party categorizations. Unfortunately, we do not know whether the authors of SOE theory really believe that 'small', 'extreme', 'radical', 'populist', and 'protest' parties should be clustered in one large category, assuming they are 'functional equivalent', or treated separately.

The last three decades have seen both important transformations in European (party) politics and an extensive body of work on party politics and political extremism (e.g. Ignazi 1992; Betz 1994; Kitschelt and McGann 1995; Mudde and Mair 1998; Mudde 2007; March 2011; Mudde 2019a; de Vries and Hobolt 2020), which can help tease out the conceptual and theoretical conflation and prevent misleading results in our empirical analyses. Consequentially,

I also aim to provide clearly established party categorization criteria, relying on the most recent and comprehensive works in this regard (e.g. Zulianello 2018; 2019a; Rooduijn et al. 2019).

Contributions

The main goal of this study is to elaborate and refine one of the most important theories in European comparative electoral behavior, SOE theory, by solving most of these theoretical and empirical issues. But I also aim to present findings that are important for other theories of comparative electoral behavior. In particular, this PhD thesis should further our knowledge about the impact of election character on vote choice and participation, based on the thesis that voters behave differently in low and high salience elections. Moreover, the importance of election timing goes clearly beyond European elections, as indeed is proved by research on the US (e.g. A. Campbell 1960; Stimson 1976; Anzia 2014) and other legislative elections (e.g. Nordhaus 1975; Franklin 2004). My research should, therefore, also contribute to those literatures and carry possible policy implications (for example, when is it convenient to call a snap election).

Studying the effects of election timing on turnout in European elections can also have more general implications for research on electoral participation, serving as an evaluation of some of its main theories. The topic is mainly mentioned in the literature on US elections (see Niemi and Weisberg 1993, 207–221), especially in both the original and revised versions of *surge and decline* theory (A. Campbell 1960; J. Campbell 1987). Outside of the US, Franklin et al. (1996) have argued that election timing is the best proxy measure for the political importance of an election. They further suggested that including timing and other "motivational factors," rather than "facilitative factors" (see Milbrath and Goel 1977), helps us better understand the

mechanisms behind the variation in turnout (see also Franklin 2004). Considering these arguments, and the rather limited number of more recent studies dealing with the effects of election timing on turnout, especially outside of the US, this research will help to fill this gap in the literature. Indeed, held at different stages in the national electoral cycles, European elections provide a unique opportunity to test election timing effects on turnout.

The emphasis on party size could be particularly important in light of the ongoing transformations in European politics. SOE theory seems to treat all government parties as large parties and all extreme parties as small parties. This was largely correct, and therefore not overly problematic, in the 1980s. But this is no longer the case, as the "extreme" has become "mainstream" in many countries (see e.g. Mudde 2019a; de Vries and Hobolt 2020). In short, several European parties that could and have been classified as 'extreme', 'radical', 'populist', or 'protest' are no longer "niche" (Adams et al. 2006; Meguid 2005; 2008; Jensen and Spoon 2010; Wagner 2011). They have become large, within the relative terms of their particular party system, and therefore also important parties. Moreover, some of them have not just acquired blackmail potential, but also coalition potential (Sartori 1976), which has made them *Koalitionsfähig*, i.e., acceptable government coalition partners (see also Bolleyer 2008; Zulianello 2018, 2019a). Analyzing how these shifts have changed the nature of European elections could tell us more about changes in electoral behavior in national political arenas as well. This can thus be seen as another broader contribution that goes beyond only evaluating SOE theory.

Finally, connected to some of the previous points, European elections can serve as laboratories for studying broader phenomena of electoral behavior (see e.g. van der Eijk et al. 1996). While this point is suggested in some of the initial works, it has not really been explored

much further. This study will evaluate some of the major theories through the analysis of aggregate outcomes of voter behavior in European elections. For instance, studying changes in vote shares between European and national elections of government coalition parties, following SOE logic, can be used to test theories on economic and performance voting (e.g. Lewis-Beck 1988; Powell and Whitten 1993; Whitten and Palmer 1999; Anderson 2000; Powell 2000; Dorussen and Taylor 2001; Nadeau et al. 2002; Fisher and Hobolt 2010; Hobolt et al. 2013; see also Downs 1957). Similar, analyzing the performance of extreme parties, while controlling for government status in European elections relative to national elections, should further our understanding of the impact of the electoral costs of governing for these parties, an inquiry that has become increasingly important in the last three decades (see e.g. Heinisch 2003; Dunphy and Bale 2011; Elias and Tronconi 2011; McDonnell and Newell 2011; Luther 2011; Mudde 2013; Albertazzi and McDonnell 2015).

Organization

The study is organized around the aforementioned three broad theoretical problems. The first chapter deals with differences in voting for government parties in European and national elections. In Hirschman's terms (1970; see also Weber 2011), the chapter thus strives to address the question of when and why some voters remain "loyal" to governing parties in European elections (*loyalty*). The second chapter addresses who the *winners* of European elections are, concentrating in particular on protest voting, i.e. voting for extreme parties (or *voice*). The third chapter examines the issue of turnout in European elections, thus addressing when and why voters abstain in European elections (*exit*). Finally, I summarize the key findings in the

conclusion and address some implications from the three empirical chapters for both SOE theory

and, more broadly, research on European comparative electoral behavior.

CHAPTER 1

Loyalty: The Government Vote in European Elections

The key argument of SOE theory is that there is less at stake in European elections given that the results of these elections do not lead to the formation of a national executive. One of the main consequences, according to the authors of the theory, is that parties in government obtain fewer votes in European elections than in national elections (see Reif and Schmitt 1980, 9–10). This proposition (i.e. "government parties lose"), which arguably has gained the most attention in the scholarly debate and is often seen as the key proposition of the theory (e.g. Curtice 1989; Hix and Marsh 2007: 2011), is the subject of the first chapter.

SOE theory clearly envisions its main propositions as related to one another. In terms of the role of national government performance in European elections, this leads to a somewhat confusing mix of mechanisms at play. First, Reif and Schmitt (1980, 10) suggested a relationship between generally lower turnout and voting for governing parties in (European) SOE, arguing that the former diminishes the latter, as voters of governing parties are less motivated to vote in these elections. The part of the electorate that still participates then might cast their ballot for an opposition party, only to signal their discontent with government policies while generally staying loyal to their party in first-order elections (FOE). In the literature, this is referred to by different names, including "strategic protest voting", "strategic defection", or "voice", in Hirschman's famous terminology (e.g. Hirschmann 1970; Weber 2007, 2011; Schmitt et al. 2020).

Second, government parties, and large parties in general, should also be hurt by the expected gains of small and new parties in European elections, as a consequence of more "sincere voting", i.e. voting *with the heart* (Reif and Schmitt 1980, 10). As Reif (1984, 1997) later added, "protest" and ideologically "extreme" parties should profit in (European) SOE as well, although he did not really explain his reasoning for this addition. But, as is clear from the most recent work (Schmitt et al. 2020), SOE theory sees protest voting as another expression of "strategic defection" or voting *with the boot* (Oppenhuis et al. 1996, 302).

Hence, connecting the key points of SOE theory leads to an explanatory mechanism of the anti-government vote in (European) SOE that combines both sincere and protest voting. Moreover, SOE theory does not only predict that parties in government lose votes in European elections, compared to national elections, it also argues, building on models of US midterm elections (e.g. Goodhart and Bhansali 1970; Tufte 1975; Stimson 1976; Kernell 1977), that the magnitude of their losses varies and is conditioned by the timing of European elections in terms of the national electoral cycle (NEC), which is, of course, different across countries. Consequentially, the decision of voters to stay loyal to a government party in SOE should be mainly driven by the timing of the election. Election timing should also drive which mechanism prevails, i.e. strategic protest voting, sincere voting, or a combination of the two. However, the theory is quite vague in this sense.

In short, the seemingly simple original SOE theory proposition that government parties lose in European elections is much more complicated when election timing and other factors are taken into account. Based on our theoretical elaborations, we have (at least) four different propositions (and plethora of potential mechanisms), which can be analyzed separately:

(1) Government parties lose votes in European elections, compared to the previous national elections.

(1a) Government parties lose votes in European elections, compared to previous national elections, because of lower turnout in European elections.

(1b) Government parties lose votes in European elections, compared to previous national elections, because of the increased amount of strategic protest and sincere voting in European elections.

(2) The losses of government parties in European elections, compared to previous national elections, are affected by election timing in terms of the national election cycle.

To be able to investigate when and why voters stay loyal to governing parties in European elections, it is thus necessary to unpack the general proposition of SOE theory by focusing on both election timing and voter motivations. The disintegration of the one 'big' proposition into multiple 'smaller' propositions, and focusing on the underlying mechanisms underlying the suggested general pattern, should provide us with more testable hypotheses, thus giving us the possibility to better evaluate the plausibility of the initial 'big' proposition (see Geddes, 2003).

In the first, theoretical, part of the chapter, I will discuss the mechanisms underlying SOE theory with regard to government party performance in European elections and deal with some other propositions of the existing literature on the subject. Building upon these, I then develop a variety of clear and testable hypotheses. In the second part, I empirically test these hypotheses on the basis of the results of European elections of the past four decades. The third part discusses the findings and the fourth, and final, part offers a brief summary of the findings and their theoretical consequence.

Theoretical Analysis

The theoretical analysis in this chapter is divided into four parts. First, I discuss the key

explanatory mechanism of the government party vote in European elections, namely the impact

of election timing. Second, I address the general adaptability of SOE theory to multi-party

(European) systems, given that the theory is based on theories on US midterm elections, which

operate in a very different institutional context – most notably a majoritarian electoral system

and, consequently, a two-party system. In terms of the vote for governing parties in European

elections, I am particularly interested in how SOE theory works in the case of government

coalitions. Moreover, following the issues raised in later reactions to SOE theory, I evaluate if

voting for governing parties in European elections is determined by the fact that a party is in the

government, and thus by the proposed cyclical popularity of the government, or (also) by its size,

and thus by the fact that governing parties are usually larger (in the specific terms of the

particular party system). Third, I address other proposed determinants of voting for government

parties in European elections, which could help us to further unpack SOE theory general

proposition, in particular turnout. Finally, I discuss alternative mechanisms proposed in reaction

to SOE theory.

1) Election Timing Effects

The logic of the SOE model in many aspects mirrors the literature on US midterm elections,

which suggests that midterm elections are mostly referendums on the record of the

president/administration (Tufte 1975) and that this evaluation will always be mostly negative

given the natural prevalence of negative over positive impressions (Kernell 1977; see also Marsh 2007, 55-6). More specifically, it builds especially upon the cyclical model of public support (Stimson 1976; see also Goodhart and Bhansali 1970), which suggests that presidential public approval follows a cyclical pattern over time. In this popularity cycle, there first is an upsurge in support, shortly after the presidential election, given the "unrealistic expectations unaccompanied by commitment to the man or his policies" (Stimson 1976, 10). However, this upsurge is followed by a steady decline for about three years, given the disillusion caused by the unfulfillment of these expectations. Finally, there is a recovery towards the end of the term, when support is "bottoming out" and is "reduced to (very roughly) the plurality which elected him" (Ibid.).

The SOE model is almost identical. First, it expects that governments will not experience losses when European elections are held shortly after national elections (*honeymoon*), given the anticipated "'over-confirmation' of the tendencies that characterize the national election results" (Reif and Schmitt 1980, 10). In later assessments of SOE theory, however, Reif made it clear that we should actually observe gains of government parties in these elections, given post-electoral euphoria among the leaders, activists, and voters of the winning parties and the depression and demobilization among the losers (Reif 1984, 246; 1997, 117). It is important to note that this addition is somewhat problematic for the general proposition of the theory, as it suggests that governments may not always lose votes in (European) SOE.

Second, governing parties will suffer the largest decline in support when European elections are halfway the NEC (*midterm*), when their popularity is at its minimum. Reif and Schmitt (1980, 10) argued that this decrease in popularity is caused by the inevitable disappointment of (some) voters with specific policies of governments, the defection of loyal

partisans to apply pressure on the government without necessarily changing their allegiance, and the increased mobilization of opposition voters who see themselves confirmed in their negative views towards the government.

Finally, in European elections held close to the end of the NEC (*late term*), government popularity will increase again, as it should reflect the "true electoral prospects" (Reif and Schmitt 1980, 10; see also Goodhart and Bhansali 1970). Moreover, these elections will give a strong impression of a "test election" and manifestations of discontent should hence be muted (Reif and Schmitt 1980, 13; Reif 1984; see also Reif 1997; Marsh 1998). In my interpretation of these propositions, which also takes into account Stimson's 1976 model, we should thus expect some recapture of the support for government parties, in the late term, relative to the *midterm*, while not reaching the same levels of support as in the *honeymoon*.

However, these propositions have received only a mixed support in empirical research. Studies have not found that government parties lose significant numbers of votes when European elections are held shortly after national elections, but neither that they gain votes at this stage (see e.g. Hix and Marsh 2007, 501; 2011, 6; Weber 2007; 2011). In terms of the government party vote at *midterm* and late in the NEC, Marsh (1998, 606) found that "while government losses are greatest around mid-term, thereafter they tend to level off rather than diminish as the cycle continues," while Hix and Marsh (2011, 6) noted the absence of a clear *midterm* effect and observed that "government losses were greater the further into the national election cycle the European Parliament elections were held."

Moreover, a lot has happened since the 2009 European elections, which is the last election analyzed in the previously mentioned works. Not only have we had two more European elections, but they are said to have become different and potentially more important too (see e.g.

Willermain 2014; Hobolt and de Vries 2016; Schmitt and Toygür 2016). Does this mean that the

voting patterns have changed? And, if so, how? And how does this relate to SOE theory? All

these questions call for a re-evaluation, which I will do by testing the following three SOE

hypotheses.

Election Timing Hypotheses

(1) Government parties experience gains in European elections held at the beginning of the national electoral cycle.

(2) Government parties suffer losses in European elections held around the middle of the national electoral cycle.

(3) Government parties do not suffer losses in European elections held close to the end of the national electoral cycle.

2) Adequacy of the SOE Model for Multi-party Systems

After addressing the key explanatory mechanism regarding the vote for government parties in

European elections, we should examine some other important, but possibly problematic, details

of SOE theory. The first issue is the adaptivity of the model, most notably to the multiparty

systems that predominate in Europe. Reif himself noted that "the [SOE] theoretical model may

be regarded as a generalization of the US mid-term electoral experience for multi-party (i.e.

possibility of coalition government) systems with parliamentary government (i.e. fusion of

legislative and executive powers with possible coalition change during the term of Parliament

and/or dissolution of Parliament, i.e. varying length of electoral cycles)" (1984, 247).

The fact that the SOE model is largely based on models for the US two-party system can be problematic in many ways, both theoretical and methodological (for the latter see Katz and King 1999). The first theoretical issue is that the impact of election timing is significantly easier to assess in the US context than in the European context, because US midterms are always held exactly halfway between presidential elections, while European elections are held at different points of the country-specific NECs.

Moreover, the US election timeline is fixed, while in European systems, it can be politically determined. In other words, in the US, the government cannot change the date of elections, which is determined by the US Constitution, while national governments (and sometimes even oppositions) can largely decide when an election is held in the vast majority of European countries. Consequentially, with the fixed election timeline, as in the US, we will only compare between two clearly given election contexts. On the other hand, with the non-fixed election timeline, as in most of Europe, we will need to take into account multiple election contexts.

The second theoretical issue is related to the clarity of responsibility and government accountability (see Powell and Whitten 1993). It is relatively clear who is responsible and hence will be held accountable in the US two-party system, given the focus on the presidency (see Norpoth 2001). In European countries, on the other hand, a one-party government is the exception rather than the rule, and power is mostly shared between different coalition parties in multiparty systems. The issue of government responsibility is thus more obscured (e.g. Lewis-Beck 1988; Powell and Whitten 1993, 401; Whitten and Palmer 1999; Anderson 2000; Powell 2000; Dorussen and Taylor 2001; Fisher and Hobolt 2010; Hobolt et al. 2013, 170; see also Downs 1957). Moreover, Europe does not have one single system, but rather a broad variety of

different systems, which has also evolved (differently) over time. The level of clarity of accountability and responsibility thus varies across countries and in time (see e.g. Whitten and Palmer 1999; Anderson 2000, Nadeau et al. 2002).

SOE theory does not truly take into account these issues and offers little guidance on how to apply the theory to coalition governments. Do all governing coalition members experience the same levels of decline in European elections? Reif (1984, 245-6) vaguely suggested that SOE effects are mainly pronounced in the vote for the party of the head of government (most often the Prime Minister). However, he has not explained why this should be the case or if this should apply across all countries or systems. For instance, what happens when the head of government is not from the largest party of the coalition? In general, SOE theory does not distinguish between one-party and coalition governments.

In later reactions to SOE theory, government coalition issues have been almost completely ignored too, maybe because the theory itself does not address them. Curtice (1989, 225) noted that "within each of the government coalitions some parties fared better than others." However, he did not make any propositions on which parties should suffer more and why. Indeed, from his findings, it was not clear if senior parties (i.e. parties that deliver the prime minister) are punished more than junior parties. Ferrara and Weishaupt (2004, 288) have made a further elaboration, suggesting that large government parties experience greater losses than their smaller coalition partners, because of expressive voting, their greater impact on policymaking, and thus their larger responsibility and accountability.

This lack of attention given to the clarity of responsibility is rather surprising. Research has showed that diminished clarity of responsibility in multiparty systems leads to a decreased ability of voters to fully use their vote to sanction politicians (Powell and Whitten 1993; Whitten

and Palmer 1999; Anderson 2000; Powell 2000; Nadeau et al. 2002; Duch and Stevenson 2005).

This logically presents an important issue for the SOE model, which assumes that some voters, at certain points of the NEC, use (European) SOE to evaluate the performance of their national governments.

Hobolt et al. (2013, 165) found that, when it comes to assigning responsibility and holding national governments to account, voters are mainly concerned with "identifying a single political party that they can reward or punish." The question is then whether they can identify such a party. In this regard, Anderson (2000) has suggested that voters should know which party is the most influential within the government, and consequentially, reward or punish this party accordingly (see also Duch and Stevenson 2008; Fisher and Hobolt 2010).

There are two possible ways to conceptualize the most influential party in the coalition government. The first is to assume that it will always be the "*prime minister party*" (i.e. the party of the head of government). It has been argued that not only this party will be powerful, if not the most powerful, within the coalition, but it will also draw more attention from the media. Moreover, government performance is closely associated with the performance of the head of government (Fisher and Hobolt 2010, 361). The second approach is related to *party size*. According to this approach, voters punish or reward coalition parties according to their cabinet strength, given that greater representation should lead to more influence over policy, and therefore, to more responsibility for decisions (Anderson 2000).

Consequentially, while SOE theory cannot adequately deal with the issues that multiparty systems bring, other theories on electoral behavior in multiparty systems enable us to come up with several testable hypotheses that can improve the original theory. This is demonstrated here through the issue of coalition governments. Taking into account the limited number of

propositions of the SOE literature itself, as well as applying the insights from other literature examined above, we can generate the following hypotheses on voting for coalition parties in European elections:

Clarity of Responsibility Hypotheses

> (4) The prime-minister-party suffers greater losses in European elections than other government parties.

> (4a) Larger coalition parties suffer greater losses in European elections than their smaller coalition partners.

Finally, there is another potential issue related to the application of the model to multiparty systems. This issue is related to party size. In the US two-party system, the overwhelming majority of votes is cast for the two main parties, the Democratic Party and the Republican Party. Punishment of the government thus translates almost by definition mainly into a vote for *the* opposition. However, this is not necessarily the case in many European multiparty systems.

As SOE theory is built upon theories on US midterm elections, it tries to predict and explain the vote for governing parties in European elections by natural popularity cycles of national governments. Therefore, it argues that the sole fact that a party is a member of government should determine its fate in European elections. But Reif and Schmitt (1980, 9) also suggested that

> "the large, electorally decisive parties may receive votes in first-order elections from voters whose actual preference lies with some small or new party. While the small party may well represent the voter's opinion more precisely, he may opt for the opportunity–

when more is at stake–of supporting a large, established party, and thereby the general direction of his political views".

In other words, some people will vote *with the heart* for small parties in SOE, yet *with the head* for big parties in FOE. Hence, irrespective whether these big parties perform well or not in government, *some* of their voters will prefer a small party in SOE anyway.

When putting these two propositions together, the former may be problematic for the latter and vice versa. Specifically, it could lead to what has been called the problem of observational equivalence (see Marsh 1998; Hix and Marsh 2007). Governing parties can lose votes in (European) SOE because they are in government, i.e., as a consequence of the natural government popularity cycles, but also because they are usually large and established parties and should thus be simply disadvantaged by the lack of strategic voting in SOE. Some authors indeed noted this potential issue soon after the theory was first published. Curtice (1989, 225) suggested that if voters are more inclined to vote for new and small parties in European elections then

> "the loss of government support may be no more than the government parties' share of a general loss of support by all big and/or established parties [and] if we are to claim that at European elections electorates express a dissatisfaction which is specifically directed at their national government rather than display a generalized protest against all established political parties, then we should expect the major opposition party(ies) to be more successful at retaining their support than government parties".

However, he found this not to be the case, as large and established opposition parties rarely performed better than governing parties, at least in the 1989 European election (Ibid.; see also Oppenhuis et al. 1996; Ferrara and Weishaupt 2004; Weber 2011; Hix and Marsh 2011). Consequentially, it is necessary to evaluate whether there is any significant relationship between

a party's government status and its size in determining its performance in European elections. I thus also test for the following hypothesis:

Party Size vs Government Status Hypothesis

(5) Large parties suffer greater losses in European elections, notwithstanding their government status.

3) Other Propositions: Turnout

One additional proposition is worthy of analysis. We have seen that SOE theory also predicted a connection between losses of governing parties and lower turnout in European elections. Reif and Schmitt (1980, 10) suggested that differential and generally lower turnout in European elections diminishes the vote for ruling parties, as a consequence of the expected higher mobilization of opposition and expressions of dissatisfaction with the government. Additionally, Martins and Veiga (2014, 275) argued, "if voters reward/punish governments, there is no reason to think that nonvoters, or at least some of them, do not use similar mechanisms to decide whether to vote or not. Consequentially, their decisions will affect the electoral results of the incumbent party." In other words, we can define this as 'protest through abstention' in European elections (see also Weber 2011, 907-8).

In general, while determinants of turnout have been extensively studied, the effects of turnout on the government vote in SOE have not received much attention from scholars (cf. Martins and Veiga 2014). In one of the few notable exceptions, van der Eijk and van Egmond (2007) found that the effects of turnout on party performance were comparatively small and non-

negligible in only a few cases. In terms of government parties, no significant effect of turnout has been found. However, given the limited number and scope of these studies, it is useful to evaluate whether there is a systematic effect of the difference in turnout on the government vote. Therefore, I additionally test for the following:

Turnout Effects Hypothesis

(6) The lower the turnout in European elections, compared to the previous national elections, the lower the vote for government parties in European elections.

4) Alternative Explanations

Since 1980, there have been few studies on electoral behavior in European elections that provided different explanatory mechanisms than SOE theory and are worthwhile to mention given that they can potentially improve the theory. First, as we have seen, SOE theory suggests that European elections function mainly as referendums on government popularity. However, Oppenhuis et al. (1996) disputed the theory on these grounds, suggesting that bad government performance is not caused by a combination of political business cycles and the inevitability of unrealized expectations, as argued by Reif and Schmitt (1980), but rather by the specific context of a given election. In terms of SOE theory, this means the position of European elections in the NEC (see also Marsh 2007, 54).

This proposition is in line with the general message of the influential edited volume *Choosing Europe* (van der Eijk and Franklin 1996), of which the Oppenhuis's et al's study is a part, and other works, mainly from the contributors of that volume (e.g. Oppenhuis et al. 1994;

van der Eijk et al. 1996; Marsh 1998; van der Brug and van der Eijk 2007; see also e.g. Hobolt 2015, 14). This research produces a theory on European elections that is to a large extent distinct from SOE theory. Given that the crucial assumption of the theory is that European elections can serve as markers for the domestic strength of parties, and thus influence the national political arena (Oppenhuis et al. 1996, 287-9; see also Somer-Topcu and Zar 2014), I refer to it as the 'Markers of Public Opinion' (MPO) theory.

MPO theory includes many propositions that could be important for several unanswered questions of SOE theory, but have only rarely been addressed, particularly since the publication of the volume in 1996. In terms of the government vote in European elections, MPO theory posits that the key mechanism is "the extent to which European elections could be regarded by politicians and commentators as 'markers' yielding useful information about the relative standing of political parties" (van der Eijk et al. 1996, 156; see also Oppenhuis et al. 1994, 1996). Specifically, van der Eijk and contributors (1996, 152) argue that there is a possibility that "some voters might use a European election to try to affect the behaviour of political parties and their leaders, particularly when a national election is near." The crucial difference is thus one of prime focus: MPO theory focuses primarily upon national politics, looking at SOE as ways to influence the national (first-order) political arena, while SOE theory focuses primarily on European politics, looking at FOE as an important independent variable to explain the outcome of European second-order elections. This also leads to different expectations in terms of the effects of election timing on the role of government performance in European elections.

According to MPO theory, voters vote rationally (*with the head*) in FOE, as they take into account the power of parties to put their programs into effect. Applied to SOE, it assumes two different contexts, according to the position of European elections in the NEC. First, when

European elections are held shortly after national elections, they are "throw-away" elections. They cannot be "markers" at this point, as national elections, held only a couple of months or days before, are still valid as stronger markers. Thus, people who still decide to participate, are liberated from strategic calculations and expected to vote sincerely (*with the heart*), hence supporting their preferred party, even if that party has little chance of playing a prominent role in government (van der Eijk et al. 1996, 156-7; Oppenhuis et al. 1996, 301-2).

Second, when European elections are held late in the NEC, they are "marker-setting" elections. At this point, national elections no longer constitute a marker and European elections can thus fill the void and become more important. In this scenario, voters, still liberated from first-order strategic considerations, as there are no consequences for the allocation of national power, are expected to be more prone to cast a strategic protest vote as an expression of their frustration and anger, so-called vote *with the boot* (Oppenhuis et al. 1996, 302, 304).

