THE CONSOLIDATION OF POWER THROUGH ELECTORAL ENGINEERING

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INTRODUCTION

The idea for this dissertation came about in the mid-2010s. As I was following political news from various different countries, I noticed a common trend in what happened in numerous places at the same time: politicians were accused of being ‘power-hungry’, and based on this accusation, everything they did was presumed to secure more and more power for them. Any act could be described as being motivated by power: a way of communication, a policy goal, voting for or against a law, changing an institutional setting, nominating someone for a position, etc. This narrative has been especially prevalent in the case of post-2010 Hungary, where the government, having a stable supermajority in parliament, could enact any policy without involving the opposition.

This type of accusation certainly existed even before 2010, it is as old as politics itself. Naturally, politicians have always been interested in power, and observers of politics have always focused on what motivates their acts. Self-interest-based motivations seem to go against the common good, and observers therefore always intended to limit self-interested political actions. The institutions of liberal democracy are also largely designed to constrain the power-seeking acts of politicians.

Nevertheless, this line of criticism appeared more prevalent to me than it was before. Based on the news, it seemed as if every little move politicians made was a cunning attempt to grab power. Journalists and analysts came up with a clever explanation as to how everything is perfectly designed to increase the power of the ruling elite, pointing out how every step was dangerous for our society or democracy as a whole, because it would grant unlimited power for the actor who initiated it.

This feeling of mine was exacerbated by the trends I noticed in the scientific literature. As a political science graduate student, I had numerous classes and readings about democratic theory and democratization, and it was eerie that while most of the literature from the 1990s and early 2000s was generally about how the political situation in the world and especially in Central/Eastern Europe was getting better and better as democracy, capitalism and liberalism all became more and more pervasive, beginning from the late 2000s, the main arguments from papers and manuscripts became bleaker and bleaker. Words like democratization, democratic consolidation, Europeanization fell out of favor, and new concepts, such as electoral authoritarianism, hybrid regimes and populism came to the forefront instead. The description of these emerging concepts seemed to indicate that power-seeking acts by politicians have become more and more common in the last decade.

As I was noticing this obvious trend in political science and news journalism alike, I felt that someone was lacking in these descriptions. I like to test theoretical claims empirically, and I did not see a lot of empirical evidence in the literature that supported this seemingly obvious trend. Sure, democracy indices confirmed the de-democratization trend somewhat, but I thought that the ever-growing prevalence of power grabs have not been analyzed empirically. If more and more power-increasing acts were undertaken, the power of politicians should have been at an all-time high, but I did not see an empirical investigation if that was indeed the case or not.

This was particularly the case in Hungarian politics. Journalists and analysts were constantly warning the public that the government was taking steps to entrench or increase its power, yet I rarely saw any analysis on whether it did so sucessfully. It was taken for granted that increasing power was the motivation for almost every act taken by Fidesz, and it was also widely accepted that politicians are so clever that every one of these moves worked perfectly if they were in fact carried out. The only antidote to the increase of power was if these steps were prevented in the first place, once they happened, they were surely effective.

The efficacy of these steps was seldom questioned, and I did not see a lot of discussion on the effectiveness of these steps in the scientific literature either. Research into these phenomena described the power-seeking nature of politicians, but whether their actions worked or not was rarely examined. My first intention was the fill this gap and analyze this apparent trend empirically.

Then I noticed that the conceptualization of this trend had largely been done from the perspective of democracy and relatedly, populism. The emergence of populism and democratic backsliding were well-documented, but how these affected power relations was not discussed in-depth. From my perspective, the ever-growing power of politicians was the interesting phenomenon, but it was only an ancillary point for the scholars contributing to the theory who were more concerned with the overall state of the democratic system. Whenever they discussed individual actors such as politicians or parties, the focus was on their impact on the system as a whole, not on their individual power. Some of this research accepted that the power of these democracy-destroying actors was increasing, but they did so without empirical evidence and were mostly concerned by how this increase affects the state of democracy, not how power relations were shaped.

I was thus lacking a sound theoretical foundation on which to build the research I intended to conduct. Since I was interested in power relations, it was logical to turn to the literature on power itself to find the theory that could serve as the background of my empirical research. I found that research on power theory has also been mostly focused on systematic power, and the power of individual actors was not discussed in-depth by mainstream contemporary