MPO theory thus defines two rather than three different terms in the NEC. If we assume that government parties benefit most from rational voting (i.e. *with the head*), which occurs primarily in FOE, they should lose in both of these two stages. In other words, there should be no over-confirmation, or improvement, of the government vote at *honeymoon* and no regaining at *late term*. Government popularity in European elections could thus be imagined almost as a flat line throughout the NEC, compared to the u-shaped curve predicted by the SOE theory. Table 1.1 summarizes the propositions described above and sets them up against those of SOE theory. We can thus additionally evaluate two following hypotheses:

Markers of Public Opinion Theory Hypotheses

(1a) Government parties suffer losses in European elections held at the beginning of
the national electoral cycle.

(3a) Government parties suffer losses in European elections held close to the end of the
national electoral cycle.

At the aggregate level, the (dis)confirmation of these hypotheses can tell us something about the underlying mechanisms and voters' motivations. As we have seen, SOE theory is based on micro-level hypotheses, but is more commonly tested with aggregate-level data (see Schmitt et al. 2020), MPO theory being the most notable exception. Consequentially, putting these two theories against one another could tell us which proposed underlying mechanisms, and thus motivations of voters, are more plausible. Obviously, this cannot fully replace an individual-level analysis, but it does allow us to evaluate propositions stemming from an individual-level analysis with a larger set of data.

Table 1.1: The Government Party Vote in European Elections

Theory	*Second Order Elections*	*Markers of Public Opinion*
Honeymoon	gains	losses
Midterm	losses	*unclear*
Late Term	stability	losses

Second, it has been suggested that loyalty to the government in European elections is not driven by election timing alone, as SOE theory argued, but by the combination of the effects of election timing and economic performance. Kousser (2004), following economic voting theories of retrospective voting (e.g. Downs 1957; Fiorina 1981; Kiewiet 1983), has argued that macroeconomic performance, specifically unemployment, rather than time itself, is the key

explanatory factor. It is thus not the natural popularity cycle but rather the retrospective

judgments of the government economic record that impact the magnitude of governing parties'

losses in these elections. Retrospective voting should occur only when European elections take

place late enough in the NEC for the governing parties to establish a clear record. That is,

according to Kousser, in the mid and late-terms of the NEC (2004, 9). If the economy suffers at

this point, government parties suffer in these elections as well (while opposition parties gain).

Finding a link between economic performance and election timing in terms of determining the

government vote in European elections would mean not only that the SOE theory mechanism

should be modified but also that general theories on (retrospective) economic voting are largely

correct. Consequentially, I also test the following hypothesis:

Retrospective Voting and Election Timing Hypothesis

(7) Government parties lose votes in European elections, compared to national elections, when the election is held in the mid or late-term of the national elections cycle *and* the economic performance has worsened since the last national election.

Finally, before getting into the empirical analysis, it is worthwhile to mention that we should also

put some theoretical emphasis on the meaning of "wins" and "losses" in (European) SOE.

Specifically, on the difference between absolute and relative increases/decreases in vote shares.

Indeed, Reif and Schmitt (1980, 4-8) differentiate between the two, however without necessarily

providing any theoretical reason for doing that. Similar, most of the subsequent analyses control

for a relative increase by including the party size variable, measured as the party vote share in the

preceding national election. This way, they indeed take into account the baseline national support

for a party. However, they mostly fail to discuss theoretical reasons for doing that and the importance of acknowledging the difference between absolute and relative gains.

It is straightforward that for a large party, which consistently obtains 20-30% of votes in national elections, an increase/decrease of 1-2 percentage points in European elections is largely meaningless in terms of percentage increase/decrease. It is thus questionable whether this should really be considered a "loss" for that party. On the other hand, for a party that typically struggles to get to 1-2% of vote shares in national elections, a similar increase/decrease, while still seemingly small in terms of percentage points, will be quite significant in terms of relative gains. Correspondingly, it will have a completely different meaning for that party. i.e., a "big win".

Arguably, this will affect strategies that parties adopt when competing in (European) SOE. For instance, if a large government party constantly loses some percentage points, but the relative decrease is marginal, it will likely not change its approach to these elections. Contrarily, a small party will care much more, as in its case winning even a minimal amount of percentage points in (European) SOE translates into a significant increase relative to national elections. This should be reinforced by the fact that European elections can provide small parties not only with more visibility in the national political arena but potentially also with access to public funding that can be crucial for its survival (see e.g. Casal Bértoa and Spirova 2019).

Empirical Analysis

1) Data and Variables

I use an original dataset that includes vote shares of political parties in all popular European elections (i.e. since 1979) and their closest preceding national elections. All parties that participated in both national and subsequent European elections are included. Consequentially, parties that, for whatever reason, did not participate in one of the corresponding elections are excluded – this includes parties for which a European election was their first nationwide election.[2] Importantly, I also exclude the parties in countries where European and national election are held on the same day.[3] Finally, I do not include cases in which the government was not yet created when the European election was held.[4]

The dependent variable for all hypotheses is the difference in vote share between European elections and national elections (*ΔVote Share*), constructed by subtracting vote shares of a government party in the previous national election from vote shares of that party in the following European election. The vote shares of parties are collected manually, using available electoral databases (e.g. Döring and Manow 2020) and official country-level sources, such as the websites of the ministries of interior. The key independent variable is the party's government status (*Govopp*). The variable is coded as 1 if a party is a member of the government and as 0 for parties that are in opposition.

The explanatory variable for the first three *Second Order Elections Theory* hypotheses, as well as for the additional *Markers of Public Opinion Theory* hypotheses, is then the temporal distance of the European election from the previous national election. There are two different ways to measure election timing: either by the number of days that have passed since the

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[2] Given that for these parties it is impossible to count my dependent variable.
[3] Given that many mechanisms presented here are based on voters' evaluation of a government party's record in the office, it does not make much sense to include the cases in which voters had no time to make such evaluations. The inclusion of such cases might also produce misleading results, especially when testing for the effects of election timing. Moreover, as the occurrence of same-day elections is fairly rare, and no country always held simultaneous European and national elections, I do not lose many observations or explanatory power by dropping these cases.
[4] There are only four of these cases: Finland (2019), the Netherlands (1994); Ireland (1989), Italy (1979).

previous national election (*TimeSinceNat*) or by dividing the number of days between the last

national election and the European election by the number of days between a national election

previous to a European election and a national election after a European election (*Cycle*) (see

Reif 1984; Marsh 1998; Hix and Marsh 2007; 2011; Weber 2011). The latter can be expressed

with the following equation:

$$C = \frac{T_E - T_{N-1}}{T_{N+1} - T_{N-1}}$$

where *cycle C* is equal to the date of a previous national election T_{N-1}, subtracted from the date of

a European election T_E, divided by the date of a previous election national election T_{N-1},

subtracted from the date of a subsequent national election T_{N+1} (cf. Reif 1984, Weber 2011).

The number of days passed since the previous national election (*TimeSinceNat*) has some

practical advantages over the *Cycle* variable. Specifically, it allows me to include several cases

from the 2019 European elections, which would have to be omitted with the use of the latter.[5]

However, the election timing variable constructed this way does not capture the potential effect

of the distance of the European election from the next national election, which the *cyclical*

variable does capture. We have seen that some of the predictions of SOE (and MPO) theory

indeed, implicitly or explicitly, refer to not only the distance from the previous national election

but also to the closeness of the next one. Consequentially, I use both measures and report

whether there is any difference in results when we switch them.

Given that SOE theory predicts a non-linear relationship for the effects of election timing

on the government vote, I use quadratic terms (*TimeSinceNat²* and *Cycle²*) of both of these

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[5] Given that in many countries the next national election has not yet happened and the variable cannot thus be
counted with full accuracy.

variables (see also Marsh 1998). Even if the relationship would be linear, the quadratic model will show us that. Therefore, the use of the quadratic versions of the election timing variables is the most plausible option to test my hypotheses. The use of quadratic terms indeed convincingly improves the fit of models in which I analyze the effects of election timing.[6]

In line with other studies (e.g. Hix and Marsh 2011), party size (*Party Size*) is measured as the vote share of a party in the preceding national elections, This is arguably a more precise measure than choosing an arbitrary cut-off point between small and large parties (cf. Marsh 1998). Moreover, constructed this way, the variable allows me to control for the "baseline" vote share, and thus for relative change in vote shares. Turnout (*ΔTurnout*) is constructed by subtracting turnout in the previous national election from turnout in the European election, both measured by the percentage of registered voters who have voted. Additionally, to evaluate the effects of a party's position in the government coalition (hypothesis 4), I construct three dummy variables: *PM Party* (coded as 1 if a party heads the government), *Opposition* (which is simply an inversion of the *Govopp* variable), and *Junior* (coded as 1 if a party is in a government coalition but does not provide the prime minister).

Finally, I also include two variables to control for the economic performance of a country, i.e. the changes in unemployment rates and GDP Growth (*ΔUnempl* and *ΔGDPGrowth*). These variables are also necessary to evaluate the effect of retrospective voting (hypothesis 7). The data for these variables are obtained from the World Development Indicators database (World Bank 2020). In both cases, the variables are constructed as a difference between the two elections, subtracting scores at the time of the previous national election from those at the time of the European election.

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[6] This is proven by a variety of tests (e.g. F-test) comparing models with first-order and second-order terms.

In the first part of the analysis, I use descriptive statistics to compare the mean change in vote shares of government parties for all sets of European elections from 1979 to 2019. This serves mainly to confirm whether these parties generally suffer losses in European elections. In addition, I also compare vote shares of senior coalition parties (PM parties), junior coalition parties, and, to preliminarily evaluate the party size effects. Except for the average vote shares, and the difference between them, I also include two-tailed p-values to demonstrate the statistical significance of these differences, as well as percentage changes, to cover relative gains/losses. Finally, I also evaluate whether there are any notable differences in the government party performance between every single set of elections, to at least partially address the effects of changes in the composition of the EU or some recent transformations in European politics.

In the second part, I run several ordinary least squares (OLS) models with clustered standard errors by country. The first basic model serves to evaluate the independent effect of being in government on the difference in vote share between European and national elections. The analyses in the following models then use interactions between different variables to test for the remaining hypotheses. First, I evaluate *Second Order Elections Theory* hypotheses (1 to 3) and additional *Markers of Public Opinion Theory* hypotheses (1a and 3a). Given that this is the crucial test for the effects of election timing on the performance of government parties, it is desirable to include both measures of election timing here. I thus construct two models (2 and 2a). In model 2, I interact government status (*Govopp*) with the second-order term *TimeSinceNat^2*, while in model 2a, I interact it with *Cycle^2*.

Second, model 3 tests for the impact of government status and party size, hence mainly

evaluating hypotheses 4a and 5. Third, model 4 evaluates the effects of change in turnout on the

performance of government parties (hypothesis 6), by interacting the difference in turnout with

the *Govopp* variable. Fourth, models 5 and 5a test for the effects of economic performance and

election timing on the performance of governing parties (hypothesis 7), using three-way

interactions. These models mainly differ from one another in terms of the specific measurement

of the effects of economic performance. Given that we have used two variables to capture these

effects, I interact election timing and party government status with (1) change in GDP Growth

(model 5) and (2) change in unemployment rates (model 5a). While I use the *TimeSinceNat[2]*

variable to measure election timing in these two models, I also run additional analyses with the

Cycle[2] variable to control whether this change in measurement affects the results in any

meaningful way.

Finally, I construct an additional model (6) to further test hypothesis 4, which uses a

trichotomous variable that includes senior coalition parties, junior coalition parties, and

opposition parties to specifically evaluate the effects of being a senior party in a government

coalition on the difference in vote share between European and national elections. Each variable

is entered into the model as a dichotomous dummy (*PM Party*, *Junior*, and *Opposition*,

respectively), with junior coalition members treated as the reference category and thus omitted.

Findings

Table 1.2 summarizes the results of the first part of the analysis. The first observation is that, as

expected, there is a significant difference in the vote share of government parties between

European and national elections. On average, from 1979 to 2019, government parties lost 3.9

percentage points in European elections compared to previous national elections. This equals to a

relative decrease of 18.1%. Moreover, the difference is statistically significant at conventional

levels (p<0.05, two-sided). The key initial proposition of SOE theory is thus seemingly

confirmed.

However, when unpacking this general proposition, only a first glance at the data

uncovers recognizable differences between different categories of governing parties. First, senior

coalition members (*PM parties*) are particularly punished in European elections, losing 6.2

points compared to previous national elections. In relative terms, their vote shares decreased, on

average, by 21%. In comparison, junior coalition members experienced much smaller losses, on

average losing just 1.4 percentage points in European elections, a relative decrease of 12%.[7]

These findings thus provide the first support for hypothesis 4. However, whether a party is a

senior or junior coalition member independently affects the difference in vote shares between

national and European elections has still to be evaluated in a multivariate analysis that follows.

Second, parties in government that obtain less than 10% of the vote in the previous

national election, on average, performed better in European than in national elections. Although

the increase is fairly marginal (0.6 percentage points in absolute, but 9.4% in relative terms), and

not statistically significant, this still might suggest that (very) small government coalition parties

are not punished in European elections, and could thus provide some support for hypothesis 4a.

The results for (very) large government parties (over 30% of vote shares in previous national

elections) further supports this hypothesis. These parties are indeed the biggest *losers* of

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[7] I also run a secondary test of means analysis, not reported in table 1.2, in which I compare the difference in *PM parties*' vote shares with the difference of junior collation members' vote shares. This difference is statistically significant (p-value=<.001, two-tailed), suggesting that there is indeed a significant difference in the performance of senior and junior coalition parties in European elections.

European elections, experiencing an absolute decrease of 8.6 percentage points, and a relative decrease of 23.4%, relative to previous national elections.

Table 1.2: Vote Shares of Governments (1979-2019)

	Gov.	PM	Junior	Size<10	Size<20	Size>30
Mean Vote NE	21.5	29.5	11.7	6.4	10.4	36.7
Mean Vote EE	17.6	23.3	10.3	7	9.6	28.1
Difference (EE – NE)	-3.9	-6.2	-1.4	.6	-.8	-8.6
p-value (two-tailed)	.001	.001	.001	.2	.03	.001
% Change	-18.1%	-21%	-12%	9.4%	-7.7%	-23.4%
No. of Observations	332	107	173	90	181	57
EE = European Elections, NE = National Elections						

However, these first analyses also demonstrate that our conclusion about the effects of the size of government parties on their performance in European elections, in a simple analysis like this, strongly depends on the cut-off point selected to indicate whether a party is large or small. This is evident from the evaluation of the performance of parties that, on average, obtain less than 20% in national elections. Their vote shares in European elections decrease by only .8 points, which means 7.7% decrease. Given these somewhat ambiguous results, a multivariate analysis capturing the effects of party size on the government vote is desirable.
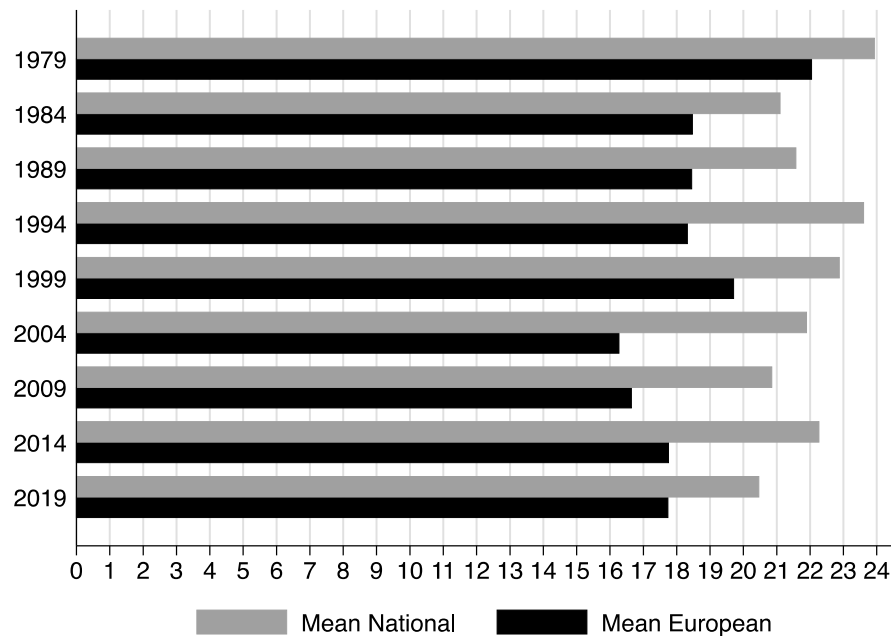
Figure 1.1: Difference in Vote Shares of Governments over Time

To recognize any notable difference in government party performance across time, Figure 1.1 plots the average difference in vote shares between European and national elections in each set of the European elections separately. First, we can see that the differences were the lowest in the first two European elections and in the last European election. Second, the opposition vote in European elections was the most evident in the 2004 election, followed by the 1994 election. Generally, we hardly observe any clear trend over time, which could suggest a strong impact of the most recent transformations of EU and European politics in general. Government parties underperformed in each European election, but the difference in vote shares between European and national elections fluctuates irregularly over time.

The results of the regression analyses demonstrate that whether a party is in government or opposition strongly determines its performance in European elections (see Table 1.3). The coefficient of the *Govopp* variable is statistically significant in all models in which it is not

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interacted with other variables. Its effect on the difference in vote share is, as expected, negative. Consequentially, government parties lose significantly more than opposition parties in European elections. The interaction models (2, 2a, 3), however, also demonstrate that this effect is likely conditional upon both election timing and party size. To fully understand these interactions, it is necessary to plot these models.

Table 1.3: The Determinants of Difference in Vote Share of Government Parties

	M1	M2	M2a	M3	M4	M5	M5a	M6
Govopp	-2.214***	3.427	4.209**	.496	-1.400	3.997***	3.096**	
	(.537)	(1.230)	(1.634)	(.587)	(1.077)	(1.335)	(1.271)	
TimeSinceNat	-.001	.003**		-.001	-.001	.004**	.002**	-.001
	(.001)	(.001)		(.001)	(.001)	(.001)	(.001)	(.001)
TimeSinceNat²	.000	-.000**		.000	.000	-.000**	-.000**	.000
	(.000)	(.000)		(.000)	(.000)	(.000)	(.000)	(.000)
Cycle			7.271***					
			(2.110)					
Cycle²			-7.061***					
			(2.014)					
Party Size	-.178***	-.175***	-.172***	-.105***	-.177***	-.174***	-.175***	-.148***
	(.026)	(.026)	(.026)	(.025)	(.026)	(.025)	(.026)	(.026)
ΔTurnout	.004	.003	.007	.003	-.006	.005	.006	.003
	(.007)	(.007)	(.007)	(.007)	(.012)	(.006)	(.006)	(.007)
PM Party								-2.509**
								(1.057)
Opposition								1.457**
								(.555)
Govopp#Timesincenat		-.013***						
		(.004)						
Govopp#Timesincenat²		.000**						
		(.000)						
Govopp#Cycle			-25.83***					
			(8.347)					
Govopp#Cycle²			19.71**					
			(7.928)					
Govopp#Party Size				-.171***				

|	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				(.029)				
Govopp# ΔTurnout					.035			
					(.036)			
ΔGDPGrowth#TimeSinceNat#GovOpp						-.001*		
						(.001)		
ΔGDPGrowth#TimeSinceNat^2#GovOpp						.000*		
						(.000)		
ΔUnempl #TimeSinceNat#GovOpp							-.001	
							(.002)	
ΔUnempl #TimeSinceNat^2#GovOpp							.000	
							(.000)	
ΔUnempl	-.039	-.038	-.079*	-.032	-.042	-.056	-.189	-.037
	(.050)	(.050)	(.040)	(.047)	(.050)	(.052)	(.191)	(.048)
ΔGDPGrowth	-.014	-.018	-.025	.000	-.014	-.187**	-.029	-.007
	(.026)	(.024)	(.023)	(.025)	(.026)	(.070)	(.024)	(.025)
Constant	2.753***	1.150***	.855	2.129***	2.534***	0.966*	1.292**	1.054
	(.490)	(.499)	(.556)	(.427)	(.568)	(.497)	(.482)	(.629)
Observations	1,192	1,192	1,043	1,192	1,192	1,192	1,192	1,192
R-squared	.245	.267	.281	.270	.246	.270	.277	.254
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Figure 1.2 plots the analysis of model 2, which evaluates the impact of government status and election timing (measured by the quadratic function of days passed since the national election), shows two things. First, government parties actually perform, on average, significantly better than opposition parties in European elections when held shortly after national ones.[8] Second, government parties experience losses, compared to opposition parties, in European elections held between ~ 440 and ~ 1500 days from national elections. Moreover, their comparative disadvantage worsens gradually until 1200 days have passed since the national election. Then it starts to level off and even getting slightly lower[9, 10].

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[8] Between 20 and ~ 100 days.

[9] There is no statistically significant effect of party's government status in European elections held between ~ 100 and ~ 440 days, as well as between ~ 1500 and ~ 1580 days, since national elections.

[10] If we plot the analysis in model 2a, the results are nearly identical (see Appendix 1.1).

These results provide mixed support for the first three SOE hypotheses. There is an increase in government party vote, relative to that of opposition parties, in European elections held shortly after national elections, which supports hypothesis 1. However, the largest decrease in governing party support happens when European elections are held slightly after the midterm of the NEC, thus providing somewhat limited support for hypothesis 2. Finally, while there is some leveling off, and if there is any regaining (compared to the previous decrease), it is hardly non-negligible and government parties are still outperformed by opposition parties in the late stage of the NEC. Consequentially, we do not find much evidence for hypothesis 3.
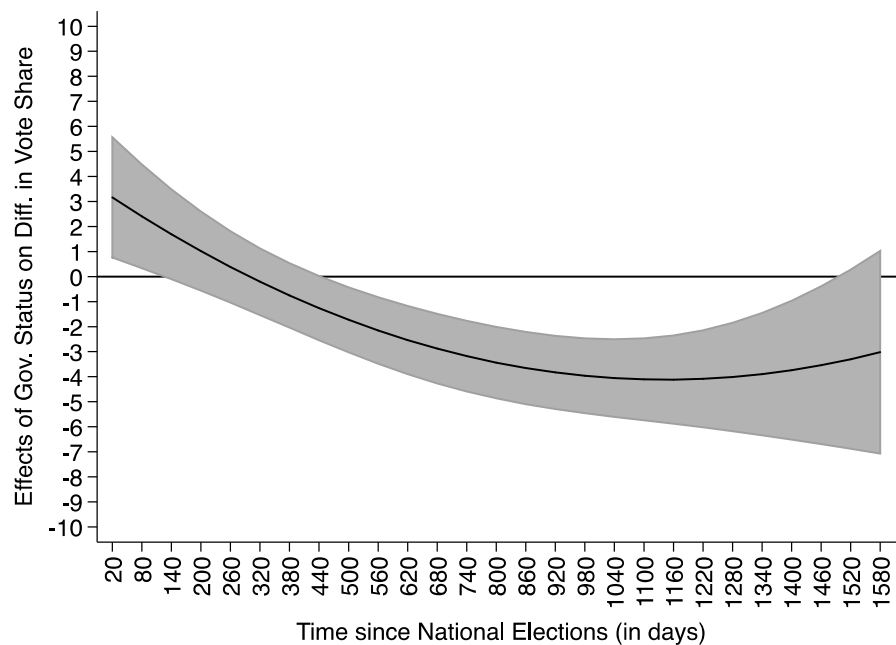
Figure 1.2: Average Marginal Effects of Government Status (95% Cls) – Model 2

The results of this analysis thus also reject the additional (MPO) hypothesis 1a, which predicted government losses in European elections shortly after national elections. At the same time, they are rather supportive of hypothesis 3a, given that government parties indeed perform

significantly worse than opposition parties in European elections held late in the NEC, although the difference in performance is not significant at the very end of the NEC.
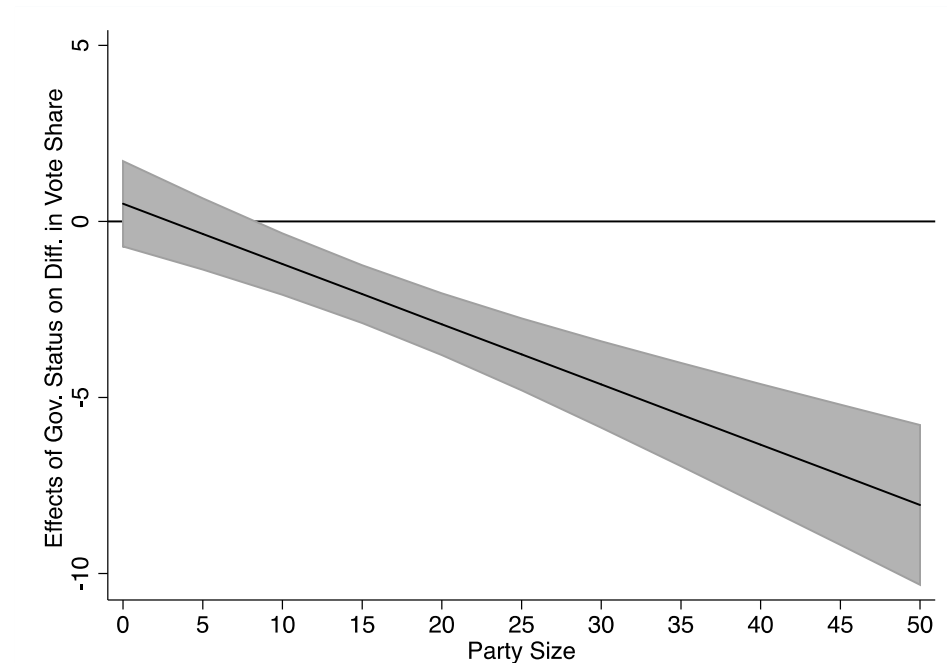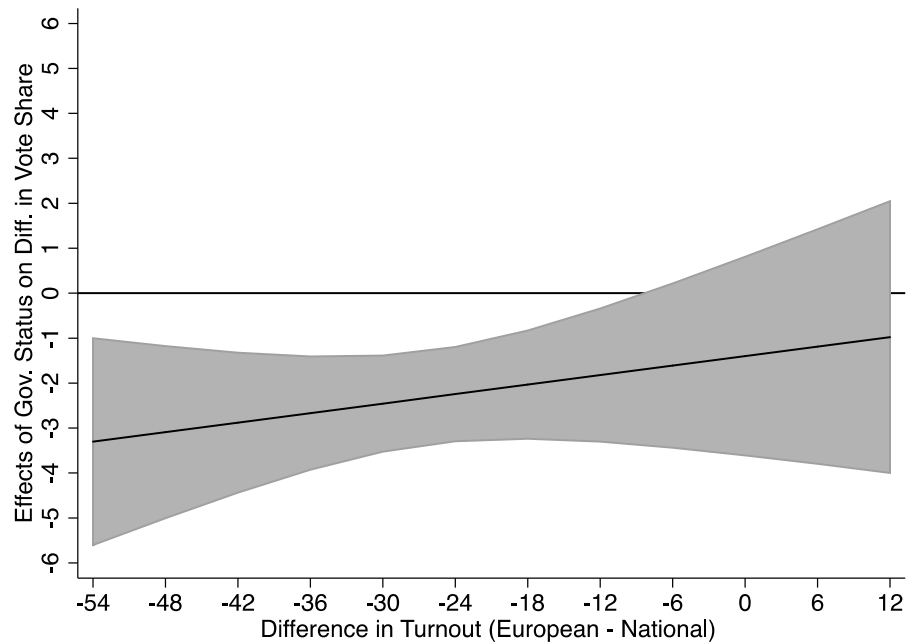

Figure 1.3: Average Marginal Effects of Government Status (95% Cls) – Model 3

Figure 1.3 plots the analysis of the interactive effects of government status and party size (model 3) and demonstrates that only very small government parties (i.e. less than 10%) do not have a significant comparative disadvantage in European elections. But the larger the government party, the worse its performance in European elections, compared to opposition parties. Consequentially, given that large government parties are significantly more disadvantaged in European elections than large opposition parties, it seems plausible to reject hypothesis 5. The finding that (very) small government parties are not punished in European elections also provides indirect support for hypothesis 4a, which suggests that small government coalition parties experience smaller losses than large coalition parties.

In terms of the *Turnout Effects Hypothesis* (6), Figure 1.4 reveals that the difference in turnout between national and European elections significantly affects government party performance in European elections as well. Compared to opposition parties, government parties tend to lose significantly more when the difference in turnout is more pronounced. Given that the *ΔTurnout* variable is constructed by subtracting turnout in the previous national election from turnout in the European election, this means that the fewer people voted in the European election compared to the national one, the larger the comparative disadvantage for government parties in the European election. Indeed, we can see that when the difference in turnout is positive, government party disadvantage in European elections is not statistically significant. Consequentially, we find support for hypothesis 6.

Figure 1.4: Average Marginal Effects of Government Status (95% Cls) – Model 4

In models 5 and 5a, I tested the impact of the interaction between changes in economic performance, election timing, and government status. Figure 1.5 plots the interactive effect of *ΔGDPGrowth*, *TimeSinceNat²*, and being in government (*Govopp*=1) and demonstrates that change in GDP growth has no significant effect on the performance of government parties, regardless of election timing. Use of the *Cycle²* variable (not reported in Table 1.3) renders no significant change in results (see Appendix 1.2). This analysis thus provides no support for hypothesis 7.
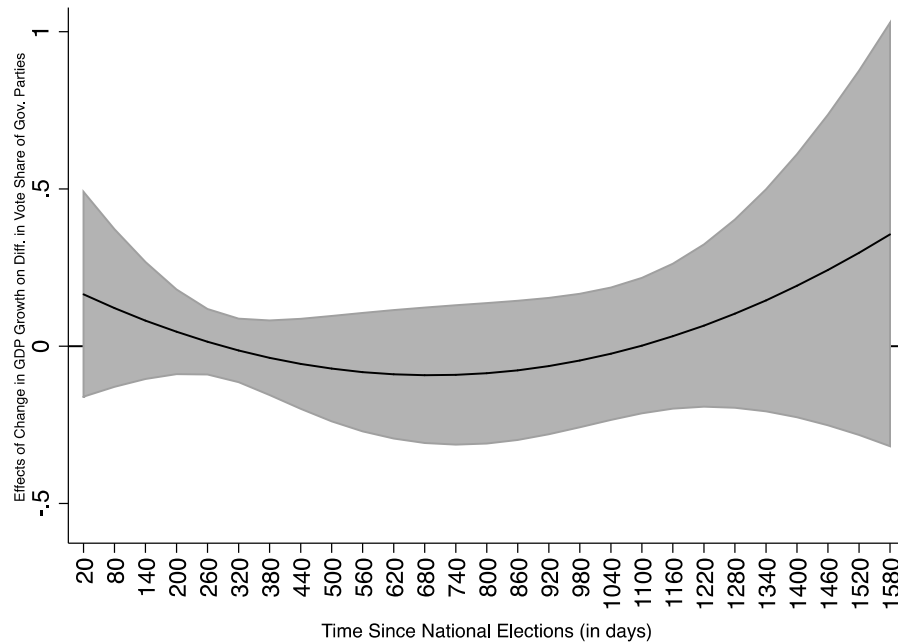
Figure 1.5: Average Marginal Effects of *ΔGDPGrowth* and Being in Government (95% Cls) – Model 5

However, when we use the change in unemployment rates as an indicator of economic performance (model 5a), the results do change. Figure 1.5a shows a significant effect of change in unemployment on vote share of government parties when European election is held between ~ 650 and ~1150 days after the national election. This effect is negative and grows with every unit

increase in temporal distance. In short, in this particular period of the NEC, an increase in

unemployment leads to a decrease in the vote for government parties in European elections,

relative to previous national elections. There is no significant effect in European elections held

shortly after national elections or shortly before the next national elections.[11] Consequentially,

we find partial support for hypothesis 7 here: there is an effect of unemployment on government

vote around *midterm*, but there is no effect in the *late term* of the NEC.
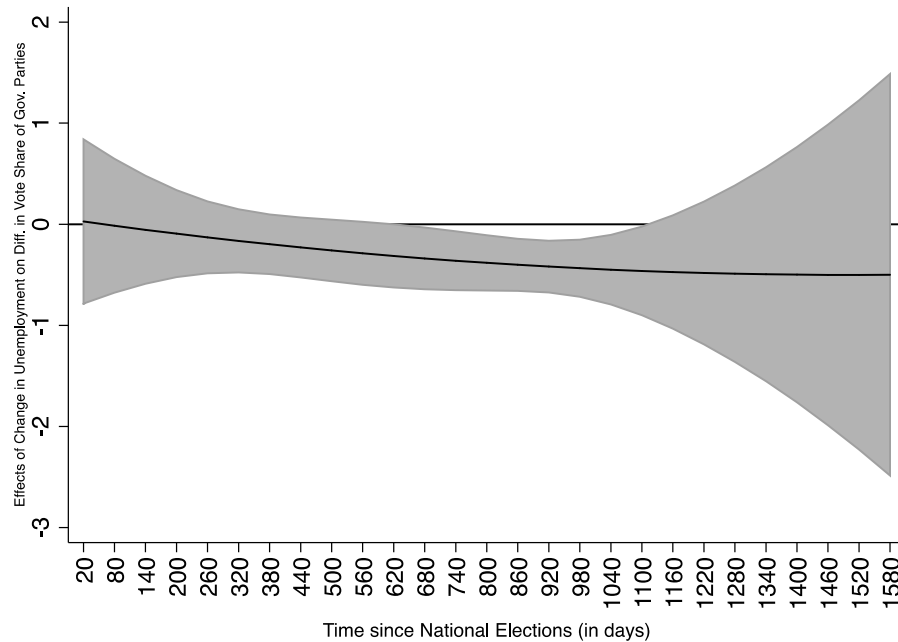
Figure 1.5a: Average Marginal Effects of *ΔUnempl* and Being in Government (95% Cls) – Model 5a

Finally, the analysis in model 6 evaluated the effects of the party position within the

government coalition. The results demonstrate that whether a party is a senior or junior partner

does have an independent effect on its performance in European elections. Compared to junior

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[11] These results are confirmed even when using our second measure of election timing, the *Cycle²* variable (see Appendix 1.3).

coalition parties, senior coalition parties perform significantly worse in European elections, hence supporting hypothesis 4. Moreover, this analysis also further confirms the findings of other analyses presented here. Opposition parties have, in general, a significant comparative advantage over government parties in European elections, and they outperform senior government parties, in particular.

Discussion

The first empirical analyses in this chapter, using descriptive statistics, confirmed the expectations that motivated this research. Governments generally lose votes in European elections, as SOE theory predicted and empirical studies have confirmed time and time again. However, there are some important differences between governing parties that were not yet analyzed before. First, senior coalition parties, or PM parties, i.e., parties of the head of government, lose on average more votes in European elections than their junior coalition partners. Second, (very) small government coalition parties, i.e., those that gained less than 10% in the previous national election, performed on average slightly better in European elections, relative to previous national elections. Finally, (very) large government coalition parties, i.e., above 30%, lose most significantly. The *clarity of responsibility* thesis hence seems to apply fairly well to European elections.

It is worthwhile to note that these results were somewhat surprising in terms of relative losses/gains. Theoretically, we would expect that losing or gaining some percentage points will be less significant for large government coalition parties and more significant for smaller ones. However, the analyses demonstrated that the relative change in votes is quite high for both large

and senior coalition parties (20% decrease), yet quite low for junior coalition parties (12% decrease) and quite low and positive for small government parties (9% increase). The main implication is that senior and large government coalition parties are, on average, punished quite severely in European elections.

We have also seen some notable differences between single sets of elections. Mainly, the predicted SOE effects were rather minimal in the first two European elections, but also, rather surprisingly, in the 2019 European elections. It is thus evident that some contextual differences exist between elections, as one could logically expect. While it is difficult to determine what these differences are, without more specific analyses of the context of each election, it is possible to make at least some educated guesses, partially building on existing research. First, Hix and Marsh (2011, 6) argued that the 1979 result might have been caused by the newness of popular European elections and that some of that newness effect may have persisted in following elections, as new countries joined the EU. However, especially the second part of this proposition does not seem to have much validity. There were no new countries in 2019, while the difference was the greatest ever in 2004, when the largest number of new member states joined the EU at once.

It is thus unclear why the gap tightened in 2019, especially compared to the preceding three European elections. One possible explanation is that the 2019 European elections were held in a relatively calmer political context than the previous three elections, which could have been affected by the EU enlargement (2004) and financial crisis (2009 and, to some extent, 2014). Another explanation is the possible effect of the transformation of national party systems in Europe, most notably characterized by the rise of "challenger parties" (e.g. de Vries and Hobolt

2020). This may have led to smaller cabinets and decreased clarity of domination of certain parties.

The suggestion is thus also related to the *clarity of responsibility* theory. As mainstream parties often opt to create *cordon sanitaires* around these "challenger parties", when these parties become very large, it may lead to (even) more fragmented government coalitions, composed by many small and often ideologically diverse parties. These coalitions often lack a dominant party as well as ideological cohesion. It has been demonstrated that this will make it more difficult for voters to hold governments accountable (see Hobolt et al. 2013). Consequentially, if these situations are more common, we can see less punishment of government parties in European elections. For instance, only 12 times (out of 166 cases), the PM party was not the largest party in the national party system and 5 of these cases were in the 2019 elections dataset. Indeed, as Figure 1.1 also shows, government parties performed on average worse in the national elections before the 2019 European elections than in any other national elections preceding European elections. If this explanation holds, we should expect to see similar outcomes in future European elections.

Multivariate analyses confirmed that government status significantly influences the difference in party vote shares between national and European elections. Government parties, on average, lose significantly more votes in European elections than opposition parties. However, these losses are conditional upon election timing, party size, and turnout.

First, government parties have a comparative advantage over opposition parties in European elections held shortly after national elections (*honeymoon*) but a comparative disadvantage in European elections held late in the NEC (*late term*). The losses become

insignificant only for a very short period right after the *honeymoon* and at the very end of the NEC. The results were almost identical for both measures of the effects of election timing.

There is thus rather strong support for the first mechanism of SOE theory. European elections held in a (rather short) *honeymoon* period seem indeed to be dominated by post-electoral euphoria amongst strongly mobilized government party voters, while depressing and demobilizing for opposition party voters. Since there is no evidence that voters defect from government parties in European elections held shortly after national ones, it seems plausible to reject the alternative mechanism proposed by MPO theory (hypothesis 1a), arguing for the prevalence of *sincere* voting at this stage of the NEC.

The results are more debatable in terms of the proposed *midterm* and *late term* effects (hypotheses 2 and 3). This is mostly because the somewhat vague description of this effect by SOE theory, especially in terms of magnitudes of expected losses and gains in different periods of the NEC, as well as the causes of these losses and gains. This is especially unclear about the suggestion that governing parties will regain some support towards the end of the NEC. In my interpretation, considering the US midterm election models upon which SOE theory is built, the regaining of support should be relative to both the *midterm* and the *honeymoon* performance. In this sense, we would expect the losses to be lower than in the *midterm,* but still evident compared to *the honeymoon*. Even in this interpretation, however, we still do not know much about how big this recovery in support for government parties should be towards the end of the NEC. Moreover, we also do not know how long the *midterm* effect is expected to last. This shows some of the problems of using theories based on the US electoral context, where the *midterm* is clearly and firmly established.

Given these issues, it is somewhat difficult to interpret the results here. Still, I believe that there is more evidence for rejection than confirmation of hypotheses 2 and 3. First, the lowest point in terms of government performance in European elections comes only sometimes after the *midterm*. Second, government parties do not regain much support after this lowest point and nothing compared to the *midterm*. Moreover, their losses, relative to opposition parties, remain statistically significant until the very end of the NEC, when there is no effect discernable from zero.

On the other hand, these results are rather supportive of the second alternative mechanism proposed by MPO theory (hypothesis 3a). Government parties clearly do lose more votes than opposition parties when European elections are held further away from the last national elections. This finding leaves open the possibility that an increased number of voters may choose to strategically defect and cast a protest vote (*vote with the boot*) in the late stages of the NEC.

Second, similar to election timing, party size significantly affects party performance in European elections. The larger a party, the more disadvantaged it is in European elections. This finding supports the basic logic of SOE theory. In this chapter, I was particularly interested in how party size affects the performance of government parties, especially given suggestions in the existing literature that losses of government parties in European elections are caused by the fact that these parties are generally larger than other parties and not by the fact that they are in government (Curtice 1989).

Multivariate analyses show that being in government significantly influences the difference in vote shares between national and European elections, even when we control for party size. At the same time, the analyses also demonstrate that there is a significant relationship between size and government status of a party in determining the difference in vote shares

between elections. As we have seen, the performance of small parties is not significantly impacted by their government status, while that of (very) larger parties is. These results are not that problematic for SOE theory though, and still do not provide any evidence for hypothesis 5. In short, while it is true that party size affects all parties, large government parties are even disadvantaged compared to large opposition parties in European elections (see also Weber 2011; cf. Curtice 1989).

A more general implication of the party size finding is that sincere voting (*with the heart*) is apparent in European elections. However, the fact that large government parties are particularly disadvantaged in European elections also provides possible evidence for strategic defection or so-called voting *with the boot*. We have seen that the prevalence of either is likely determined by election timing, of the European election in terms of the NEC, and thus electoral context.

Third, in terms of the effects of turnout, the regression analyses show that government parties are significantly more disadvantaged in European elections where turnout is (very) low compared to previous national elections. These findings contradict those of van der Eijk and van Egmond (2007) but potentially confirm that opposition voters are more mobilized in European elections than government voters, which would explain why government parties suffer the most when turnout is much lower in European elections than in the preceding national elections (Reif and Schmitt 1980, 10).

However, building upon previously mentioned literature (Weber 2011), we could also interpret these results as supportive of the variation in the context of European elections across cases. In short, the effect of the difference in turnout can be explained by the "protests through abstention" (Ibid., 907-8), meaning that in certain electoral contexts some core voters of

government parties chose to punish them by their non-participation rather than by defecting to some other party (see also Martins and Veiga 2014). This can logically be tied to the MPO theory proposition that the context of European elections changes according to the position of these elections in the NEC.

In terms of another mentioned mechanism alternative to SOE theory, the results of the analyses of the combined effects of economic performance and election timing (hypothesis 7) were somewhat mixed and overall, far from convincing. Kousser (2004, 9) argued that "unemployment should 'interact' with the timing of elections, with its negative effect on governing parties' support coming in mid- and late-term races." He used only unemployment as a measure of economic performance, as other variables (inflation and GDP growth) did not affect the electoral fortunes of governments in his analysis. This already makes the argument about the impact of economic performance on support for government parties in European elections somewhat less valid. My findings are very similar in this matter. GDP growth does not affect the change in vote share of governing parties between national and European elections at any point of the NEC. Unemployment, on the other hand, affects government performance in European elections when these happen around the *midterm* of the NEC. However, there is no significant effect towards the end of the NEC.

While the support for Kousser's proposition is thus rather limited, it is still interesting to observe that economic performance potentially affects the vote for government parties around *the midterm* point of the NEC. From the perspective of SOE theory, this is something that we would expect, given that the *midterm* should be the point where European elections are truly referendums on government performance. In terms of voter motivations, it could mean that voters may have economic performance in their minds (*economic voting*) and vote

retrospectively in European elections held at *midterm*, but more prospective in the *late term* of

the NEC, when the next national election is looming closer and other issues might become more

important in shaping their vote. Individual-level analyses of electoral behavior in European

elections should thus take economic voting into account (which is not often the case).

Finally, in terms of the *Clarity of Responsibility* hypotheses, the analysis in model 6

confirmed the effects of the party's dominant position in the coalition. This suggests that the

dominant position of the PM or senior government party in a coalition is indeed recognized by

voters. Voters thus likely find these parties particularly responsible for "poor" government

performance (whether real or caused by unfulfilled unrealistic expectations). Consequentially,

they choose to punish these specific government parties, rather than junior government parties

(or opposition parties) in European elections.

Conclusions

To sum up, the major findings of this chapter are: (1) government status of a party significantly

affects its performance in European elections – on average, government parties lose more votes

than opposition parties in European elections; (2) the losses of governing parties in European

elections are mediated by election timing, party size, their position in government coalitions, and

the difference in turnout between European and national elections; (3) an increase in

unemployment has a negative impact on the vote for government parties in European elections,

but only if these are held around the *midterm* of the NEC.

Taken together, these findings are rather supportive of most propositions of SOE theory

about government parties. But they also present an issue for its general thesis that government

parties are the losers of European elections. This is related to the often contradictory predictions of SOE theory in terms of its general hypotheses and proposed mechanisms. In short, the results of the analyses are for the most part not contrary to the proposed mechanisms of SOE theory, but they do suggest that the initial universalistic statement on government party losses is technically incorrect. Government parties do not always lose in European elections. Conditional upon their size, coalition position, and the election timing, they can also win in these elections. Consequentially, we can conclude that while the support for SOE theory in terms of the party government status remains quite strong some forty years later, some modifications and updates should be added.

Finally, through the aggregate-level analyses in this chapter, we have also learned something about individual-level mechanisms (voter motivations). First, the confirmation of the SOE theory's prediction regarding the so-called *honeymoon* effect, shows that European elections held shortly after national elections seem indeed to be dominated by post-electoral euphoria of government party voters and depression and demobilization of opposition voters. On the other hand, both sincere and protest voting seems to be more frequent when European elections are held after the *honeymoon* period, which lasts for some 100 days after the last national election. However, at the very end of the NEC, and thus just days before the next national election, the situation is a bit unclear, given that no significant difference in the performance of government and opposition parties is found. The following chapter should shed more light on these mechanisms.

CHAPTER 2

Voice: The Extreme Vote in European Elections

In the previous chapter, I have addressed the key proposition of SOE theory about governing

parties, i.e., that they lose votes in European elections compared to the preceding national

elections. We have seen that this proposition was generally confirmed, but not all types of

government parties are affected equally. More specifically, their losses are conditioned upon

election timing, party size, position in the government coalition, and difference in turnout. Given

these findings, the next logical step is to investigate who, and under which circumstances, are the

winners of European elections (cf. van der Eijk and Franklin 1996; van der Eijk et al. 1996).

This question has, somewhat surprisingly, not gained much attention in the scholarly

community (see van der Eijk et al. 1996; Oppenhuis et al. 1996). We have seen that SOE theory

only vaguely suggests that European elections offer brighter prospects for small and new parties

(Reif and Schmitt 1980, 9). However, the terms 'small' and 'new' are arguably too broad, partly

overlapping, and SOE theory does not truly define or operationalize them. Moreover, with

regards to the predicted *winners* of European elections, SOE theory leaves out other potentially

important variables, such as party ideology or election timing.

One small specification came from Reif (1984, 247), who added in a follow-up article

that European elections should also offer brighter prospects for "more radical" parties. In his

later reflection on the state of SOE theory, he wrote

"Had I to write the article today, I would not only speak of 'small and new' parties having a better chance at SOE than big, established, parties (as in 1980) or, in addition, of 'radical' parties (as in Reif 1984b,c) but also of 'protest' and 'populist' parties. The reasoning behind this proposition is the relative capacity of a party to mobilise voters either because of strong organisational resources (e.g. Communist parties before the mid-end-1980 and/or because leaders of established parties and the mass media have already contributed to their visibility)." (1997, 118)

This was merely a reflection on the state of SOE theory, as Reif did not provide us with any empirical evidence for his argument. Even in the most recent assessment on the theory, one of its original authors, Hermann Schmitt (and his collaborators), confirm that the theory predicts a comparative advantage to (1) small and (2) ideologically extreme and protest parties" (Schmitt et al. 2020, 7). While Schmitt and colleagues provide very good insights on individual voters' decisions in the 2004 and 2014 European elections in this article, they neither test to which parties voters switch nor explain the predicted effects of party extremity.

Studying the impact of party extremity on its performance in European elections is somewhat complicated given that SOE theory suffers from some important categorization, conceptualization, and measurement problems, which become particularly evident here. As already mentioned in the introduction of this study, definitions and measurements of concepts such as 'small', 'extreme', 'radical', 'niche', 'protest', 'populist', or 'established' are completely lacking. This is especially problematic because of the transformations of European party systems in the past decades (e.g. Mair 2013), which might have changed the meaning and function of some of these concepts.

SOE theory often uses these terms almost interchangeably. This is somewhat understandable, given that when the theory was written, they indeed often overlapped. In the 1980s, and to large extent even in the 1990s, it was almost unimaginable that these types of

parties would ever reach some considerable and consistent electoral success, let alone become part of the mainstream (cf. Mudde 2019a), or be in government (cf. Heinisch 2003; McDonnell and Newell 2011; Mudde 2013; Albertazzi and McDonnell 2015). However, many 'radical', 'protest', 'outsider' and 'populist' parties today are among the largest and most established parties in their respective party systems (Mudde 2019a; de Vries and Hobolt 2020). And, in Sartori's famous terms (1976), many of them have now acquired not only blackmail but also coalition potential.

The missing definitions and measurements of the key terms radical and extreme is clearly problematic for any study that strives to empirically assess the effects of extremity on electoral behavior in European elections. Any such study will thus need to first address this issue. Consequentially, I strive to offer novel conceptualization, categorization, and measurement criteria necessary for the analysis of the effects of ideological extremity on electoral behavior in European elections and elsewhere. Moreover, given that the function of these so-called "extreme" parties in their party systems often changed radically over time, it is also necessary to evaluate the effects of variables that were previously not taken into account, either theoretically nor empirically, especially party size and government status.

Furthermore, considering the suggested crucial impact of the effects of election timing on the government performance (see Chapter 1), it is rather surprising that SOE theory has not predicted any effects of election timing on the performance of parties that should experience a boost in support in European elections. This could be because the theory either simply believes that (1) election timing only impacts government parties or (2) because it posits that the effects of election timing on the performance of opposition parties will be exactly the opposite of those on the performance of government parties.

Both of these interpretations are highly problematic. First, if election timing strongly affects the performance of some major parties in the system, it will logically affect the performance of other parties in that system as well. Second, given the heterogeneity of opposition parties, and taking into account other propositions of the theory on the effects of party size and ideological extremity, a uniform effect of election timing on non-governmental parties is rather improbable. Consequentially, an evaluation of the effects of election timing on parties that are considered the *winners* of European elections is necessary, and one of the main aims of this chapter.

In addition to these theoretical and empirical issues, previous studies on the vote for extreme parties in European elections seldom found a general pattern of increase in support (e.g. Oppenhuis et al. 1996; Minkenberg and Perrineau 2007; Ivaldi 2012; see also Ferrara and Weishaupt 2004; Mudde 2019b; cf. van Egmond 2007). Moreover, as has been also argued already in the early 1990s (e.g. Ignazi 1992), the extremity of a party might lead to an overall decline rather than an increase in electoral support. Further and more systematic testing is thus necessary.

Motivated by these gaps in the literature, unconvincing results of the few existing analyses, categorization, conceptualization, and measurement issues, and the prospect to learn more about voter motivations, this chapter offers a comparative analysis of the performance of so-called extreme parties in European and national elections. This analysis will also serve the theoretical purpose of unpacking SOE theory's general hypotheses. In this sense, I again first inquire whether the 'big' argument that extreme parties gain votes in European elections, compared to national elections, is generally correct and then deconstruct it into more nuanced, smaller inquiries that strive to address the effects of ideological extremity.

The chapter is organized as follows. In the first part, I analyze the theoretical propositions of SOE theory on the performance of extreme parties in European elections, address the party categorization issues, and develop a variety of testable hypotheses. The second part empirically assesses the proposed hypotheses on the basis of European elections of the past four decades and in the third part I discuss the findings. Finally, the conclusive part offers a brief summary of these findings and their theoretical consequence.

Theoretical Analysis

The SOE model does not truly offer a theoretical explanation for the expected advantage of extreme parties in European elections. Other authors have suggested that this proposition is arguably driven by the "fact" that small parties are often more radical (see van der Eijk et al. 1996, 151). It is indeed safe to assume that the SOE model treats small and extreme parties as the same or at least as functionally equivalent. The mechanism here should thus still be based on the 'less-at-stake' dimension and ideological proximity/sincere voting (Reif and Schmitt 1980, 9; Schmitt et al. 2020, 7).

This is also clear from Reif's description below:

> "At FOE [first-order elections], there is more 'tactical voting'. Some people who ideologically feel close to a more extreme and/or small and/or new and 'untested' party which has little chance of weighing enough to really have chance of influencing the policy of the future government, vote for the one of the bigger established parties which comes closest to their own political orientation. At SOE, they 'follow their heart' or tentatively 'try' a new party. Consequently, smaller, newer and more extreme parties have somewhat brighter prospects of mobilizing support than bigger and more established parties. This effect is enhanced by the tendency of loyal voters of the non-established parties to mobilize somewhat more readily at any kind of election" (1984, 246).

In other words, given that the stakes are lower in (European) SOE elections, voters who ideologically feel closer to 'small', 'new', and 'extreme' parties should be liberated from strategic considerations that drive their vote choice in first-order elections (see also Marsh and Franklin 1996, 21; Duch and Palmer 2002; van Egmond 2007). Drawing further upon the SOE logic, if voters truly do not care much about the outcome of European elections, they should also be less reluctant to cast a vote for a party they feel close to but also believe to be too extreme to have any real power.

However, treating 'small' and 'extreme' (and to some extent 'new') as functionally equivalent is highly problematic, especially given the transformations of European party politics in the last decades. These changes have arguably started with the transformation, as well as crisis, of traditional mainstream political parties (see e.g. Kirchheimer 1966; Wolinetz 1991; Katz and Mair 1995; Ignazi 1996; Mair and van Biezen 2001; Mair 2013), followed by the rise and consolidation of parties that were long considered marginal or "niche" (cf. Adams et al. 2006; Meguid 2005; 2008; Jensen and Spoon 2010; Wagner 2011), especially the far right (see e.g. Minkenberg 2013; Akkerman et al. 2016; Mudde 2019a; de Vries and Hobolt 2020). Consequentially, some parties that SOE theory probably labels as 'extreme' have not just increased in size but also acquired blackmail and even coalition potential (cf. Sartori 1976). Indeed, a growing number of extreme parties have been included in governing coalitions in the past two decades (e.g. the Austrian Freedom Party or the League in Italy), and not only as junior partners. (e.g. Five Star Movement in Italy or Syriza in Greece). In this sense, the lines between "mainstream" and "extreme" have become blurry.

While it was almost unimaginable (with some notable exceptions, consisting mainly of communist parties before 1989) that an extreme party could be one of, or even, the largest party in a party system, let alone be in the government, when Reif and Schmitt were writing their seminal study, this no longer holds today. The effects of these transformations thus logically need to be taken into account when analyzing the original SOE proposition. Specifically, we need to pay special attention to the relation between party size, ideological extremity, and government status.

While SOE theory has also predicted the impact of party newness, I do not evaluate this effect for several reasons. First and foremost, the measurement of the term "new" is again somewhat unclear in SOE theory and the term is difficult to operationalize in an analysis like this one. For instance, should new parties be those for which the European election is the first election they have ever contested? If that would be the case, it would be difficult to evaluate their success in European elections from a comparative perspective, as there is no "baseline" (previous national elections) to compare their results with. Connected to this, it would be rather complicated to establish how long "newness" lasts. Is a party still new if it has already participated in an election before the European election? Finally, a new party is often solely a splinter from another already established party and it is thus often questionable how "new" it truly is.

In terms of party classification, the novel categorization by Mattia Zulianello (2018, 2019a) offers several practical advantages. Given the rather generic basic proposition of SOE theory, I strive for the broadest possible set of parties that could be seen as extreme. Consequentially, I focus on only one core common feature that should signal the party's ideological extremity (cf. Sartori 1976; Eatwell 1996; Mudde 2007). Clearly, this leads to a

group of parties that are in many aspects very different from each other and can also be studied separately. But, given this one common denominator, they can be all considered ideologically extreme to some extent (see Mudde 2007, 15).

Zulianello's (2018, 2019a) party categorization framework provides a particularly useful tool to find this common core feature of extreme parties. This is especially the case for an aggregate-level analysis with a large time span, which cannot rely on individual voters perceptions of every single party from 1979 (and earlier) until today (cf. Oppenhuis et al. 1996; van Egmond 2007), given data scarcity and imperfection. This framework provides clearly established criteria to categorize parties on the basis of their ideological "anti-metapolitical" opposition and their visible interactions with the political system. The former will be particularly crucial for this analysis.

Zulianello (2018, 654; 2019a, 3-4) identifies four types of political parties: anti-system, halfway house (or negatively integrated), complementary, and pro-system. For our task, I concentrate in particular on the first two types, while the latter two categories constitute a baseline in my analysis. This does not mean that I suggest that we should always treat anti-system and halfway house parties as identical for other purposes. It is indeed one of the goals of Zulianello's framework to clarify that they are different. However, they have a common ideological future that is crucial for this analysis. In the following paragraphs, I shortly describe how Zulianello defines his party types and further explain why we can bundle 'anti-system' and 'halfway house' parties to test my hypotheses.

In terms of defining the anti-system party, Zulianello (2018, 657) draws on seminal works of Sartori (1976, 1982) and Capoccia (2002). He proposes two features that need to be simultaneously present to define a party as anti-system:

(1) "the ideological orientation of a party towards the status quo does not simply result in the articulation of a conventional anti-incumbent and policy-oriented opposition, but also in questioning the established metapolicies";
(2) a party cannot take part in "very visible cooperative interactions at the systemic level […] or despite a previous involvement in such interactions deliberately favor a return to the margins of the party system through the process of radical disembedding".

Regarding the first feature, metapolicies are defined as

"the choices that concern the basic arrangements of the political regime, of the political community or of the social and economic system, or else the country's location in an international system of alliances expressing fundamental conflict between two sides, or, lastly, support for all-encompassing visions of the world" (Cotta 1996, 29).

In this sense, metapolicies "refer to crucial values and/or practices of the political, social or economic system that are enshrined by the existing order" and are ranked higher than "*mesopolicies* (e.g. the transformation of the electoral system; a reform of the pension system) and *micropolicies* (e.g. patronage), as a party questioning one or more metapolicies challenges crucial elements of the status quo" (Zulianello 2018, 659; see also McDonnell and Newell 2011, 445)[12].

In the first four decades after World War II, the most prominent *anti-system parties* in Western Europe were communist parties but in the last two or three decades, the best examples are populist parties, whether they are "radical right," "right," "center/technocratic/valence," "left," or "radical left" (see Caramani 2017; Bonikowski et al. 2019; Buštíková and Guasti 2019; Zulianello 2019a). Other examples of anti-system parties, together with the few remaining

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[12] For a detailed description of which metapolicies a party can question see Zulianello (2018, 660–2).

radical left communist parties (see Hanley 2001), are "neo-fascist" or "extreme right" parties (see Mudde 2007, 49-50; see also Ignazi 1992)[13].

Importantly, the anti-system character of a party is not static but dynamic in this framework. Zulianello observes that an anti-system party can, at various points in time, integrate into the system and thus cease to be anti-system (i.e., Syriza in Greece in 2015), or (more rarely) disembed from the system again, and thus return to be 'anti-system' (i.e., the Austrian FPÖ in 2005). The first process is defined as "systemic integration," the second as "radical disembedding" (Zulianello 2018, 663-5; 2019a, 34-7).

Systemic integration is the key to defining the *halfway house parties* and can be achieved in multiple direct or indirect ways (see Zulianello 2018, 662-5; 2019a, 34-6). The most notable ones are: (a) becoming the leading party of the system, and thus directly influencing its functioning; (b) becoming a member of a governing coalition with mainstream pro-system parties; or (c) acquiring "coalition potential," which means that a party is, on the one hand, perceived as a potential coalition partner by mainstream parties and, on the other, itself available to cooperate with one or more mainstream parties (Ibid. , 664; see also Sartori 1976; Bolleyer 2008).

Systemic integration can be negative or positive. Negative systemic integration leads to a 'halfway house' party, positive systemic integration to a full-fledged (pro-)system party. Negative integration means that, "despite integration into cooperative interactions at the systemic level, [the] ideological core [of a party] remains in contrast with one or more crucial elements of the metapolitical system" (Zulianello 2019a, 4). In contrast, positive integration indicates that a

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[13] In that way, the interchangeable use of the terms "extreme" and "radical" is not overly problematic, as both radical and extreme parties serve a similar function in their system and fit the theoretical expectations of the aforementioned theories as parties that should enjoy an electoral boost in support in European elections, compared to national elections.

(former) anti-system party not only visibly interacts with the system but also substantively moderated its core ideological concepts (Ibid.). In other words, a positively integrated party clearly accepts the metapolicies of the system, while negatively integrated (or 'halfway house') parties continue to contest one or more of these metapolicies, while being visibly integrated into the system (see also Zulianello 2018, 666).

Regarding the remaining two types of parties, *pro-system parties* have "a 'conventional' anti-incumbent and policy-oriented ideological profile and present the property of systemic integration" (Zulianello 2018, 668), while *complementary parties* may "tactically adopt an antagonistic stance [for which] they have not been integrated in visible cooperative interactions, either because they present themselves as unavailable for cooperation with mainstream parties or because the latter may view such parties suspiciously" (Ibid., 669).

Consequentially, the crucial difference between anti-system parties and halfway house parties is in their visible interactions with the system, while they share an ideological contestation of one or more "crucial values and/or practices of the political, social or economic system that are enshrined by the existing order" (metapolicies) of the system. In this crucial aspect, then, they differ from the other two party types (complementary and pro-system). Hence, to analyze the SOE proposition on the effects of party extremity on its performance in European elections, I classify anti-system and halfway house parties as ideologically extreme and complementary and pro-system parties as mainstream or non-extreme (see Table 2.1; see also Zulianello 2018, Table 1, 667). Thus, the contestation of one of more metapolicies of the system is the core concept of my "minimum" definition (cf. Sartori 1976; Eatwell 1996; Mudde 2007) and the measure of ideological extremity that I employ to categorize European political parties from 1979 (and earlier) till today.

Table 2.1: Party Types and Ideological Extremity

Party Type	Anti-metapolitical opposition	Systemic Integration	Ideologically extremity
Anti-system	yes	no	**yes**
Halfway house	yes	yes	**yes**
Complementary	no	no	**no**
Pro-system	no	yes	**no**

I believe that this novel classification and measurement of ideological extremity provides me with a considerable advantage over studies concentrating on solely one or another type of extreme parties (e.g. Minkenberg and Perrineau 2007; Ivaldi 2012) or using fairly problematic and somewhat dated scaled measures of ideological extremity based on individual-level survey data.[14] It allows me to include a wide variety of parties that significantly differ from one another on different issues yet are ideologically extreme in their opposition to core ideological features of the metapolitical system. Using this classification, I evaluate the first basic hypothesis of SOE theory on the role of ideological extremity on party performance in European elections:

Second Order Elections Theory Hypothesis

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[14] For instance, Oppenhuis et al. (1996, 293) define the extremity of a party as "the difference between the median position of the party and the median of the electorate, both measured on 10-point scales" (see also van Egmond 2007). This fairly vague definition seems to be quite risk-prone to producing inaccurate results. For instance, the left-leaning voter may simply consider everything on the right as "extreme" and vice versa. Indeed, when we look at the list of parties they include (see van der Eijk and Oppenhuis 1996, 428-31), we can spot some rather surprising results. Generally, the scores of classic mainstream center-right and center-right parties on extremity are high in almost every party system, and in some cases, such as the PP in Spain, even higher than some of far-right and far-left parties. Moreover, a similar assessment of ideological extremity through survey data can be done for smaller-scale analyses, including only a limited number of elections or countries, but would be very difficult to do for large-scale analyses such as mine.

(1) Ideologically extreme parties perform better in European elections than in previous national elections.

Party Size and Election Timing

After evaluating this basic hypothesis, the next step is to add other theoretically important "trigger" variables (cf. Schmitt et al. 2020), and thus get a better grasp of the underlying mechanisms. Adding these variables should, ultimately, help us understand why and when ideologically extreme parties are *winners* in European elections. Moreover, this analysis should also teach us more about voter motivations in European elections. Specifically, whether some voters really use these elections to raise their 'voice' (cf. Hirschman 1970), and thus vote *with the boot* to signal their discontent, and in which contexts are they more likely to do so.

To unpack the big first hypothesis into several smaller hypotheses, we build upon the logic of the SOE model. However, the proposed mechanism is rather simplistic and vague, as is often the case in such broad theories. Importantly, as already mentioned, SOE theory does not lay out the specific reason why specifically extreme parties would have an advantage in European elections. Particularly, how is the effect of party extremity related to party size and to the electoral context? Party size and election timing are thus our two 'trigger' variables.

First, as we have seen, SOE theory treats 'extreme' and 'small' almost interchangeably. While this is highly problematic in the current context, in which not all extreme parties are small, it can still be argued that to be successful in European elections, an extreme party needs to be also small. For instance, in the previously mentioned follow-up to SOE theory, Oppenhuis et al. (1996, 302) indeed talked specifically about the success of "radical small parties". While neither

SOE theory nor following studies discuss (the possibility of) large extreme parties, it makes sense that with an increase in party size, there is no reason to vote strategically for another party in (national) FOE. At the same time, following the SOE logic, with an increase in size, even an extreme party will lose some of its appeal to voters in (European) SOE, given that voters in these elections generally prefer smaller parties (see also Chapter 1). Considering this, we should thus investigate whether the effects of ideological extremity are conditional upon party size. Therefore, I also evaluate the following:

Ideological Extremity and Party Size Hypothesis

> (2) The smaller an extreme party, the larger its gains in European elections relative to previous national elections.

Second, given the shortcomings of SOE theory in theorizing and testing the effects of election timing on the performance of non-governmental parties, we need to build upon other theories to address the second part of the question. MPO theory is particularly useful with this regard. We have seen that its key argument is that voters and politicians are aware of, and react to, the changing context of the European elections, which is given by election timing. In terms of the performance of extreme parties, Oppenhuis et al. (1996, 302) concluded that when the European election is held at a considerable temporal distance from the previous national election, it becomes a "marker-setting election" that

> "is characterized by an apparent lack of consequences for the allocation of power on the one hand and by the attentiveness of politicians and media on the other. In this circumstance, strategic voting may take form of what is generally referred to as 'protest voting', benefiting radical small parties in particular. Knowing that politicians are

attentive to the results while no actual power is at stake, some voters apparently take the opportunity (in the phrase of the British football hooligans) to 'put in the boot'".

These conclusions were motivated by the results of their analysis of quasi-switching, i.e. a vote switch between the 1989 European election and a hypothetical national election. They found no independent effects of ideological extremity on party performance in European elections. However, the interactions with election timing were significant and extreme parties benefited the most when the European election was held "quite some time after the most recent national one" (Ibid., 297). It is thus reasonable to expect that the effects of party extremity will be conditional upon the position of the European election in the NEC. Consequentially, the third hypothesis is:

Ideological Extremity and Election Timing Hypothesis

(3) The more time elapses between the national and European election, the larger the gains for ideologically extreme parties.

Empirical Analysis

Data and Variables

Testing the presented hypotheses should tell us more about the mechanisms and thus the voter motivations in European elections. In the ideal world, we would conduct a panel survey and monitor the behavior of voters across elections to directly test these expectations, with a timespan from 1979 (and earlier) to 2019, but this sort of individual-level analysis is impossible.

First, even at the level of individual EU member states, suitable panel data for such an analysis is scarce at best (see Schmitt et al. 2020, 10). Second, existing survey data with both national and European elections across the continent usually rely on voter recall in multiple elections measured at one point in time. Schmitt et al. (2020) use such data and admit that this is less than perfect given "memory fading" and "wishful thinking coloured by the opinion climate of the day" of respondents (see also van der Eijk and Niemöller, 1979; Waldahl and Aardal, 1982, 2000). Moreover, the fluidity of party systems can mean that "choice options in one election are no longer available in the next because parties in between have split or merged or simply disappeared" (Schmitt et al. 2020, 10).

The available individual-level studies are thus inadequate in their scope and reliability. However, it is possible to use aggregate-level data to make some inferences about individual behavior. The unpacking of the general SOE proposition in terms of timing (hypothesis 3) is particularly useful here. For instance, if we observe a pattern of increases in the "extreme" vote when European elections are held at a considerable temporal distance from national ones, it is possible to assume that voters are indeed voting *with the boot* in this electoral context, while they do not, for instance, when the European elections are held shortly after the national one. Obviously, I am aware that there are many other possible micro-level mechanisms can drive the aggregate-level pattern, which I cannot capture with this analysis. However, the results can at least be indicative of certain mechanisms and could be tested subsequentially in smaller-scope individual-level case studies, possibly using national panel data.

As described above, to categorize different parties, I rely on the classification of parties by Zulianello (2018, 2019a, 2019b). For older parties, which are not always included, I employ

my own categorization, adhering to Zulianello's classification rules.[15] To ensure (face) validity

of my categorization, I compare it with the most recent wave of Chapel Hill Expert Survey–

CHES (Bakker et al. 2020). Although many (especially historical) parties are not included in

CHES, the results of this comparison mostly supported my classifications[16].

The dependent variable is the difference in vote share of a party between European

elections and preceding national elections (*ΔVote Share*), constructed by subtracting the total

vote shares of a party in the previous national election from vote shares of that party in the

following European election. I use the same electoral databases to extract the vote shares of the

parties as in the previous chapter (e.g. Döring and Manow 2020) and include all sets of European

and national elections from 1979 to 2019.

The key explanatory variable (*Extremity*) measures the ideological extremity of a party.

The variable is coded as 1 when a party is classified as extreme, i.e., as anti-system or halfway

house in Zulianello's classification, and 0 if non-extreme, and thus as pro-system or

complementary in his classification. Further models include variables that serve as controls in the

basic model but are hypothesized to interact with *Extremity*, notably *Party Size* (hypothesis 2),

Timesincenat², and *Cycle²,* which capture the effects of election timing. In addition, party

government status (*Govopp*) is included as a control variable here. It stems from the SOE logic

that whether a party is in government should strongly affect its performance in European

elections (see also chapter 1). While the theory does not provide any guidelines in this regard, it

is reasonable to expect that this effect will still be present even for extreme parties. I also control

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[15] I have also consulted some help of other datasets, such as the PopuList (Rooduijn et al. 2019), as well as Mattia Zulianello.

[16] For the list of anti-system and halfway house parties, see Appendix 2.1.

for changes in the economy, using two indicators of economic performance, changes in GDP

growth and unemployment rates (ΔGDP Growth and ΔUnempl).

Finally, it seems plausible to include variables that capture the effects of party ideology

in terms of left and right. As we have seen, SOE theory has not predicted any effects in this

matter. Moreover, a good portion of extreme (anti-system and halfway house) parties are not

adequately captured by a single left-right political dimension. Nevertheless, it is arguably still

interesting to see whether my classification of extremity is correlated with the left-right

dimension. I therefore constructed three categorical control variables: *FarRight*, *FarLeft*, and

Other. The first two are coded as 1 if a party is classified as far right or far left, respectively. The

third is coded as 1 when a party is neither far right nor far left.

To classify far-right and far-left parties, I mainly use the most recent and comprehensive

classifications of these parties, in particular the PopuList (Rooduijn et al. 2019). For parties that

are not included in these datasets, mostly minor and older parties, I use my own classification,

relying on criteria established in the relevant literature (e.g. March and Mudde 2005; Mudde

2007; 2017; March 2011; March and Rommerskirchen 2015). I prefer to use these controls over

continuous measures that could be retrieved from expert surveys (CHES), mostly because these

surveys include left-right scores for just slightly over half of the observations in my dataset (708

out of 1224).

Statistical Tests

In the first part of the analysis, I use descriptive statistics to compare the mean vote shares of

extreme parties in European and national elections for all sets of elections from 1979 to 2019 to

see whether these parties generally experience gains in European elections. Except for the average vote shares, and the differences between them, I also include two-tailed p-values to demonstrate the statistical significance of these differences, as well as percentage changes, to cover relative gains/losses.

In the second part of the analysis, I run several ordinary least squares (OLS) analyses with clustered standard errors by country to evaluate the independent effects of party extremity on the difference in vote shares between national and European elections (model 1) and to investigate the conditionality of this effect upon other factors, specifically party size (model 2) and electoral context (models 3 and 3a). Finally, I run four additional models (1a, 2a, 3a, and 3b), in which I add controls for left-right extremity, to observe whether this affects the results of previous models in any way, and thus whether my measure of extremity is orthogonal to left-right extremity.

Findings

The results of the first part of the analysis are presented in Table 2.2. The first observation is that the difference in vote shares of extreme parties is in the expected direction. However, it is only very marginal. Extreme parties gained only 0.05 percentage points in European elections compared to previous national ones, on average. This is equal to a relative increase of less than one percent (0.79%). hence, it is not surprising that these gains are not statistically significant (p-value = .81, two-tailed). The basic (SOE) hypotheses 1 is thus not completely supported: extreme parties, on average, barely gain any votes in European elections compared to national elections.

Table 2.2: Vote Shares of Extreme Parties in European and National Elections

	Nat	EE	Difference (EE – Nat)	% Change	p-value (two-tailed)
Mean Vote Share	6.36	6.41	.05	.79%	0.81
No. of Observations = 470					
EE = European Elections, Nat = National Elections					

The results of the regression models are presented in Table 2.3. First, the analysis in model 1 demonstrates that party extremity does affect vote share in European elections. However, this effect is in the opposite direction than expected. When controlling for potentially important variables of party performance (party size, election timing, government status, economic performance), extreme parties perform worse than non-extreme parties in European elections, relative to the previous national elections. The negative effect of party extremity is statistically significant (p<.01, two-sided). Hypothesis 1 is thus clearly rejected.

Table 2.3: Determinants of the Difference in Vote Share of Extreme Parties between European and National Elections

	Model 1	Model 2	Model 3	Model 3a
Extremity	-1.259***	-1.955***	-2.352***	-3.111*
	(.416)	(.566)	(1.368)	(1.610)
Party Size	-.190***	-.207***	-.188***	-.184***
	(.027)	(.028)	(.027)	(.028)
TimeSinceNat	-.001	-.001	-.002	

	(.001)	(.001)	(.002)	
TimeSinceNat^2	.000	.000	-.000	
	(.000)	(.000)	(.000)	
Extremity#Size		.084		
		(.063)		
Extremity#TimeSinceNat			.001	
			(.004)	
Extremity#TimeSinceNat^2			.000	
			(.000)	
Extremity# Cycle				7.166
				(6.240)
Extremity# Cycle^2				-5.463
				(5.826)
Govopp	-2.467***	-2.514***	-2.472***	-2.756***
	(.563)	(.561)	(.573)	(.625)
ΔUnempl	-.042	-.030	-.034	-.069
	(.049)	(.050)	(.049)	(.040)
ΔGDPGrowth	-.011	-.006	-.010	-.019
	(.027)	(.027)	(.026)	(.025)
Constant	3.355***	3.603***	3.713***	3.910***
	(.543)	(.503)	(.778)	(.859)
Observations	1,192	1,192	1,192	1,043
R-squared	.254	.259	.259	.270
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Second, model 2 evaluates whether the effect of party extremity is conditional upon party size (hypothesis 2). Figure 2.1 reveals that only for parties that have obtained between less than

13% in previous national elections party extremity is a significant determinant. However, this relation is again not in the expected direction. As we can see, compared to other parties, the smaller an extreme party, the worse its performance in European elections. Hypothesis 2 is thus also not supported.
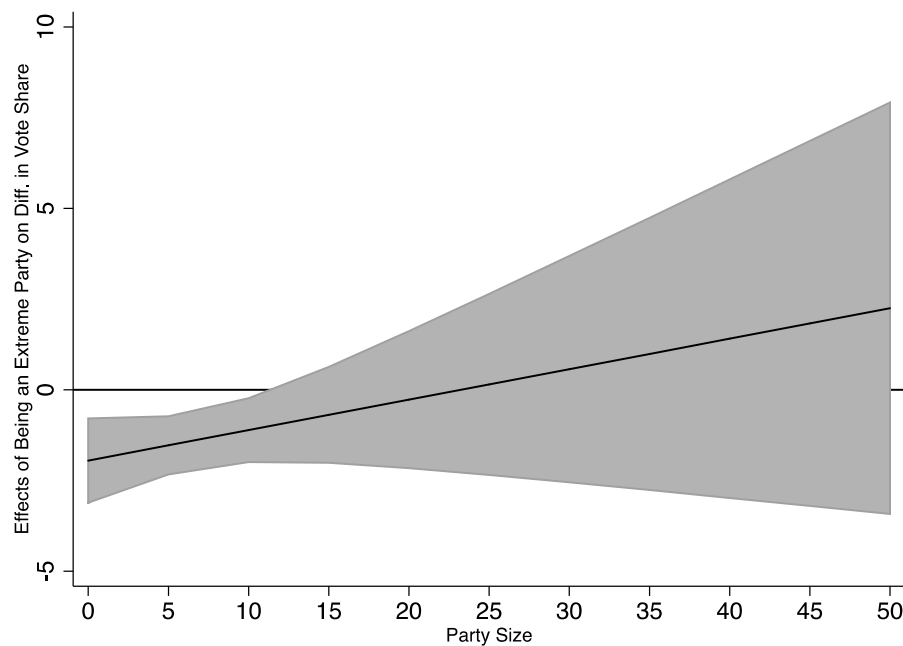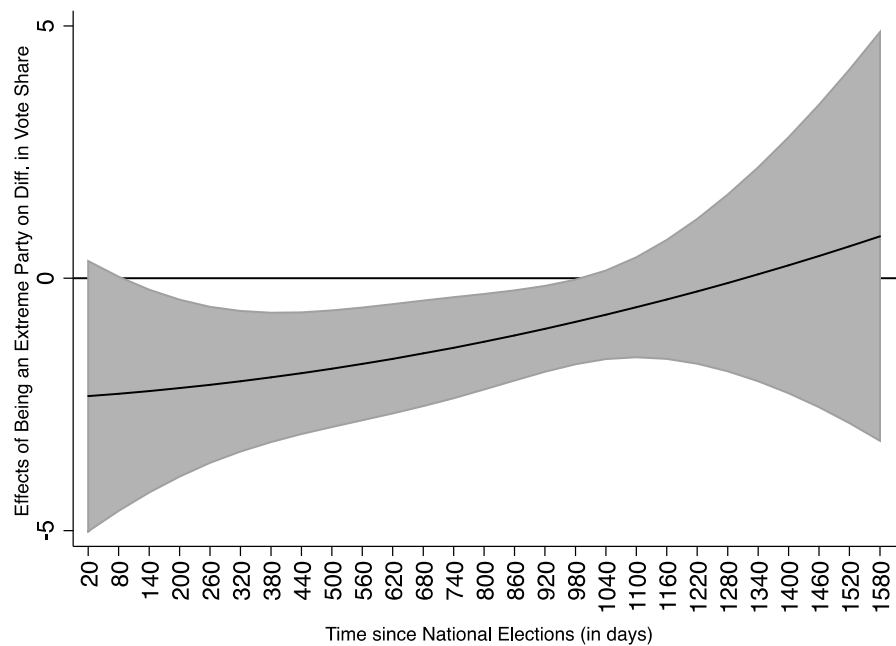

Figure 2.1: Average Marginal Effects of Being an Extreme Party (95% CIs)

Third, model 3 evaluated the relationship between party extremity and electoral context, measured by election timing (hypothesis 3). Figure 2.2 demonstrates that the effect of party extremity is significant when the European elections are held between roughly 80 to 980 days after national elections. While the performance of extreme parties in European elections improves over time, relative to non-extreme parties, it never becomes significantly better than in previous national elections. This is another rather surprising finding, rejecting hypothesis 3. When we switch to our second measure of election timing effects, $Cycle^2$ (model 3a), the effect

is smaller but overall, rather similar. Figure 2.2a, plotting this model, shows that party extremity has a significant negative impact on the difference in vote shares only between roughly 0.05 to 0.4 of the NEC, therefore when European elections are held shortly before the *midterm* of the NEC, while no clear impact before or after these points.

Finally, to control for potential correlation between my classification of ideological extremity and extremity in terms of left-right, I ran models 1 to 3a with the addition of left-right extremity controls. The results of these additional analyses are almost identical to the previous analyses in all models (see Appendix 2.2). This confirms that the left-right extremity is not an important variable for this analysis.

Figure 2.2: Average Marginal Effects of Being an Extreme Party (95% Cls)
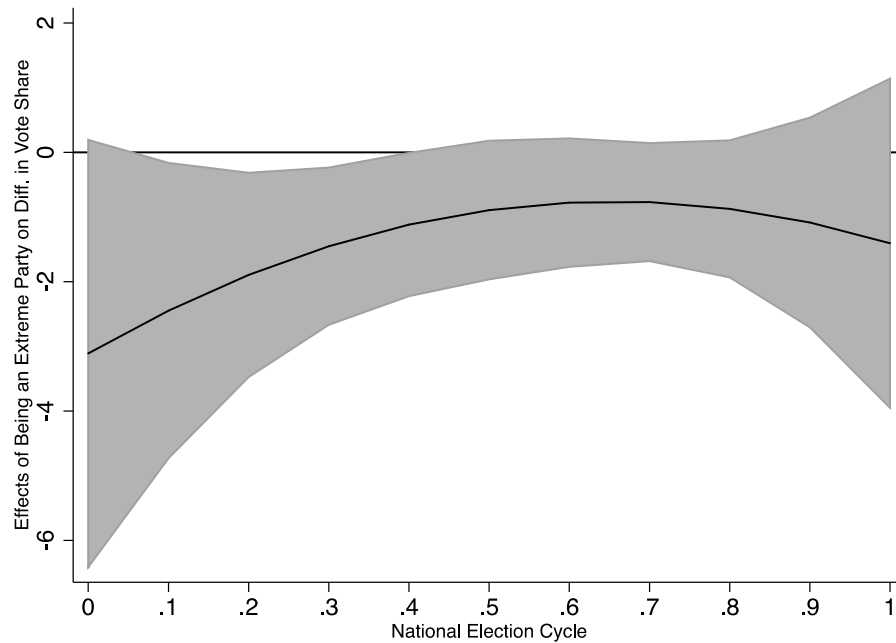
Figure 2.2a: Average Marginal Effects of Being an Extreme Party (95% Cls)

Discussion

This chapter investigated which parties profit in European elections, compared to (previous) national elections, according to SOE theory. The theory posits that European elections offer brighter prospects for small and extreme parties. This expectation was elaborated upon by MPO theory, which has added that the fate of extreme parties is determined by electoral context, specifically by the temporal distance of the European elections to the national ones. Consequentially, to evaluate who the expected *winner* of (European) SOE is, it was necessary to test not only the simple proposition of the effect of party extremity but also address the (mediating) effects of two "trigger" variables: party size and election timing. This is particularly important in the face of the recent transformations of European party politics, which have radically impacted even the previously stable party systems. More specifically, we had to take

into account that extreme parties are no longer all small and that they can be both in opposition and in government. Both of these facts could significantly impact the original SOE logic.

The results of this inquiry were somewhat surprising and again suggest the necessity to update SOE theory (as well as its subsequent re-elaborations, such as MPO theory). First, the basic hypothesis that ideologically extreme parties perform better in (European) SOE was not really supported. The gains of these parties are on average almost non-existent, even when we look at relative change in vote shares between previous national and European elections. Their performance in European elections is therefore rather similar to that in national elections. Moreover, multivariate analysis demonstrated that when we take into account other determinants of party performance in SOE, party extremity actually significantly decreases the likelihood of any gains in (European) SOE. In other words, on average, non-extreme parties performed better than extreme parties in European elections from 1979 to 2019.

Second, the analyses on the effects of our two "trigger" variables, and their relation to ideological extremity, showed a significant impact on party performance but not in the expected directions. We expected the effect of party extremity to be conditional upon party size and that only small extreme parties will have a comparative advantage in European elections (hypothesis 2). The analyses demonstrate that party size does indeed have a significant impact on the effect of party extremity on the difference in vote share between national and European elections. However, the effect was in the opposite direction. The smaller an extreme party, the worse its loss in European elections, relative to previous national, compared to non-extreme parties. Based on these findings, hypothesis 2 is rejected.

This finding could show a possible "new" logic regarding the extreme party vote in European elections. I have emphasized that neither SOE nor its following elaborations (such as

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MPO theory) seem to consider the possibility that not all extreme parties are small, probably because when these theories were written this combination did not exist. However, things have changed. To some extent, it makes sense that small extreme parties no longer have much of an advantage over small non-extreme parties in European elections. One rather simple explanation, for instance, is that if voters truly want to *put in the boot* by voting for an extreme party, they will arguably prefer the largest extreme party in the system. However, if that would be true, we would arguably see some significant improvement in the performance of extreme parties with an increase in their size, and this is not the case.

A completely different explanation is that, given an overall increase in their size, many extreme parties simply became too important players in their respective party systems. Voters vote for them already in FOE and thus do not have many reasons to subsequently vote for them in SOE to express "voice." This would have some broader implications though, as it suggests that (some) voters are now willing to cast a protest (anti-metapolitical) vote in FOE but not in SOE.

One way to preliminarily (and imperfectly) evaluate this suggestion, is to simply compare the average vote shares of extreme parties across all sets of elections. Figure 2.3 shows that their vote shares in national elections have indeed grown gradually since the 1994 European elections. While their votes were increasing correspondingly, and they were generally performing better in European elections,[17] this has changed in the 2019 European elections. In 2019, their national average was the highest ever, while the European average was worse than the national average

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[17] While their gains may seem marginal in terms of percentage points, especially in these initial years – in 1984, the relative increases (about 1%) were not that small, given the size of extreme parties at that time. For instance, in 1984, the relative increase is around 26%, which is quite sizeable. However, a gradual increase in the size of these parties also meant a decrease in relative gains. For instance, in 2014, the 0.6 absolute increase (from 7.5% in national to 8.1% in European) was a relative increase of only 8%.

for the first time since 1979 – as well as the worst in general since 2004. Moreover, the

difference in vote shares was also the highest in history, going from 8.7% to 7.3%, which

presents a 16% decrease.
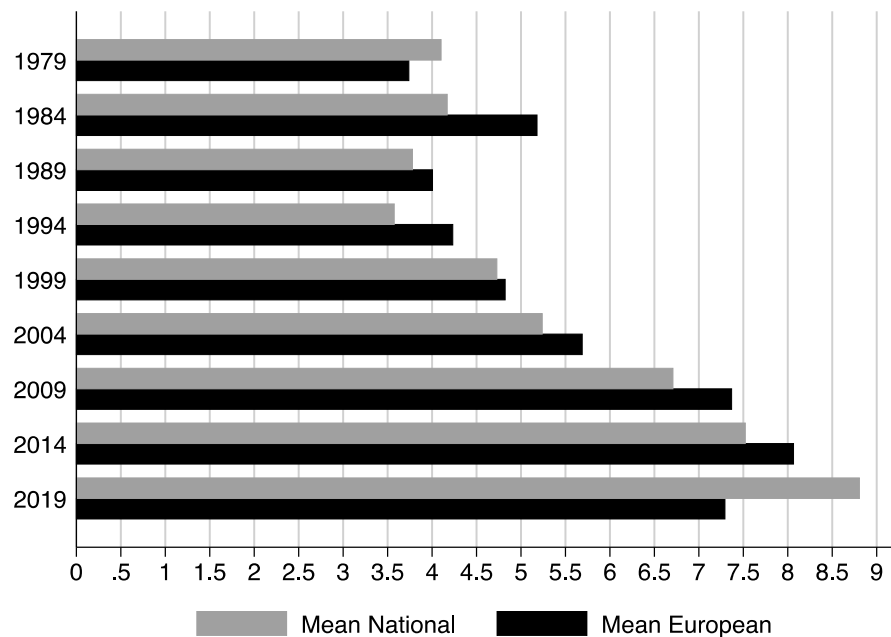
Figure 2.3: Difference in Vote Shares of Extreme Parties over Time

Although very preliminarily at this point, this could indeed suggest that if a vote for an

extreme party is truly a protest vote, some voters are increasingly ready to cast it in (national)

FOE, thereby losing an incentive to cast it (again) in (European) SOE. Somewhat connected to

this, the incentive to vote for extreme parties in European elections could have decreased

because some of these parties are, or recently were, in government. Still, it is rather improbable

that this could account for the general underperformance of extreme parties in European

elections, given that it is still fairly rare for these parties to partake in government coalitions.

Indeed, over 90% of extreme parties in my dataset was in opposition at the time of the European

election – even in 2019 81% of extreme parties were in opposition. Moreover, in multivariate analyses, I controlled for government status.

In terms of the second "trigger" variable, I expected a relation between electoral context, measured by election timing, and the effect of party extremity. As we have seen, Oppenhuis et al. (1996) argued specifically that a surplus in votes for extreme parties should only be apparent when there is a large temporal distance between European and national elections. However, the analyses demonstrated that, while it is true that extreme party performance in European elections gradually improves with more time passing since national elections, their performance is still significantly worse than that of non-extreme parties. In short, extreme parties never experience any significant gains over non extreme parties throughout the NEC.

This is clearly problematic for MPO theory, as it somewhat undermines its basic proposition on marker-setting elections. It is less problematic for the validity of SOE theory though. While the theory makes no predictions on the effects of election timing on other parties than government parties, it does predict a late term recovery for government parties (see Chapter 1). Hence, that extreme parties (which are assumed to be in opposition) would not outperform mainstream parties (including those in government) when the European elections are held in the late term is consistent with SOE theory.

One potential explanation for the absence of an increase in vote shares of extreme parties (relative to non-extreme parties) in the late term of the NEC is connected to the previously suggested mechanism of 'protest through abstention.' In this regard, Weber suggested that

> "[s]omewhat paradoxically, abstention may also reflect voice. Citizens may withhold electoral support from a party to signal dissatisfaction. In elections, action and inaction are not mutually exclusive (cf. Ajambo 2007). Nonvoting does not necessarily imply

abandonment of political influence (as in the case of alienation), but may also be interpreted as purposeful behavior." (2011, 908)

Following this mechanism, some (core) voters would express their dissatisfaction by abstaining rather than defecting to another party in European elections. Consequentially, there is still a possibility that European elections held late in the NEC are protest events, as MPO theory suggests, but contrary to its prediction, voters protest voting through (strategic) abstention (*exit*) rather than "extreme" voting (*voice*).

Finally, it is necessary to emphasize that this empirical analysis has taken a very broad approach to analyzing the impact of party extremity on electoral performance in European elections. Consequently, the results must be understood in very broad terms as well. A different, narrower, approach could prove that certain types of extreme parties do indeed have an advantage in (European) SOE compared to (national) FOE. For instance, sticking with Zulianello's categorization, we could analyze the fates of anti-system and halfway house parties separately. The expected finding of this sort of analysis, still building on the basic SOE prediction, would be that anti-system parties are more extreme, and smaller, than halfway house parties, and thus should perform better in European elections.

Table 2.4: Vote Shares of Anti-system and Halfway House Parties in European and National Parliamentary Elections

	Anti-system	Halfway House
Mean Vote National	3.39	13.82
Mean Vote European	4.04	12.36
Difference (European – National)	.65	-1.46
p-value (two-tailed)	.001	.01
% Change	19.17%	-10.5%
No. of Observations	336	134

This suggestion finds support in the descriptive statistics (see Table 2.4) and provides a preliminary confirmation of Zulianello's key point that anti-system and halfway house parties are not the same, despite often being treated as such. Systemic integration is very visible to the public and thus will likely impact the competitive image of a party in the eyes of voters. Mechanisms that determine the electoral success of these parties could therefore differ significantly from one another. Consequentially, a more nuanced analysis, concentrating on differences within the 'extreme' party group, might be desirable in the future.

Conclusions

The main finding of the chapter is that the ideological extremity of a party does not provide any comparative advantage in European elections. This contradicts one of the key (later) propositions of SOE theory. It also adds to the skepticism about the validity of this prediction that is already presented in works concentrated on the performance of different kinds of extreme parties in European elections (Minkenberg and Perrineau 2007; Ivaldi 2012, Mudde 2019b). Unpacking the basic prediction further proved that if there is any effect of party extremity on electoral performance in European elections, it is conditional upon party size and election timing, although not in the way that SOE (and MPO) theory predicts. Small extreme parties have no particular advantage in (European) SOE and do not enjoy a significant support boost when (European) SOE are held at a considerable temporal distance from (national) FOE. In fact, the empirical analysis clearly shows that the *winners* of these elections are small non-extreme parties. Which non-extreme parties tend to profit the most, and when, is thus a good question for

future inquiries on electoral behavior in European elections. In my analysis, the left-right dimension had no significant effect.

Finally, in terms of voter motivations, the findings suggest one major implication: European elections are not dominated by protest voting (*with the boot*) at any point. In other words, if voters truly chose to defect from government and large parties in these elections, this defection is clearly not in the form of voting for parties that contest the metapolicies of a political system, which could indeed be seen as protest voting. This suggests that if there is a defection, it comes in the form of voting for parties that are ideologically closer to a voters' heart but are not anti-system (or "extreme"). In other words, this would mean that (most) sincere voting (*with the heart*) is not ideologically extreme.

CHAPTER 3

Exit: Turnout in European Elections

In the previous two chapters, I have concentrated on SOE theory's propositions on vote choice in

European elections. This chapter switches the focus to voter participation. In this regard, the

proposition of SOE theory is crucial for the suggested second-order nature of European elections.

The main aim of the chapter is to unpack the general SOE theory proposition that participation in

(European) SOE is lower than in (national) FOE. Borrowing again from Hirschman's famous

terminology, I am thus interested in determinants of the choice of some voters to "exit" in

European elections. At the aggregate level, this means that I aim to explain variation in turnout in

European elections.

We have seen that, in terms of turnout, SOE theory mainly proposes that participation in

(European) SOE will be low. This proposition is crucial for the theory, as it confirms its key

mechanism, the less-at-stake dimension. Indeed, Reif (1984, 246) suggested that turnout rates are

one of the key determinants of the election "hierarchy." The less-at-stake dimension has two

crucial consequences for participation in European elections: (1) fewer voters consider these

elections sufficiently important to cast ballots; and (2) top-level politicians, party activists, and

journalists attribute less significance to these elections, which is usually manifested by a subdued

campaign (Reif and Schmitt 1980, 9). Concerning the second point, Reif and Schmit went as far

as to state that some voters might not even know that elections are being held (Ibid.). There is

clearly a causal relationship between the two points: politicians do not think European elections are important as voters do not regard them as such, and vice-versa.

Unfortunately, it may be somewhat difficult to answer these questions without relying on individual-level analysis. Most importantly, we cannot truly confirm whether the reason for the general increase in abstention in European elections, compared to national elections, is caused by the less-at-stake dimension without asking voters whether they consider European elections less important. Unfortunately, this is difficult to do with such a large timespan and a fairly large number of elections to cover. Indeed, the existing studies on turnout in European elections that do rely on individual-level data usually cover only one or two elections (see e.g. Wessels and Franklin 2009; Söderlund et al. 2011; cf. Franklin and Hobolt 2011; Nonnemacher 2021). However, there are different methods that, with some obvious limitations, allow me to at least address these questions on the aggregate level. For instance, comparing the mean turnout in European and national elections will arguably give us some indication into whether the less-at-stake mechanism is correct.

Still, a simple comparison of means of voter participation between European and national elections, without taking into account important contextual factors, will hardly be useful to convincingly explain either the variation (if there is any) in turnout between cases and across time (see e.g. van der Eijk et al. 1996; Franklin et al. 1996; Hobolt 2015) or the underlying mechanisms. Consequentially, it is again necessary to meticulously discuss the impact of the proposed determinants of turnout in European elections, as well as of those that might have not been explicitly proposed in this particular context, but should logically apply. From an empirical standpoint, this is the main goal of this chapter.

The chapter is organized as follows. First, I analyze the theoretical propositions of SOE theory on turnout in European elections, from which I generate testable hypotheses. Second part then empirically tests these hypotheses. Third, I discuss the findings. Finally, I conclude with a summary of what we have learned and its theoretical implications.

Theoretical Analysis

It may seem that SOE theory does not expect much variation in turnout in European elections, given the less-at-stake dimension. However, this is not completely correct. Variation was not ignored by the theory and its authors provided some (limited) explanations for it. First, other than the fairly obvious predicted effect of compulsory voting, Reif and Schmitt (1980, 12-3) suggested that "the more distinct the electoral procedures, as compared to the national tradition, the lower the turnout," while recognizing that differences in electoral procedures will probably be rather low.

This is a somewhat surprising suggestion, given that European elections universally use proportional representation (PR), while several EU members use a majoritarian electoral system in their national elections. Most authors argue that PR boosts turnout (e.g. Powell 1986; Blais and Carty 1990; Franklin 1996; Karp and Banducci 1999; see also Cox 2015), so it seems counterintuitive to argue that turnout will be lower in European because of changes in electoral procedures if these changes are mainly from majoritarian to PR electoral systems.

Reif and Schmitt provided the following reasoning for this mechanism, building mostly upon the French case:

"Of course, the change of electoral system in France from two-ballot majority system to PR [proportional representation] would favour small parties and liberate all parties from the negative aspects of being forced into electoral alliances. The five percent hurdle, however, counteracts this, thus reserving the advantages of PR to big parties. The problems of reaching the quorum threshold is much more severe for small parties in the small member states and consequently discourages small parties in these countries, or, at least, puts pressure upon them to form alliances[18] . . . Also regionalization and personalization of PR by dividing the country into several constituencies and by permitting preference voting could increase turnout. If, however, these constituencies are new and unfamiliar to voters and party workers, and therefore bear little meaning for them, one must fear relatively lower turnout because political actors will have difficulties in identifying with these territorial entities, perceived as artificial. This holds for the first-past-the-post system as well" (1980, 13).

Second, the authors of the theory also subsequently argued that turnout in European elections will be affected by the newness of an election. For instance, Schmitt (1990a, 170) stated that "the more often these 'additional' [European] elections are held, the more people get used to it and the less participate" (see also Ibid. 1990b). Consequentially, we could also expect some variation in turnout in European elections across cases according to how many European elections have been held in a country. Specifically, we should observe relatively higher turnout in the first European elections ever held in a country. Building on these two propositions, based on different mechanisms affecting participation in European elections, I first evaluate the following hypotheses:

Second Order Elections Theory Hypotheses:

(1) Turnout in European elections will be lower in countries in which the electoral procedures of the European and the national election are different.

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[18] Basing this only on the French example is misleading. In the majority of the EU member states, the threshold in European elections is lower or similar as in national elections.

(2) Turnout in European elections will be higher in countries that hold these elections for the first time.

There are a couple of other mentions in SOE theory that can help us to further unpack the general proposition on participation in (European) SOE and generate more testable hypotheses. First, Reif and Schmitt (1980, 13) also argued that "[i]f European elections are held the same day as other national elections (e.g. local, regional, legislative) turnout is expected to be higher [relative to European elections held on a different day than national ones], even if differential abstention of those who do vote cannot be excluded." Consequentially, while I have excluded the cases in which European and national elections are held at the same time in the first two chapters, I will include them here.

The second mention is more complicated and somewhat left open for interpretation. Reif and Schmitt (1980, 18) suggested that voters should be more inclined to vote in (European) SOE in which "electoral participation in the 'new' arena has the aspect of a referendum." While it is not completely clear what they mean by this, one possible interpretation can be derived from the theory's emphasis on the role of popularity cycles. Following this logic, European elections should not have a referendum aspect, given post-electoral euphoria (see Reif 1984, 246; 1997, 117), when held shortly after national ones. On the contrary, they could be seen as a referendum at *the midterm* of the NEC, when voters start to evaluate government policies. Similarly, this should apply to European elections held long after the national one, given the "test election" aspect (see Reif and Schmitt 1980, 13). Consequentially, turnout should be higher in European elections held at *the midterm* and *late term*, yet lower in the *honeymoon* of the NEC.

Moreover, Reif (1984, 247) explicitly stated that "[t]urnout at SOE, regularly lower than FOE turnout, reflects the relevance and weight of different SOE, i.e. reflects how much there is

perceived to be at stake." In his "hierarchy" of elections suggestion, Reif clearly considers European SOE to be at the bottom of the ladder, even compared to other SOE (such as regional elections). Hence, his hypothesis is probably not meant to propose that stakes in European elections could change according to the specific electoral context. However, as the context of European elections is likely not static, as even SOE theory posits given its popularity cycle effects predictions, we can apply this hypothesis in a somewhat modified way. The mechanism is then similar to the effect of the referendum aspect. The stakes of the (European) SOE are higher when held at *midterm* and towards the end of the NEC, while lower when held at *honeymoon* the NEC. The higher the stakes, the higher turnout.

This interpretation of SOE theory is not very different, in its basic logic, from the mechanisms proposed by the authors of previously mentioned MPO theory. They are more explicit regarding the effects of the electoral context on turnout in European elections, however. Marsh and Franklin (1996) argued that turnout variation in European elections is caused by the spillover from the national context. Specifically, people tend to pay more attention to politics, and parties are making more efforts, when European elections are held shortly before expected national elections (Marsh and Franklin 1996, 18; see also van der Eijk et al. 1996; Franklin et al. 1996; Franklin 2007; van der Brug et al. 2007, 231, Weber 2007). This is consistent with the general idea of "marker-setting" elections (Oppenhuis et al. 1996). Logically, voters (as well as parties) should perceive "marker-setting" European election as more important than "throw-away" European elections, happening shortly after the national ones. Consequentially, turnout should be higher in European elections held in the *late term* but lower in the *honeymoon*.

The results of Marsh's and Franklin's analysis, while necessarily limited to a relatively small number of elections and countries, were largely supportive of this thesis. They found that

turnout is comparatively low at the start of the NEC and gets higher when national elections are coming closer (see also Franklin et al. 1996, 318). This was further confirmed by Franklin (2001) for the first five European elections and by Wessels and Franklin (2009) for the 2004 European elections. Finally, these findings are also consistent with the propositions on the timing effects on turnout in legislative elections (see Franklin 2004).

Finally, the effects of election timing on turnout are also consistent with the so-called "electoral fatigue" thesis. For example, studying electoral participation in the Netherlands, van Egmond et al. (1998, 286-7) argue that a high number of elections held shortly after one another may alienate voters and decrease the perceived importance of subsequent elections (see also Rallings et al. 2003; Nonnemacher 2021). Consequentially, this again suggests that European elections held shortly after national ones should have a lower turnout than European elections held a long time after national elections.

To sum up, both SOE and other theories seem to suggest that the stakes in some European elections will be higher than in others, depending on the particular context of these elections, that is given their position in the NEC. Consequentially, we can break down the general proposition that turnout will be lower in European relative to national elections into several sub-hypotheses.

Election Timing Hypotheses

(3) Turnout in European elections will be higher when they are held concurrently with national elections.

(4) Turnout in European elections will be lower when they are held at the beginning of the national electoral cycle.

(5) Turnout in European elections will be higher when they are held around the middle of the national electoral cycle.

(6) Turnout in European elections will be higher when they are held towards the end of the national electoral cycle.

Other than evaluating these hypotheses, I will also examine the effect of turnout rates in national elections (or "baseline" turnout) on turnout in European elections. This should be useful to evaluate whether another potential mechanism could apply. The key assumption here is that determinants of variation in turnout in national elections will impact turnout in European elections as well.[19] Consequentially, I expect that generally low/high turnout in national elections will lead to generally low/high turnout in European elections (see also Franklin and Hobolt, 2011; Wessels and Franklin, 2009).

Finally, there is one more possible relationship that can be added. The turnout rate in the previous national election could potentially affect the impact of election timing. Specifically, when voters generally participate at high rates, it is possible to assume that electoral context will have a lesser impact on them than on voters who generally participate at lesser rates, and thus will likely give more weight to the importance of a given election. In other words, while electoral context changes the stakes of European elections, in some cases this may not have such a visible effect on participation as voters generally tend to participate more. Consequentially, the effects of election timing on turnout in European elections might be conditional upon the turnout rates in

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[19] There are multiple explanations for generally high or low turnout: enforced compulsory voting (e.g. Panagopoulos 2008; Singh 2011); trust in democracy (e.g. Downs 1957); satisfaction with democracy (e.g. Franklin 2004; Karp and Banducci 2008; Hobolt 2012; Ezrow and Xezonakis 2016); state capacity (e.g. Kasara and Suryanarayan 2015); socio-economic factors (e.g. Brady et al. 1995, Blais and Dobrzynska 1998); political institutions and electoral laws (e.g. Jackman 1987); party systems and ideology (e.g. Crepaz 1990); or social rewards and sanctions associated with voting (e.g. Gerber et al. 2014).

the previous national elections. Combining these two insights, I generate two additional

hypotheses:

Baseline Turnout Hypothesis:

(7) Turnout in European elections will be higher in countries where turnout in previous national election was higher.

Election Timing and Baseline Turnout Hypothesis:

(8) The higher turnout in the previous national election, the lower the effects of election timing.

Testing this series of hypotheses should also help us evaluate the thesis on strategic

protest/defection by "exit" rather than by "voice." In short, the possibility that some voters chose

to protest in European elections by abstaining rather than by vote switching. If (some) voters do

chose to protest through abstention, they should be more tempted to do so when European

elections matters more (i.e., the stakes are higher), i.e., in elections that have a "referendum

aspect" (Reif and Schmitt 1980, 16) or are "marker-setting" (Marsh and Franklin 1996;

Oppenhuis et al. 1996). Consequentially, at the aggregate level, we should not observe much of

an increase in turnout in European elections held in the midterm or late term.

This would not necessarily mean that (some) voters do not choose to *protest-by-voice*, but

rather that the increase in turnout these protest voters provide in these elections is leveled off by

voters who normally vote but, specifically in these contests, chose to *protest-by-exit*. Lack of

support for hypotheses 5 and 6 would be consistent with such a pattern, although do not provide

direct evidence for them (see Rainey 2014). More testing, notably with individual-level data, would still be needed.

Empirical Analysis

Data and Variables

The data for the analyses in this chapter were again mainly collected by using various available electoral databases (e.g. Döring and Manow 2020) and official country-level sources, such as the websites of the ministries of interior. The dependent variable for all hypotheses is the turnout rate in European elections (*TurnoutEE*), measured as the percentage of the electorate that voted in the European election. This is thus a differently constructed dependent variable than I have used in the previous two chapters, where we looked at the *difference* between outcomes in national and European elections. I construct the dependent variable this way, as we are mainly interested in variation in turnout rates in European elections here, rather than the change in rates between the two types of election.

The first independent variable (*ΔElectSystem*) is used to evaluate the first hypothesis. The variable is coded as 1 when there is a change in the electoral system between national and European elections and 0 when the election method is similar. Other, continuous rather than binary, variables could be included to measure these effects, but their use is rather problematic in this setting. For instance, the difference in the effective number of electoral parties (ENP) between national and European elections (see Laakso and Taagepera 1979) might be an attractive option. However, while ENP is correlated with the type of electoral system employed,

it is also affected by a plethora of other issues related to specific party systems in EU countries. Therefore, the change in ENP is not a fully adequate measure of the change of electoral procedures. Gallagher's index that measures disproportionality between the vote distribution and the seat distribution (see Gallagher 1991) could also be potentially used. However, only a short glance at the index values shows that they are always higher for European elections. This is probably given the smaller numbers of seats reserved for different country members in the European Parliament. This measure would thus not adequately capture electoral procedures. Consequentially, the dummy variable seems to be our best option to test hypothesis 1.

The second independent variable (*First Election*) captures the effects of the newness of European elections (hypothesis 2). The variable is coded as 1 when the European election was held for the first time in a country and 0 for following European elections. Third, the previously discussed quadratic versions of the *Cycle* variable, capturing the location of the European election in the NEC ($Cycle^2$), and the *TimeSinceNat* variable, measured as the number of days passed since the previous national election ($TimeSinceNat^2$) are used to evaluate the *Election Timing* hypotheses 3 to 6. The last independent variable, used to evaluate hypothesis 7, is the turnout rate in the previous national elections (*TurnoutNat*).

In terms of control variables, I include the two indicators for economic performance again, namely changes in unemployment rates and GDP growth ($\Delta Unempl$ and $\Delta GDPGrowth$). The reason to include these variables here is that levels of wealth of electorates, which these variables should capture, can have an effect on the dependent variable and at the same time confound the effects of the explanatory variables. For instance, if the economic situation in a country worsens between national and European elections, more voters might decide to participate in the European election to signal their discontent. Consequentially, an increase in

turnout in this sort of election would be potentially triggered by the worsening economic

performance of a country rather than anything else. Finally, I also control for the impact of levels

of electoral democracy at the time of election. Lower levels of electoral democracy could

potentially depress turnout, and thus affect my dependent variable, while logically confound the

effects of my independent variables as well. To construct this control (*Electoral Democracy

Index*), I rely on the Varieties of Democracy (V-Dem) Project's (Coppedge et al. 2021a;

Pernstein et al. 2021) "Electoral democracy index" (see Coppedge et al. 2021b, 43).

Statistical Tests

The analyses in the chapter are structured as follows. First, I compare the mean turnout rate in

European and national elections for all sets of elections from 1979 to 2019, to confirm whether

participation in European elections is truly lower. Except for the average turnout rates, the

difference between them, and the percentage changes, I also include p-values (two-tailed) to

demonstrate the statistical significance of these differences. In addition, I compare average

turnout rates in each election separately, to evaluate whether there are any observable trends over

time. This could particularly help us to see whether the EU enlargements had any overall effects

on turnout, and thus provide an initial evaluation of hypothesis 2.

Second, I run several ordinary least squares (OLS) models with clustered standard errors

by country. Different from the previous analyses, some models here include the cases in which

European and national elections are held on the same day. This is because turnout between these

elections can still theoretically differ when they are held concurrently. However, the inclusion of

these cases can also lead to misleading results in terms of the effects of some independent

variables here. For instance, the potential effect of the election timing on turnout in European

elections could be solely driven by the fact that turnout is exceptionally high when elections are

held on the same day, while significantly lower in comparison when held later in the NEC.

Consequentially, to test the *Election Timing* hypotheses, I construct models with and without

same day elections. Additionally, in analyses that include the same-day election cases, I use

various post-estimation tools (*DFIT, DFBETA*) to identify whether these cases are truly the most

influential observations. These models differ from one another in the measurement of election

timing.

Findings

First, Table 3.1 shows that, as expected, there is a significant difference in turnout between

European and national elections. On average, from 1979 to 2019, turnout in European elections

was 22 points lower than in previous national elections (a relative drop of 30%). The difference

is statistically significant at conventional levels (*p-value* = < .001, two-sided). The key initial

proposition of the SOE theory is thus confirmed.

Table 3.1: Turnout in European and Previous National Elections 1979 – 2019

	Nat	EE	Difference (EE – Nat)	% Change	p-value (two-sided)
Mean Turnout	73.2	51.2	-22	-30.1%	.001
No. of Observations = 166					

EE = European Elections, Nat = National Elections

Second, plotting the average turnout in all European and previous national elections, Figure 3.1 demonstrates that participation has been getting lower in every subsequent election since the first European elections in 1979. This changed, however, in the 2019 European elections, which saw a higher turnout than in 2014, almost at the 1999 level. We can further see that turnout in national elections, while clearly higher than in European ones, has declined over time as well, following almost a similar pattern. Indeed, the difference in participation between European and national elections does not change much over time until the 2019 election.
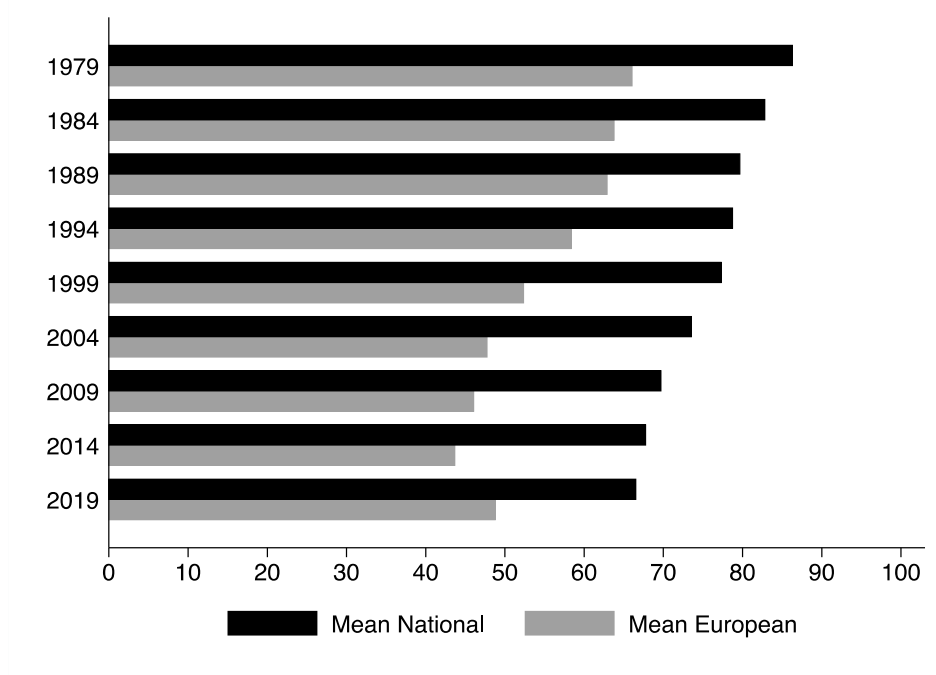
Figure 3.1: The Difference in Turnout in National and European elections over Time

The results of the regression analyses are reported in Table 3.2. First, the analyses in all models demonstrate that changes in the electoral system have no significant effect on turnout in

European elections, hence disconfirming hypothesis 1. Second, whether the European election was held for the first time does have a significant impact on turnout in European elections. However, the negative coefficient of the *First Election* variable signals that turnout was comparatively lower in European elections held for the first time. The newness effect is thus in the opposite direction, which means that hypothesis 2 is not supported either. Third, turnout in previous national elections strongly affects turnout in European elections. The variable is statistically significant across all models. Its positive coefficient means that when turnout is higher in the previous national election, it tends to be higher in the following European election (see also Figure 3.2). Moreover, the strong performance of the variable in models 1a, 2a, 3a, and 4a indicates that the effect does not decrease when we exclude same-day elections, which logically would have very similar turnout rates.

Table 3.2: The Determinants of Turnout in European Elections

	Model 1	Model 2	Model 1a	Model 2a	Model 3	Model 4	Model 3a	Model 4a
ΔElectSystem	.010	-1.023	1.601	.348	.759	-.179	1.580	.618
	(5.136)	(5.011)	(5.109)	(5.055)	(4.836)	(5.111)	(5.011)	(5.200)
First Election	-4.357**	-4.652**	-3.370*	-4.037*	-3.663	-4.115*	-3.319	-3.750*
	(2.172)	(2.307)	(1.915)	(2.020)	(2.442)	(2.391)	(2.103)	(2.002)
TurnoutNat	1.070***	1.162***	0.994***	1.096***	1.963***	1.822***	1.503***	1.277***
	(.166)	(.175)	(.170)	(.181)	(.210)	(.305)	(.219)	(.370)
TimeSinceNat	-.025**		.001		.170***		.087*	
	(.011)		(.007)		(.050)		(.050)	
TimeSinceNat^2	.000**		.000		-.000**		-.000	
	(.000)		(.000)		(.000)		(.000)	
Cycle		-43.87**		4.463		147.5		10.81
		(18.00)		(20.16)		(113.7)		(137.3)
Cycle^2		37.95**		-2.233		-89.79		29.81
		(18.03)		(20.80)		(112.3)		(134.1)
TurnoutNat#TimeSinceNat					-.002***		-.001	
					(.001)		(.001)	
TurnoutNat#TimeSinceNat^2					.000 ***		.000	
					(.000)		(.000)	

|								
TurnoutNat# Cycle						-2.346		-.084
						(1.412)		(1.732)
TurnoutNat# Cycle²						1.522		-.445
						(1.381)		(1.672)
ΔUnempl	.126	.274	.190	.223	.261	.369	.289	.375
	(.442)	(.508)	(.415)	(.481)	(.423)	(.572)	(.421)	(.577)
ΔGDPGrowth	.184	.137	.193	.149	.117	.105	.163	.150
	(.244)	(.264)	(.227)	(.251)	(.238)	(.267)	(.231)	(.265)
Electoral Democracy Index	-45.38	-59.61*	-47.63	-64.97**	-49.71*	-64.16**	-48.91*	-59.65*
	(31.58)	(31.12)	(29.93)	(29.27)	(27.94)	(30.95)	(28.31)	(32.21)
Constant	20.10	27.16	17.12	23.82	-47.79	-22.88	-19.60	5.559
	(27.38)	(26.59)	(24.65)	(22.54)	(30.71)	(34.97)	(30.62)	(34.78)
Observations	166	148	155	134	166	148	155	134
R-squared	.566	.611	.499	.540	.617	.638	.515	.550
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Fourth, the findings were rather unconvincing in terms of the effects of election timing. First, the effects of election timing seem to be clearly driven by the cases in which European and national elections are held on the same day (see models 1 and 2). If we drop these cases, both variables measuring election timing fail to reach statistical significance in their impact on turnout in European elections (see models 1a and 2a). Indeed, the post-estimation tests (*DFIT, DFBETA*) of the regression analyses in the models with all elections convincingly demonstrate that same-day election cases are the most influential observations in these analyses. Finally, comparing the association between election timing and turnout in European elections with a fitted quadratic line and confidence levels in models 1 and 1a (Figure 3.3), shows that the effects of election timing are driven by a cluster of cases in which the European and national election were held concurrently.
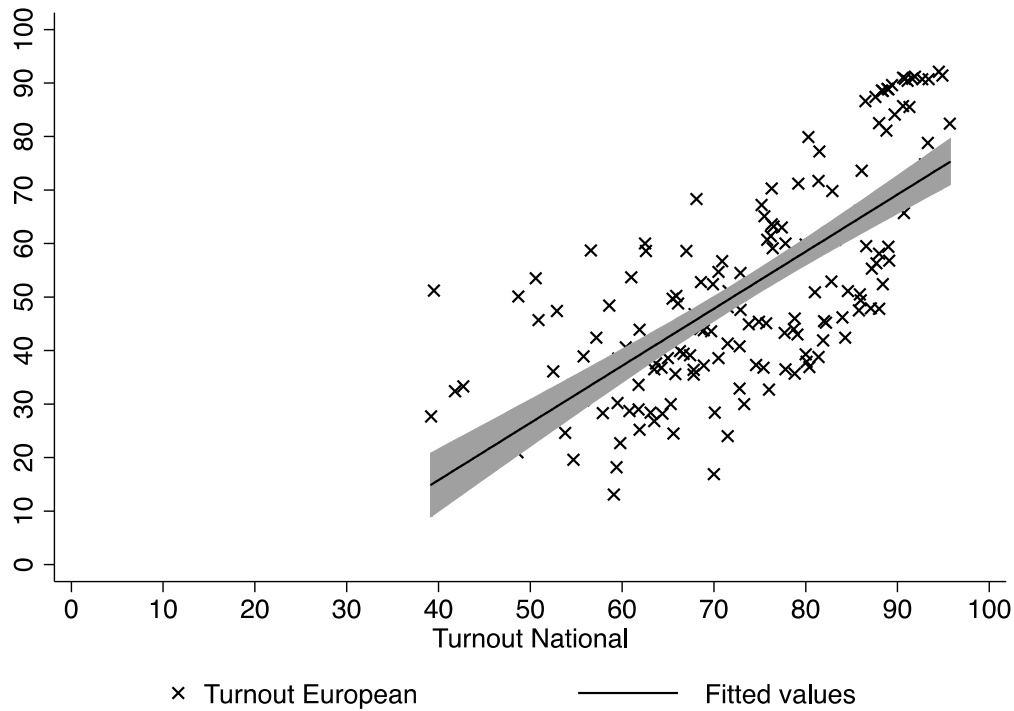
Figure 3.2: The Association Between Turnout in National Elections and Turnout in European Elections (95% Cls)

The same figure also shows that European elections held towards the very end of the

NEC have only a very marginal increase in turnout in model 1. When we exclude the same-day

elections (model 1a), the effect disappears altogether. Moreover, the dispersion in turnout across

the NEC is rather high, suggesting that there is no clear effect of election timing on turnout. This

finding is independent upon the specific measurement of election timing. Consequentially,

hypotheses 4 to 6 are not supported: we observe neither lower turnout shortly after the previous

national election nor an increase in turnout with the increase in temporal distance between

elections. On the other hand, we do observe an increase in turnout in European elections held

concurrently with national elections, and thus find support for hypothesis 3.
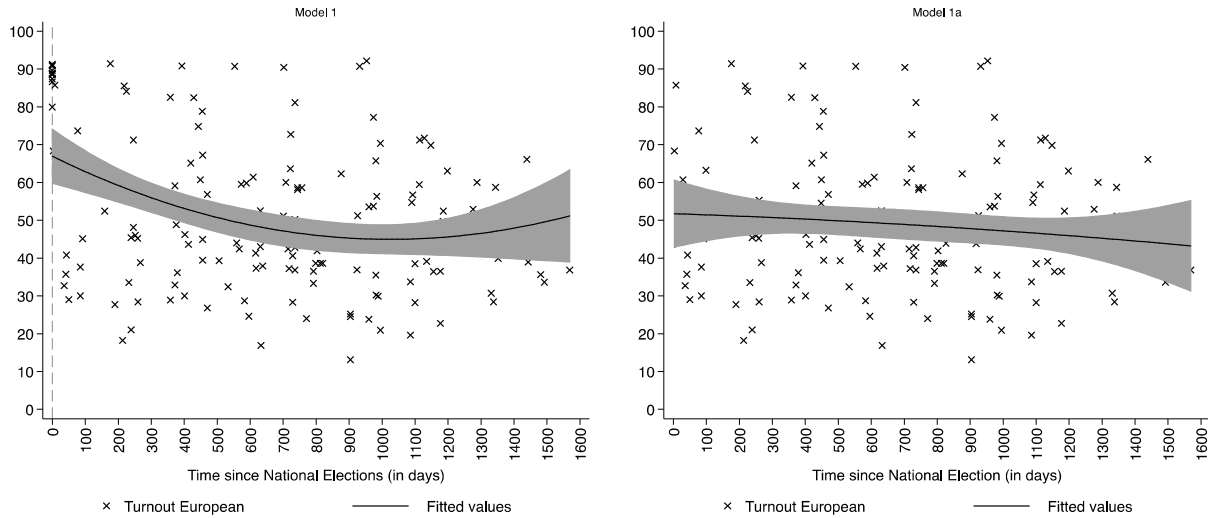
Figure 3.3: The Association Between the Days Elapsed since National Elections and Turnout in European Elections (95% Cls)

Finally, Figure 3.4 shows that the effects of election timing on turnout in European elections are indeed conditional upon the "baseline" turnout, however, not in the way that we expected. Plotting the analysis in model 3 reveals that election timing has a significant impact on turnout in European elections when participation in previous national elections was rather low (below 60%). This would mean support for hypothesis 8, given that it indicates that turnout in European elections is indeed significantly affected by election timing in countries with low turnout in national elections. However, as we can see, election timing also significantly impacts turnout in European elections when participation in national elections is quite high (over 75%), which goes against hypothesis 8.

Figure 3.4a shows that when we use the *Cycle²* variable to measure election timing (Model 4), there is no effect when turnout in national elections was low but a significant effect when it was high, thus rejecting hypothesis 8 even stronger. The patterns are similar when we drop the cases in which elections are held concurrently (models 3a and 4a). The effect of election

timing only becomes smaller when we use the *TimeSinceNat²* variable to measure election timing (model 3a) and not significant with the *Cycle²* variable. Consequentially, we can conclude that the analyses have not shown enough support for hypothesis 8.
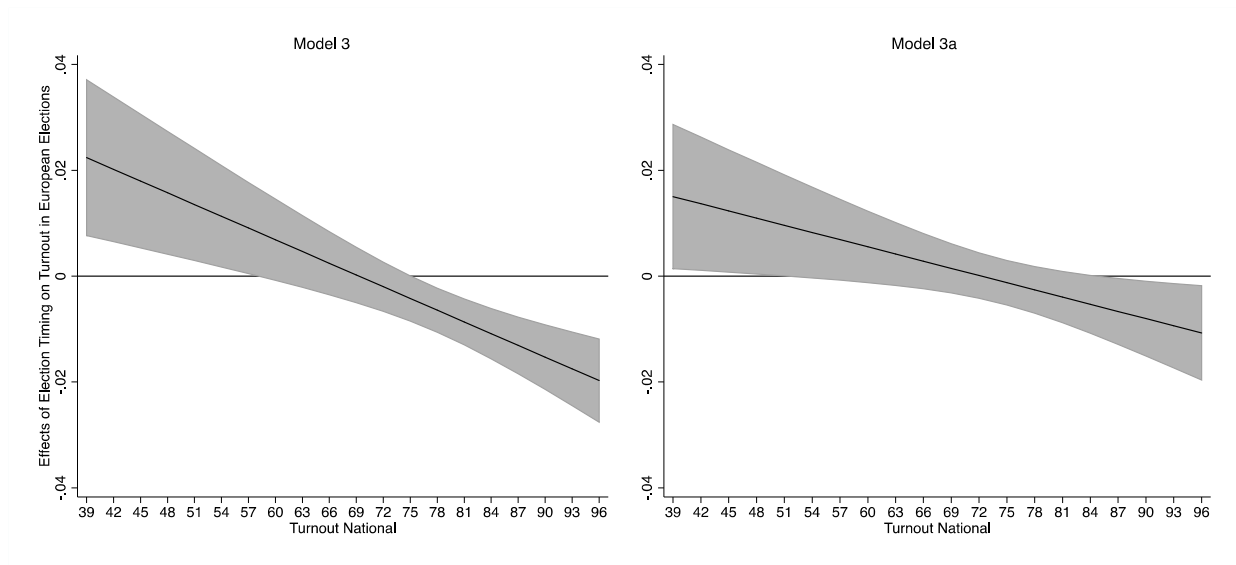
Figure 3.4: Average Marginal Effects of the Days Elapsed since National Elections (95% Cls)
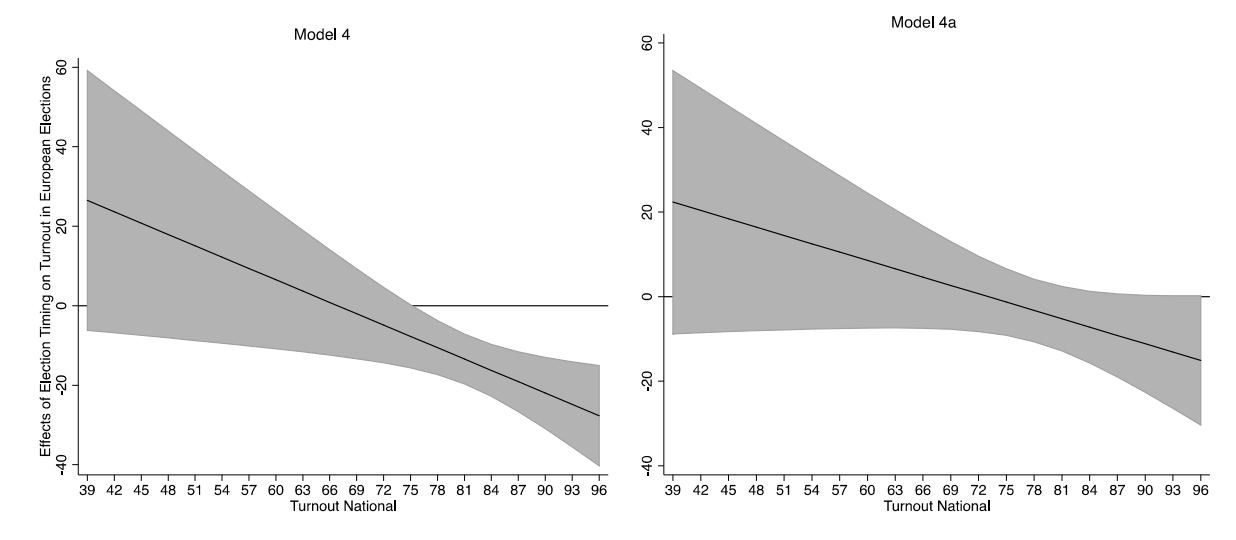
Figure 3.4a: Average Marginal Effects of the Position of European Election in the NEC (95% Cls)

Discussion

The analyses in this chapter have demonstrated that turnout in European elections is significantly lower than in national ones, thus confirming the key proposition of SOE theory. This suggests that, in general, there seems to be indeed less-at-stake in European elections and justifies their classification as SOE. While this was expected, the main aim of the chapter was to explore whether there is any variation in turnout across cases and, if so, what explains it. We have seen that the theory itself allows for this possibility, making short (and often a bit vague) mentions on the effects of changes in electoral procedures, the newness of an election, and (indirectly) election context (election timing). These suggestions were then complemented by more explicit mechanisms, mostly provided by MPO theory, and other general theories on electoral behavior.

First, we have seen that Reif and Schmitt (1980) suggested that change in electoral procedures between national and European elections should negatively affect turnout in the latter (hypothesis 1). The analyses do not provide any support for this hypothesis. Change in the electoral system does not account for variation in turnout in European elections, when we control for other determinants of turnout. This finding is not overly surprising though. As I have suggested, we could easily imagine the relationship going the other direction as well, given that proportional representation (PR) is often argued to boost turnout (which is, however, not the case either) and Reif and Schmitt themselves were somewhat hesitant when making the proposition.[20]

[20] See Reif and Schmitt (1980, 12-13): "As electoral procedure regulations for the European elections were determined by national legislators, and most have retained the main features of the system used nationally, the impact of procedural differences may be expected to be low. Nevertheless we assume: the more distinct the electoral procedures were, as compared to the national tradition, the lower the turnout".

Important for our debate, this non-effect could mean that there is not much variation in turnout in European elections, thus supporting the initial basic SOE proposition and further reinforcing the validity of the less-at-stake dimension.

Second, average turnout in European elections has declined from 1979 to 2019. At the first glance, this seems to confirm the newness effect (hypothesis 2). The gradual decline in turnout in European elections may indeed signal that voters participate less once the newness of the first European elections washes away and more such elections are held. However, this observation might not be completely correct. When we limit the analysis to the nine country members that held all European elections since 1979 (see Figure 3.5), the trend is rather inconclusive and there is no clear evidence of declining participation election-by-election. When we look at different waves of enlargement, there is no clear trend either (see Figure 3.6). Most importantly, this has been confirmed in the multivariate analyses. Controlling for other determinants, turnout was actually lower in the first-time European elections than in later European elections. Consequentially, support for hypothesis 2 is rather limited, when looking more carefully at the available data and testing the newness effect with multivariate analysis.

Still, changes in average turnout over time showed some potentially interesting developments. The slight growth in turnout in the 2019 election is surprising. While it is too early to tell, this may signal a possible change in the context of European elections. Particularly, it could mean the long-anticipated increase in salience of European elections, which could potentially change the second-order nature of these elections (see e.g. Carrubba and Timpone 2005; Hobolt et al. 2008; Clark and Rohrschneider 2009; de Vries et al. 2011; Hobolt and Wittrock 2011; Hobolt and Spoon 2012; Hobolt 2015; Hobolt and de Vries 2016; Hernández and Kriesi 2016). Future research will have to tell.
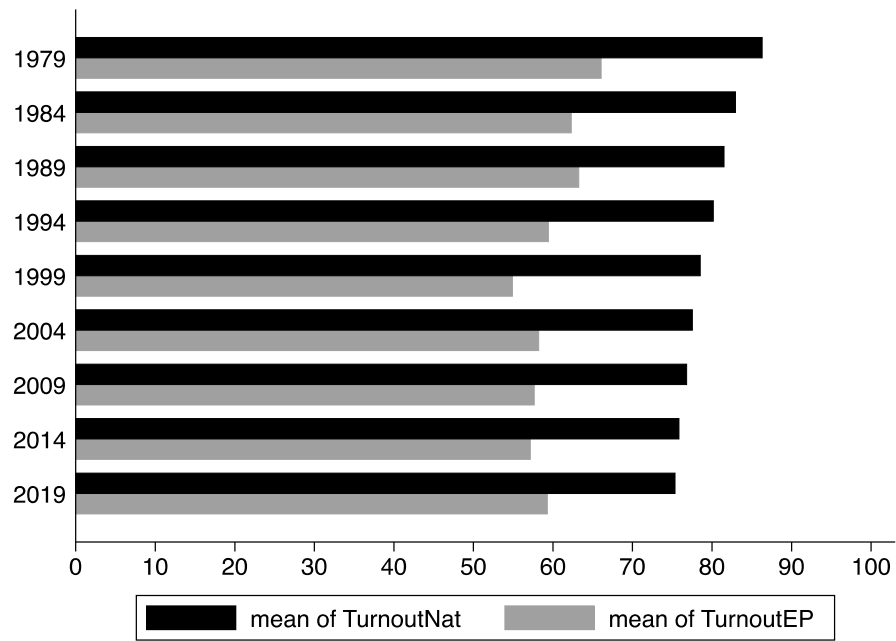
Figure 3.5: Change in Average Turnout in National and European Elections for the Nine Original Members
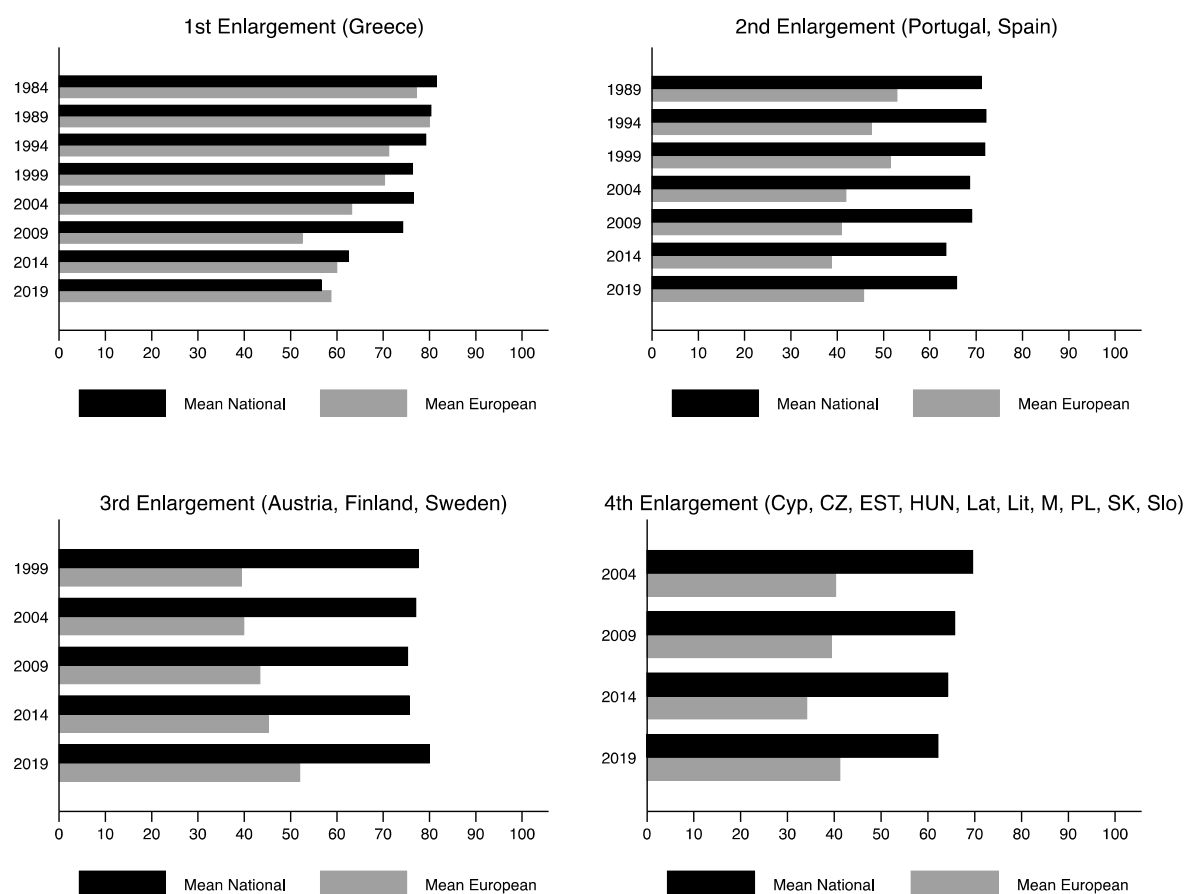
Figure 3.6: Change in Average Turnout in National and European Elections According to Enlargement Waves

We have also seen that the difference in turnout between national and European elections has not changed much over time, given that turnout gradually declined in national elections as well. Hence, the declining turnout since the first European elections (until 2014) is probably a product of the overall declining voter participation in (enlarged) Europe. This is indeed supported by the confirmed strong impact of turnout in national elections on turnout in European elections (see below). To claim that the importance of European elections declined rather than increased over time may thus be misleading (cf. Franklin and Hobolt 2011). Other processes,

such as an overall decrease in interest in politics between 1979 to 2014 or the EU enlargements to countries with low turnout (see e.g. Franklin 2001), may better explain the trend in participation.

Third, the effect of turnout in previous national elections was significant and positive across all models. In short, when voters generally vote at higher rates in national elections, they will also vote at higher rates in European elections. This is likely caused by different mechanisms that, most likely, have little to do with European (or second-order) elections. Related to this is the suggestion that, in this sense, elections are not that much different from one another and voters base their decision to participate in European elections on similar cues as they do for national elections (cf. Schmitt and Mannheiemer 1991; Franklin and Hobolt 2011).

But this finding might also be solely caused by the predicted effects of (enforced) compulsory voting (see Reif and Schmitt 1980, 10). Indeed, in elections where voting is compulsory (with some form of enforcement), the average difference in turnout between European and national elections is only -5.2% (see Appendix 3.1) and purely motivated by one outlier, Cyprus.[21] Still, when I exclude these cases, the effect of turnout in previous elections remains significant. Similarly, including a compulsory voting dummy variable as a control[22] does not impact the effects of turnout in previous national elections, not even when I drop the case of Cyprus.

Fourth, election timing has only limited effects on turnout in European elections. Voters

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[21] When I exclude Cyprus and limit the cases to only Belgium and Luxembourg, the difference is only -1.3%.
[22] The variable is coded as 1=compulsory voting, 0=non-compulsory voting. I differentiate between enforced and unenforced compulsory voting, relying on the most recent data on the subject (see e.g. Singh, forthcoming), as well as the Varieties of Democracy (V-Dem) Project (Coppedge et al. 2020a; Pemstein et al. 2020) data that go more back in time. Unenforced compulsory voting is coded as 0: to be coded as 1, the enforcement level needs to be at least "medium" according to Singh's scale or corresponding to the score of 2 in the V-Dem data. I also take into account the cases which used to adopt enforced compulsory voting but ceased to do so at some point between 1979 and 2019 (i.e., Cyprus). The list of cases of (enforced) compulsory voting included can be found in Appendix 3.1.

participate at significantly higher rates only when European and national elections are held on the same day (hypothesis 3). There is, however, no clear pattern in variation in turnout across the NEC (hypotheses 4 to 6). We can interpret these non-effects again as providing fairly strong support for the basic logic of SOE theory. The stakes in these elections seem to be simply always low. However, we have also seen that SOE theory made several implicit suggestions that are not supported by the analyses; for instance, the suggested impact of the "referendum" aspect. As is often the case with SOE theory, the interpretation of the results, in this regard, depends mostly on how closely we pay attention to all of its propositions.

On the other hand, the results are clearly problematic for other theories, especially MPO theory. The analyses provide hardly any evidence that turnout in European election changes according to the temporal distance from the national election. Specifically, the suggestion that European elections held in long temporal distance from the previous national ones should be perceived as more important by voters, and thus experience higher turnout (e.g. Marsh and Franklin 1996), is not supported. The analyses convincingly demonstrate that if there is an increase in turnout in European elections towards the end of the NEC, it is purely driven by the other election timing effects, more specifically same-day elections.

These results also disconfirm findings of other literature on voter participation in European (and other) elections that have been making similar arguments as MPO theory (e.g. Wessels and Franklin 2009; Franklin 2004). While these studies mainly measure election timing as the time until (rather than since) the next national election, the cyclical variable captures the effect of temporal distance from the following national elections. We have seen that the results of the analyses using the quadratic form of this variable ($Cycle^2$) are even weaker, in terms of the effects of election timing than those using the quadratic form of the *TimeSinceNat* variable.

Moreover, additional analysis (not reported), in which I used the time until next national elections (in days) as a measure of election timing, have not shown any significant effect of this variable on turnout in European elections.

However, I have also suggested that it might be exactly because of the strong impact of turnout in the previous national election that we do not find strong independent effects of election timing on participation in European elections (hypothesis 8). Consequentially, I have also tested the relationship between election timing and "baseline" turnout. The results of these analyses demonstrate that turnout in previous national elections has some impact on the magnitude of effects of election timing on turnout in European elections but it is not the effect that we would logically expect to find. Specifically, we have seen that election timing significantly impacts turnout in European elections when participation in national elections is quite high, which directly contradicted our initial prediction. Similar to previous findings, this conclusion is rather supportive of the impact of the less-at-stake dimension, and thus the basic logic of SOE theory.

Finally, there is another possible explanation for the weak effect of election timing on turnout in European elections, which is connected to the previously mentioned thesis of strategic defection through abstention, or "voice" through "exit" (Weber 2011; see also Marsh 1998, Schmitt and van der Eijk 2007). In short, it is possible that because the "marker-setting" election held late in the NEC is more important for political parties (the stakes are truly higher), some "angry" and "frustrated" voters (cf. Oppenhuis et al. 1996, 304) of these parties chose to withdraw their regular support by abstaining rather than vote switching to show their discontent. While this is highly speculative and, as pointed out at the beginning, rejection of hypotheses 5 and 6 merely suggests the possibility of this pattern while not directly testing it, these results can

be seen as a first step and a motivation for further, more in-depth, individual-level analyses (cf. Weber 2011).

Conclusions

The findings of the analyses in this chapter again added additional pieces to our larger puzzle. Mainly, it was confirmed that turnout in European elections is indeed lower than in the previous national elections. Even after unpacking this general proposition of SOE theory, the plausibility of the less-at-stake thesis, which is the key in explaining low turnout in (European) SOE, was largely confirmed. In short, European elections continue to be perceived as less important than national parliamentary elections. There is almost no evidence that the stakes of European elections change according to their position in the NEC, and thus election context. In other words, the specific timing of European elections in terms of the NEC does not motivate voters to participate more in those elections. The only time that election timing clearly affects turnout in European elections is when these are held on the same day as national elections. This is arguably the strongest reason to continue to consider these elections SOE. Voters also tend to participate more in European elections if turnout was already high in previous national elections. This can mean multiple things but, mainly, it seems that processes that determine turnout in national elections have an impact on turnout in European elections as well. To a large extent, this can still be interpreted as confirmation of the basic logic of SOE theory, according to which, European elections are low-stimulus national election.

Finally, unpacking the general proposition of SOE theory on turnout in European elections still demonstrated at least one interesting point, which suggests a potential deviation

from SOE logic. Turnout in the 2019 European elections was higher than in previous European elections, for the first time since 1979. This can have different explanations, some of which I have already mentioned, and should be further explored. It will be particularly interesting to see if this is the start of a new trend and if the nature of European elections is truly changing. If that is the case, this could affect the processes described in this and previous chapters and SOE theory as a whole.

CHAPTER 4

Conclusion

The general aim of this study was the theoretical elaboration and rigorous empirical testing of

one of the key theories of European comparative behavior, Second Order Election theory, first

proposed by Reif and Schmitt in 1980. This task consisted not only of testing the theory's key

hypotheses on the basis of a bigger number of elections and by applying previously not used

methods, but also to explore a plethora of mechanisms that the theory provides, as well as its

following elaborations in existing literature, and thus unpack the theory further. This exercise is

particularly important given that several of these mechanisms were never truly (or sufficiently)

assessed, either theoretically or empirically. Moreover, some are somewhat contradictory to the

basic SOE logic. In fact, few authors have looked at how many different propositions (the

authors of) SOE theory makes, let alone at how some propositions present challenges for the

general logic of SOE theory.

        After only a few comprehensive evaluations (van der Eijk and Franklin 1996; Marsh

1998; Hix and Marsh 2007; 2011), SOE theory has become sort of received wisdom in the field

of European comparative electoral behavior, something that is usually just mentioned in passing

in academic articles, a truth without a need for empirical justification. Because of the remarkable

performance of the general propositions of the theory, at least based on averages in vote shares

and turnout, it is not that surprising that almost nothing has been written in terms of the general

assessment of the state of the theory in the last two decades. But especially given the broadness of the theory, and the issues it presents, this is problematic.

What especially motivated me, in this sense, are the transformations of European politics in the last two or three decades. As pillar parties evolved in several West European democracies, many have lost their dominant position in the party systems. On the other hand, parties that were long considered "niche" have become more important players, often being among the strongest parties in the systems, and sometimes even entering national governments coalitions, and not solely as junior partners. In short, political mainstream became more radicalized (Mudde 2019a). These changes can be hardly ignored. It is logical to expect that they will have a strong impact on numerous propositions of such a broad theory on political behavior that was written some forty years ago. Specifically, it is hard to imagine that, in light of these changes, the SOE mechanisms of punishment of government parties and support boosts for small extreme parties in European elections will still work the same way.

My research strives therefore to not only evaluate the various processes suggested by this grand theory but also to, at least partially, contribute to the understanding of the more general impact of changes in European politics on political parties and electoral behavior. I strongly believe that European elections can serve as a laboratory for studying wider phenomena in European politics (cf. van der Eijk et al. 1996) and hope that this research has shown ways in which this is the case (as I will also discuss in more detail below).

To sum up, the general contribution of this study should be three-fold: (1) it examines one of the grand theories of European comparative electoral behavior on the basis of a larger and updated dataset and by using innovative methods and measurements; (2) it unpacks Second Order Elections theory and tests many of the suggested mechanisms of vote choice and electoral

participation in European elections, thus creating a more detailed and elaborate version of the theory; (3) it contributes to the debate on the impact of transformations in European politics on electoral behavior and political parties and has several implications for other general theories in these fields.

In the rest of the chapter, I will review what we have learned in terms of these contributions. First, I shortly address the state of the theory with a quick recapitulation of the main lessons from each of the three empirical chapters that unpacked the theory's general propositions. Second, I connect these findings and assess the general picture in terms of voter motivations in European elections. Third, I suggest more general implications of this research, especially with regard to the transformations of European politics. Finally, the last section concludes while also proposing some ideas for future research in the field.

State of Second Order Elections Theory and its Unpacking

I have started this thesis with an evaluation of the question of when and why some voters, in Hirschman's (1970) terms, remain "loyal" to governing parties in European elections (*loyalty*). Consequentially, addressing the key hypothesis of SOE theory, which holds that parties in government lose votes in European elections, as well as that the magnitude of their losses is conditioned by the timing of the European elections in terms of the national election cycle (Reif and Schmitt 1980; Reif 1984, 1997; Schmitt et al. 2020). Unpacking this general proposition provided me with more testable hypotheses, the evaluation of which add new pieces to the larger puzzle of electoral behavior in European elections. Other than the main emphasis on election timing and voter motivations, I also introduced variables and theories that are not typically used

in the literature on the topic, such as coalition composition and clarity of responsibility theory (e.g. Lewis-Beck 1988; Powell and Whitten 1993; Whitten and Palmer 1999; Anderson 2000; Powell 2000; Nadeau et al. 2002; Duch and Stevenson 2005, 2008; Fischer and Hobolt 2010; Hobolt et al. 2013).

The analyses in this thesis generally demonstrated that the government status of a party affects its performance in European elections. In line with the key proposition of SOE theory, on average, government parties lose more votes in European elections than other parties. However, multivariate analyses showed that these losses are conditional upon election timing. The main finding, in these terms, is that, relative to opposition parties, government parties experience gains in European elections held shortly after national, i.e., during the so-called *honeymoon* period. In elections after this short period, the losses become statistically significant and are only very slightly moderated towards the end of the NEC. The lowest point in government performance is then, on average, shortly after the *midterm* of the NEC.

Moreover, four other variables have a moderating effect on the impact of government status on party performance in European elections too: party size, the position of a party within the government coalition, turnout differential, and change in unemployment rates. First, smaller government parties are punished less than large government parties, to the extent that (very) small parties do not experience any significant losses in these elections. Second, when turnout is particularly low, compared to previous national elections, government parties are significantly more disadvantaged in European elections than other parties. Third, an increase in unemployment hurt government parties in European elections, but only when these elections are held around the *midterm* of the NEC. Fourth, and finally, the analyses show that the *clarity of responsibility* theory applies strongly in European elections. Parties that nominate the prime

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minister (i.e. senior parties) experienced bigger losses in European elections than other government coalition members (i.e. junior parties) as well as other (opposition) parties. Put together, these results thus show that SOE theory's general statement on government party losses is technically incorrect. Government parties are not always the clear *losers* of European elections and some can even be *winners*.

In the second empirical chapter, I investigated who the *winners* of (European) SOE are (cf. van der Eijk and Franklin 1996; van der Eijk et al. 1996). I concentrated in particular on the proposition that extreme parties should have a comparative advantage in European elections (Reif 1984, 1997; Oppenhuis et al. 1996; Schmitt et al. 2020), which so far has received only scant attention in scholarly research. The emphasis was thus on protest voting, i.e. raising the *voice* in Hirschman's framework. I also strived to offer solutions to several categorization, conceptualization, and measurement problems that are related to this specific thesis. A recently developed classification scheme of "anti-system" parties (Zulianello 2018; 2019a) allowed me to categorize a broad range of parties as 'extreme' for all European and national elections between 1979 and 2019 and to evaluate their performance.

The analysis reveals that, on average, extreme parties hardly gain votes in European elections, compared to previous national elections. The multivariate analysis further demonstrates that, relative to other parties, extreme parties are disadvantaged in European elections. One elaboration of SOE theory, based on MPO theory (Oppenhuis et al. 1996), argues that party extremity itself does indeed not provide a general advantage in European elections, except if the elections are held closer to the end of the NEC, when they become "marker-setting" (see also van der Eijk et al. 1996). In other words, they suggest that the effects of ideological extremity are conditional upon the context of that election, in terms of the place in the NEC.

However, I have not found any evidence for this thesis either. While election timing does impact the performance of extreme parties, it is not in line with MPO theory predictions. Relative to other parties, extreme parties do not get any statistically significant boost in support when (European) SOE are held at a considerable temporal distance from (national) FOE.

Finally, following both SOE mechanism, extreme party success in European elections is also believed to be conditional upon party size. Both theories simply assume that these parties are (or almost all) rather small. However, given the transformation of the European party system in the last two or three decades, this is no longer the case. Consequentially, I investigated if these changes have any impact on the basic proposition, i.e., whether only small extreme parties have a comparative advantage. Multivariate analyses demonstrate that party size does indeed affect the impact of party extremity on its performance in European elections. However, it showed that small extreme parties have a significant *dis*advantage in European elections, relative to other small parties. Given these findings, I have to conclude that extreme parties are not the *winners* of European elections.

The third empirical chapter then addresses when and why voters abstain in European elections or *exit* in Hirschman's (1970) terminology. Consequentially, it has evaluated SOE theory's proposition on turnout, more specifically that turnout in (European) SOE is lower than in (national) FOE (Reif and Schmitt 1980; Reif 1984, 1997; Schmitt et al. 2020). Unpacking this hypothesis, I was mainly interested in how determinants of vote choice and party performance studied in the previous two parts, such as election timing, affect turnout in (European) SOE (cf. Marsh and Franklin 1996). In particular, I strived to evaluate whether the key SOE "less-at-stake mechanism" is universally correct; in short, whether the stakes are always low in European

elections and voters therefore always participate at low(er) rates in these elections. Hence, the key inquiry was into the potential variation in turnout and its potential determinants.

The analyses in this chapter confirm that voters generally participated at lower rates in European elections compared to national elections, suggesting that the former are perceived as less important than the latter, consistently with the basic logic of SOE theory. However, I found no evidence that the stakes of these elections change according to their position in the NEC, and thus election context. The only significant effect of election timing on turnout in European elections was when these were held at the same time as national elections. If this happens, voter participation in European elections increases significantly. In terms of the effects of other variables, the only clear finding was that voters participate more in European elections if participation is high in previous national elections. The less-at-stake thesis was thus largely confirmed.

Voter Motivations in European Elections

Through the aggregate-level analyses, we have arguably learned something about voter motivations and about the effects of the transformations of European politics in the last decades. First, European elections that are held shortly after national elections seem indeed to be dominated by post-electoral euphoria of government party voters and by depression and demobilization of opposition voters. At the same time, voters do not use such early European elections to protest by defecting to extreme parties. Even voters who defect thus rather select a non-extreme (mainstream) option. Taken together, this signals that in the *honeymoon*, the majority of voters vote sincerely (*with a heart*), whether it is because of their "sincere" happiness

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with the results of the previous national election or because of the "throw-away" aspect of the election. They vote for a party they feel ideologically most attached to, even when it has no real chance to affect policymaking in the national political arena.

Second, a combination of sincere and (potentially) protest voting seems to be more frequent when European elections are held after the *honeymoon* period, which lasts for some 100 days after the last national election. However, we have seen that voter defection from government parties in this period is not in the form of voting for extreme parties, i.e., parties that contest the metapolicies of a political system, which is the most clearly recognized form of protest voting. It is thus questionable if voters really vote *with the boot* in these elections, as voting for (other) non-extreme parties could suggest that they are still voting *with a heart* (cf. Weber 2011). The difference relative to the *honeymoon* period is then mainly in the gradual decrease in sincere voting for government parties, as post-electoral euphoria washes away throughout the NEC. The question then becomes, is defection to (other) non-extreme parties also a form of "voice" and of protest (cf. Hirschman 1970, Weber 2011)?

Third, the situation becomes really unclear towards the very end of the NEC, i.e., just days or a couple of months before the next national election. On the aggregate level, the analyses did not show any significant effects in any direction in European elections held in the *late term* of the NEC. Somewhat flipping MPO theory, this could suggest that these are actually "throw-away" elections, rather than "marker" elections, given that voters already know that the next national election is coming.

Finally, when we combine the findings of the three chapters, there is some support for the presence of another pattern of electoral behavior in (European) SOE, which could also explain some of the rather surprising results of the aggregate-level analyses: so-called "protest through

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abstention" (Weber 2011). While the likelihood of voters using European elections to punish governments rises with an increase in temporal distance between these elections and the previous national elections, this translates into an increase in neither support for extreme parties nor turnout. This should indeed signal that some voters chose to protest through abstention rather than by voting for extreme parties.

Implications and Transformations of European Politics

Particularly, the findings of the first chapter can have some potential policy implications for both government and opposition parties. For government parties, in the systems where they can control the national elections day, it may seem plausible to follow Reif's and Schmitt's (1980, 13) advice and strive to call national elections shortly before European ones, while staying away from calling them shortly after. Opposition parties, on the other hand, which may be able to push for an early national election by the motion of no confidence, might want to do this shortly after European elections rather than shortly before. It is interesting to note here, that by giving this advice to government parties, Reif and Schmitt actually suggest an impact of European elections on the first-order national political arena, which the basic logic of SOE theory denies.

The analyses also provided some indications that the transformations of European politics might have important implications for SOE theory. These possibly new patterns of electoral behavior are particularly visible in the 2019 European elections. First, we have seen that the difference in vote shares of government parties between those European elections and the previous national elections was lower than in the other elections. This was mainly because of the lowest average vote share of government parties since 1984 in the preceding national elections. I

have suggested that these results could be caused, amongst other things, by the overall increase in voting for "challenger" parties (e.g. de Vries and Hobolt 2020) and, in turn, less clarity in terms of government responsibility. The suggestion thus directly relates to the *clarity of responsibility* thesis. In short, when other parties do not want to work with a large "challenger" party, government coalitions of various small and often ideologically diverse parties will become more likely. Without either the clear dominance of one coalition party or ideological cohesion, it is more difficult for voters to hold governments to account (see Hobolt et al. 2013). Consequentially, voters may be less clear in their decision who to punish in (European) SOE.

Indeed, the electoral results of extreme parties across time demonstrate that their vote shares had increased gradually in each national election since 1994 (see Chapter 2, 25). This is in line with the lack of effect of party extremity on party support in European elections. As I have suggested, this may mean that some extreme parties have simply become so important in the (first-order) national arena that voting for them in (European) SOE lost its appeal. This seems apparent in the 2019 election set where, for the first time, extreme parties performed better in national than in European elections. In short, the general non-effect of ideological extremity on party performance in European elections could indeed suggest changes in the first-order election arena, namely the "mainstreaming" of extreme parties (cf. Mudde 2019a).

Some of these new developments also suggest a potential deviation from SOE logic. In 2019, for the first time in the history of popular European elections, turnout was higher than in the previous European elections. This can have different explanations, but I have proposed that it may signal the long-anticipated increase in the salience of issues related to the supranational European arena, a possibility that has been proposed and evaluated (with mixed results) multiple times in the past (see e.g. Inglehart and Rabier 1978; Carrubba and Timpone 2005; Hobolt et al.

2008; Clark and Rohrschneider 2009; de Vries et al. 2011; Hobolt and Wittrock 2011; Hobolt and Spoon 2012; Hobolt 2015; Hobolt and de Vries 2016; Hernandez and Kriesi 2016).

This would be very problematic for a theory that assumes that European elections are low stimulus elections that are dominated by national political issues (rather than by specific "European issues"). This assumption feeds the theory's "less-at-stake" logic. When voters do not truly believe that a multinational European body has any significant power over them, they do not truly care, for instance, about the composition of the European parliament or about who has better chances to become its leader. Consequentially, European elections are considered to be not much more than "additional national elections" and many voters will simply choose not to participate in them (Reif and Schmitt 1980; Reif 1984, 1997).

The observed increase in participation in 2019 could signal that this is finally beginning to change. It will therefore be very interesting to observe whether this pattern will continue in the next European elections and whether it will affect vote choice. We can already see some potential effects. First, as already noted, the gap in the vote shares difference of government parties was lower in the 2019 European elections than in previous elections. It was indeed argued that government parties lose more when turnout in European elections is particularly low (Reif and Schmitt 1980, 10). Logically, an increase in turnout should help government parties. Second, and possibly related, extreme parties fared particularly bad in the 2019 European elections, suggesting that an increase in turnout hurt them.

Finally, some of the findings are potentially important for other topics of comparative electoral behavior, such as theories on the cost of governing for extreme parties (e.g. Heinisch 2003; Dunphy and Bale 2011; Elias and Tronconi 2011; McDonnell and Newell 2011; Luther 2011; Mudde 2013; Albertazzi and McDonnell 2015). I have suggested that the observation that

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extreme parties do not perform better in European than national elections might also be caused by the (negative) integration into the system of some of them. This is in line with the discussion on mainstreaming above. As this integration is often in the form of an extreme party's participation in government, it could mean that this costs these parties in European elections and causes the pattern that we have observed. In fact, I did find that voters do indeed mainly punish negatively integrated (halfway house) parties rather than non-integrated (anti-system) parties in European elections.

Similarly, the confirmation of the validity of key assumptions of the *clarity of responsibility* theory in European elections can be used as further evidence for the broader applicability of this important theory of comparative electoral behavior. While true confirmation of these suggestions requires individual level analyses, the empirical analysis presented here should push us to investigate them further and will hopefully serve as inspiration for generating and testing new hypotheses. This will ultimately lead to not only a better understanding of electoral behavior in (European) second-order elections but also in (national) first-order ones.

Conclusions, What Next?

Taken together, the findings were thus rather supportive of the original SOE theory, while less so for later added effects (like ideological extremity). However, while support for the theory remains quite strong some forty years later, these findings also revealed that it needs to be updated and further unpacked. In particular, there is a discrepancy between some of the general hypotheses of SOE theory and some of the explanatory mechanisms that the theory proposes. This was most evident from the analyses of the government vote. It is worthwhile to reiterate that

the fact that government parties are not always the *losers* of (European) SOE can have important implications beyond these elections. Specifically, as already noted, government and opposition parties might be(come) aware of the election timing effects that impact vote choice, which may then affect the calling of national elections or the proposal of motions of no confidence (cf. Nordhaus 1975; Tufte 1978).

Generally, this thesis shows that a lot remains to be explored in terms of SOE theory, especially with regard to voter motivations. Moreover, the elaboration of the numerous mechanisms proposed by the theory can also increase our understanding of broader processes affecting European politics. In other words, studying SOE theory and European elections can provide us with important insights into vote choice and voter participation in general (cf. van der Eijk et al. 1996). Consequentially, SOE theory should not (yet) become received wisdom or something that is only mentioned "in passing". Indeed, the apparent changes in voting behavior in the last European elections should motivate us to continue questioning the key mechanisms of SOE theory.

Let me conclude with a few avenues for future research. First and foremost, it is of the utmost importance to re-evaluate the role of protest voting in European elections, as well as the importance of 'European' issues. In terms of the former, we have seen that the evidence of protest voting in European elections is limited at best, at least at the aggregate level. This is rather surprising in elections that are often characterized as merely or primarily a protest event. In terms of the effects of 'European' issues, the last European election showed some previously unobserved trends, potentially suggesting that these issues could finally become more important for voters. If that would be confirmed, the SOE character of European elections will have to be put into question.

Moreover, this study presented several tentative explanations of numerous processes observed at the macro-level. These explanations are mainly suggestive, given that aggregate-level analyses are incapable to adequately evaluate the micro-foundations behind the macro-level findings. For instance, there seems to be fairly strong support for the so-called "protest through abstention" thesis, but we can only truly prove this if we take into account voter motivations. Consequentially, we should pursue individual-level analyses of these processes in the future. These analyses could complement the findings of this, and other, aggregate-level research (and vice versa).

Finally, some issues should be addressed at both the aggregate and individual level in the future. Given the somewhat different results of the 2019 European elections, we should pay special attention to the next European elections and, in particular, observe whether some of the findings and suggestions of this study are confirmed. Moreover, we still do not completely understand which specific parties are the usual *winners* of (European) SOE. This research shows that it is non-extreme (mainstream) small parties rather than extreme parties that profit in these elections. Future analyses should inquire which particular "non-extreme" parties do well in European elections.

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Appendix 1.1: Average Marginal Effects of Government Status (95% CIs) – Model 2a
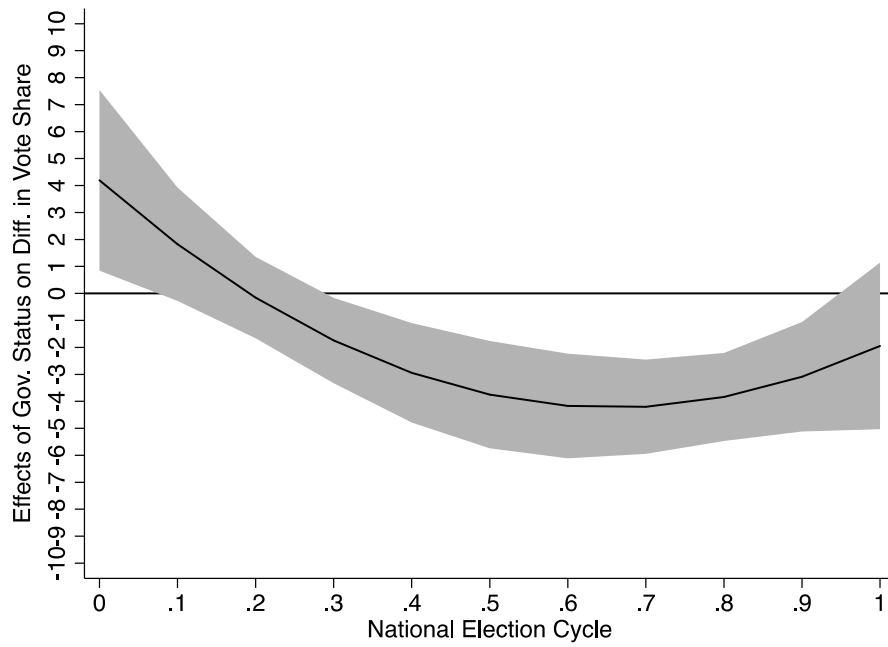
Appendix 1.2: Average Marginal Effects of *ΔGDPGrowth* and Being in Government (95% Cls)
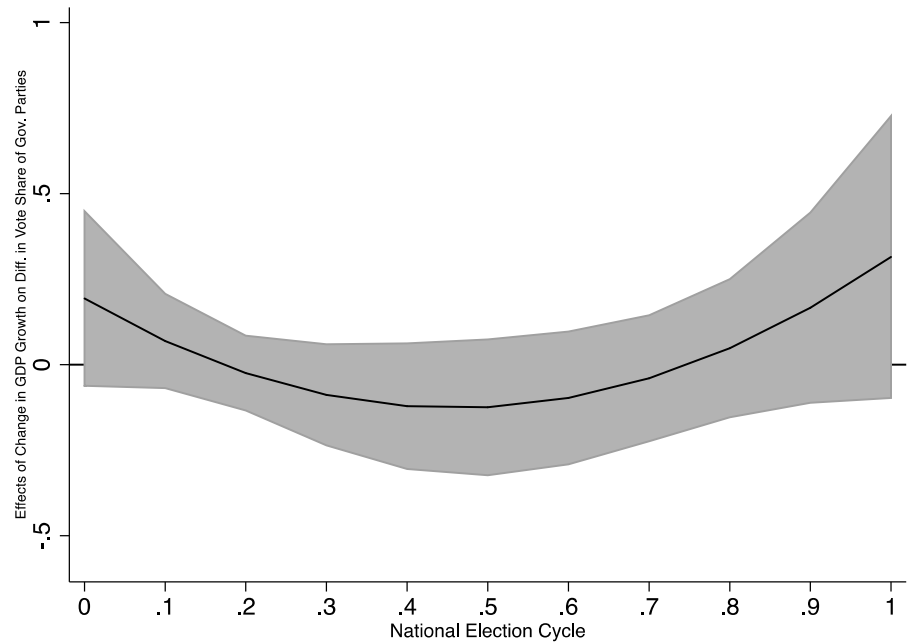
Appendix 1.3: Average Marginal Effects of *ΔUnempl* and Being in Government (95% Cls) – Model 5a

Appendix 2.1: List of 'Anti-system' and 'Halfway house' Parties

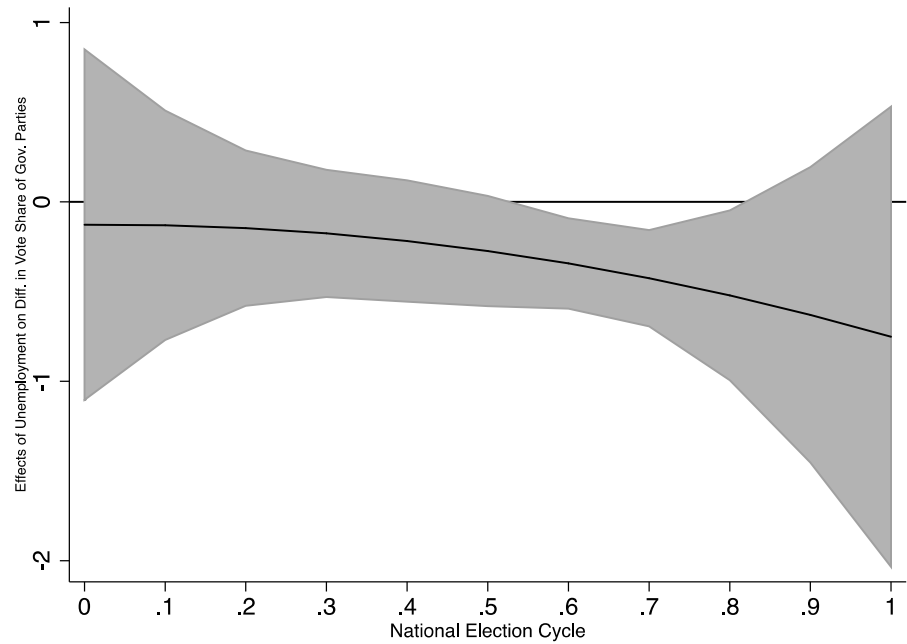| Country	Anti-system	Negatively Integrated (Halfway house)
Austria	Freiheitliche Partei Österreichs-FPÖ (1999, 2009, 2014), Bündnis Zukunft Österreich-BZÖ (2014), Kommunistische Partei Österreichs-KPO, EU STOP/ EU Exit	Freiheitliche Partei Österreichs-FPÖ (2004, 2019), Bündnis Zukunft Österreich-BZO (2009)
Belgium	Kommunistische Partij van België/Parti Communiste de Belgique-KPB/PCB; Vlaams Belang/Blok-VB; Front National-FN; Partij van de Arbeid van België/Parti du Travail de Belgique-PVDA/PTB; Libertair, Direct, Democratisch-LDD; Parti populaire-PP	
Bulgaria	Vŭzraždane	GERB; Ataka; Nacionalno Dviženie za Stabilnost i Văzhod-NDSV; Red, Zakonnost i Spravedlivost-RZS*, Balgarija bes Zensura-BBZ*, Natzionalen Front za Spasenie na Bulgaria-NFSB, Volya, Vatreshna Makedonska Revolyutsionna Organizatsiya-VMRO
Croatia	Zivi Zid, Hrvatski demokratski savez Slavonije i Baranje-HDSSB (2014), Hrvatska Stranka Prava-HSP	Most
Cyprus	Ethniko Laiko Metopo-ELAM, Neoi Orizontes	Anorthotikó Kómma Ergazómenou Laoú-AKEL , Symmaxia-SYM
Czech Republic	Svoboda a přímá demokracie-SPD, Komunistická strana Čech a Moravy-KSČM (2004, 2009, 2014), Suverenita Jany Bobošíkové*, Strana svobodných občanů-SSO, Úsvit přímé demokracie, Pravý Blok, Narodní Strana, Dělnická Strana Sociální Spravedlnosti-DSSS, Rozumní, SPR-Republ.str.Čsl. M.Sládka	Akce Nespokojených Občanů-ANO, Věci veřejné-VV, Komunisticka Strana Cech a Moravy-KSČM (2019)
Denmark	Danmarks Kommunistiske Parti-DKP*, Venstresocialisterne-VS, Retsforbundet-RF, Fremskridtspartiet-FrP,, Dansk Folkeparti-DF (1999)	Dansk Folkeparti-DF (2004, 2009, 2014, 2019), Enhedslisten – De Rød-Grønne-En-O

Estonia	Eesti Konservatiivne Rahvaerakond-EKRE (2014), Martin Helme-EKRE (2009), Eesti Iseseisvuspartei-EIP	Erakond Res Publica-ERP, Eesti Konservatiivne Rahvaerakond-EKRE (2019)
Finland	Perussuomalaiset – Sannfinländarna-Ps (2009, 2019)	Vasemmistoliitto – Vänsterförbundet-VAS, Perussuomalaiset – Sannfinländarna-Ps (2014)
France	Lutte ouvrière-LO, Front National/Rassemblement national-FN/RN), Parti communiste française-PCF (1989, 1994), Mouvement national républicain-MNR*, Ligue communiste révolutionnaire-LCR, Debout la république/Debout la France DLR/DLF), Front de gauche-FdG, La France Insoumise-FI	Parti communiste française-PCF (1979, 1984, 1999, 2004, 2009); Rassemblement pour la France-RPF
Germany	Nationaldemokratische Partei Deutschlands-NPD, Friendensliste*, Deutsche Kommunistische Partei-DKP*, Deutsche Volksunion-DVU*, Die Republikaner, Partei des Demokratischen Sozialismus-PDS/Die Linke, Bund freier Bürger – Offensive für Deutschland-BFB, Alternative für Deutschland-AfD	
Greece	Kommounistikó Kómma Elládas-KKE, Kommunistikon Komma Ellados Esoterikou-KEE(I), Ethniki Politiki Enosis-EPEN, Synaspismós-SYN (1994,1999, 2004), Proti Grammi, Chrysí Avgí-XA, Kómma Ellinismoú, Synaspismós Rizospastikís Aristerás-SYRIZA (2004,2009, 2014), Anexartitoi Ellines-ANEL (2014), Laïkós Orthódoxos Synagermós-LAOS, Kommounistiko Komma Elladas (marxistiko-leninistiko-KKE(M-L), Ellinikó Métopo, Metopo Rizospastikis Aristeras-MERA, ANTARSYA, Nea Dexia*, Elliniki Lisi-EL*, ELLADA, Ethniko Metopo-NF*	Synaspismós-SYN (1989), Dimokratiko Koinoniko Kinima-DIKKI; Synaspismós Rizospastikís Aristerás-SYRIZA (2019), Anexartitoi Ellines-ANEL (2019)
Hungary	Magyar Igazság és Élet Pártja-MIEP, Jobbik, Magyar Munkáspárt-MMP, Mi Hazánk Mozgalom-MHM*	Fidesz (2009, 2014, 2019)

Ireland	Sinn Fein, The Worker's Party, Socialist Party-SP, People Before Profit Alliance-PBPA, Direct Democracy-DDI, Identity Ireland*; Independents 4 Change	
Italy	Movimento Sociale Italiano-MSI, Partito d'Unità Proletaria, Democrazia Proletaria, Partito Comunista Italano-PCI (1984, 1989), Lega Veneta+Lega Lombarda, Lega d'Azione Meridionale-LAM, Lega Nord-LN (1999), Partito della Rifondazione Comunista-PRC (1999, 2004, 2014), Movimento Sociale - Fiamma Tricolore-MS-FT, Partito dei Comunisti Italiani (2009), Azione Sociale, Fronte Nazionale, Italia dei Valori (2004, 2014), Forza Nuova-FN, Movimento Idea Sociale-MIS*, La Destra (2009)*, Partito Comunista dei Lavoratori. Movimento 5 Stelle (2014), L'Altra Europa*, CasaPound-CPI, Partito Comunista, La Sinistra*, Potere al Popolo*	Partito Comunista Italiano-PCI (1979), Forza Italia, Alleanza Nazionale-AN, Lega Nord-LN (1994, 2004, 2009, 2014, 2019), Partito della Rifondazione Comunista (1994), Italia dei Valori (2009), Fratelli d'Italia, La Destra (2014)*. Movimento 5 Stelle (2019), Partito dei Comunisti Italiani (2004)
Latvia	Visu Latvijai!-VL, Latvijas Sociālistiskā partija-LSP	Tēvzemei un Brīvībai/Latvijas Nacionālās Neatkarības Kustība-TB/LNNK, Kam pieder valsts?-KPV LV
Lithuania	Tautos pažangos partija-TPP*, Fronto-SPF, Tautininkų Sąjunga-LTS	Darbo Partija-DP, Tvarka ir Teisingumas-TT, Tautos prisikėlimo partija-TPP, Lietuvos Centro Partija-LCP
Luxembourg	Kommunistesch Partei Lëtzebuerg-KPL, Revolutionär Sozialistesch Partei-RSP*, Kommunistesche Bond Letzeburg-KBL*, National Bewegong-NB*, Alternativ Demokratesch Reformpartei-ADR, Déi Lénk, Liste des citoyens*	
Malta	Imperu Ewropew-IE, Azzjoni Nazzjonali-AN, Moviment Patrijotti Maltin-MPM, Alleanza Bidla-AB	

Netherlands	Staatkundig Gereformeerde Partij-SGP, Communistische Partij van Nederland-CPN, Gereformeerd Politiek Verbond-GPV, Centrumpartij-CP/CP 86, Centrum Democraten-CD, Socialistische Partij-SP (1994, 1999), Partij voor de Vrijheid-PVV, Forum voor Democratie-FvD	Socialistische Partij-SP (2004, 2009, 2014, 2019), Lijst Pim Fortuyn-LPF, 50+
Poland	Liga Polskich Rodzin-LPR (2004), Kongres Nowej Prawicy-KNP, Kukisz 15, Koalicja Odnowy Rzeczypospolitej Wolność i Nadzieja-KORWIN/Wolność/Konfederacja Wolność i Niepodległość, Ruch Narodowy*	Prawo i Sprawiedliwość-PiS (2009, 2014, 2019), Samoobrona Rzeczpospolitej Polskiej-SRP
Portugal	Coligação Democrática Unitária-CDU/PCP–PEV (1989, 1994, 1999, 2004, 2009, 2014), Partido Popular Monárquico-PPM, União Democrática Popular-UDP, Partido Comunista dos Trabalhadores Portugueses/Movimento Reorganizativo do Partido do Proletariado-PCTP/MRPP, Partido Operário de Unidade Socialista-POUS, Partido Socialista Revolucionário-PSR, Partido Democrático do Atlântico-PDA, Bloco de Esquerda (1999, 2004, 2009, 2014), Partido Nacional Renovador-PNR, Nova Democracia-PND, Portugal pro Vida-PPV, Partido Democrático Republicano-PDR	Coligação Democrática Unitária-CDU/PCP–PEV (2019), Centro Democrático e Social – Partido Popular, CDS-PP (1999), Bloco de Esquerda (2019)
Romania	Partidul Socialist Român-PSR; Partidul România Unită-PRU	Partidul România Mare-PRM, Partidul Poporului – Dan Diaconescu-PP-DD
Slovakia	Komunistická strana Slovenska-KSS, Právo a spravodlivosť-PaS, Národ a Spravodlivosť-NaS - ns, VZDOR, Ľudová strana Naše Slovensko-L'SNS	SMER, Ľudová strana – Hnutie za demokratické Slovensko-ĽS-HZDS, Občianska konzervatívna strana-OKS, Sloboda a Solidarita-SaS, Sme Rodina-SR, Slovenská národná strana-SNS, Pravá Slovenská národná strana-PSNS, Aliancia nového občana-ANO, Obyčajní Ľudia a nezávislé osobnosti-OĽaNO

Slovenia	Slovenska Nacionalna Stranka-SNS (2004, 2009, 2014)	Slovenska demokratska stranka-SDS (2009, 2014, 2019), List Marjana Šarca-LMŠ, Slovenska Nacionalna Stranka-SNS (2019), Levica
Spain	Partido Communista de España-PCE/Izquierda Unida-IU (1989,1994, 1999), Herri Batasuna-HB/Euskal Herritarrok-EH, Partido de los Trabajadores de España–Unidad Comunista, Falange Española de las JONS-FE-JONS, Partido Socialista de los Trabajadores-PST, Bloque Nacionalista Galego-BNG (1989, 1994, 1999), Partido Comunista de los Pueblos de España-PCPE, Partido Obrero Revolucionario de España-PORE, Alianza por la Unidad Nacional-AUN, Falange Española Independiente-FE(I), Partido Obrero Socialista Internacionalista-POSI, Democracia Nacional-DN, Alternativa Española-AES, Partido Comunista de los Trabajadores de España-PCTE	Izquierda Unida-IU (2004, 2009, 2014, 2019) ,VOX (2019), Podemos (2019), Bloque Nacionalista Galego-BNG (2009, 2014, 2019)
Sweden	Sverigedemokraterna-SD, Junilistan	Vänsterpartiet-V
United Kindgdom	Democratic Unionist Party-DUP (1979, 1984, 1989, 1994, 1999, 2004, 2009, 2014), Sinn Féin, Worker's Party, National Front-NF, British National Party-BNP, United Kingdom Independence Party-UKIP, Scottish Socialist Party-SSP, English Democrats-ED, Socialist Labour Party-SLP	Conservative (2019), Democratic Unionist Party-DUP (2019)
years in parentheses relate to the year of European elections		
*-party excluded from the dataset for one of the following reasons: 1) did not participate in comparable set of elections, 2) participated in different coalitions in comparable set of elections		

Appendix 2.2: The Determinants of the Difference in Vote Share of Extreme Parties between European and National Elections

	Model 1	Model 2	Model 3	Model 3a

Extremity	-1.381**	-2.232**	-2.522*	-3.154*
	(.523)	(.820)	(1.307)	(1.688)
Party Size	-.190***	-.207***	-.189***	-.185***
	(.027)	(.028)	(.027)	(.028)
TimeSinceNat	-.001	-.001	-.001	
	(.001)	(.001)	(.001)	
TimeSinceNat^2	.000	.000	-.000	
	(.000)	(.000)	(.000)	
Extremity#Size		.085		
		(.063)		
Extremity#TimeSinceNat			.001	
			(.004)	
Extremity#TimeSinceNat^2			.000	
			(.000)	
Extremity# Cycle				7.278
				(6.143)
Extremity# Cycle^2				-5.546
				(5.723)
FarRight	.499	.657	.542	.276
	(.535)	(.562)	(.521)	(.660)
FarLeft	-.176	.064	-.184	-.216
	(.469)	(.531)	(.460)	(.567)
Govopp	-2.465***	-2.497***	-2.468***	-2.758***
	(.559)	(.559)	(.568)	(.619)
ΔUnempl	-.042	-.032	-.034	-.069*
	(.049)	(.050)	(.049)	(.040)
ΔGDPGrowth	-.011	-.007	-.009	-.018
	(.028)	(.028)	(.027)	(.026)
Constant	3.344***	3.576***	3.712***	3.913***
	(.552)	(.509)	(.784)	(.858)

Observations	1,192	1,192	1,192	1,043
R-squared	.256	.260	.260	.271
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Appendix 3.1: Turnout Rates in Countries with Compulsory Voting

Country	Year	Turnout National	Turnout European
Belgium	1979	94.9	91.4
Belgium	1984	94.5	92.1
Belgium	1989	93.4	90.7
Belgium	1994	92.7	90.7
Belgium	1999	90.6	91
Belgium	2004	91.6	90.8
Belgium	2009	91.1	90.4
Belgium	2014	89.4	89.6
Belgium	2019	88.4	88.5
Cyprus	2004	91.8	71.2
Cyprus	2009	89	59.4
Cyprus	2014	78.7	44
Luxembourg	1979	88.9	88.9
Luxembourg	1984	89	88.8
Luxembourg	1989	87.6	87.4
Luxembourg	1994	88.3	88.6
Luxembourg	1999	86.5	86.6
Luxembourg	2004	91.9	91.2
Luxembourg	2009	90.8	90.8
Luxembourg	2014	91.3	85.5
Luxembourg	2019	89.7	84.1
Average		**90**	**84.8**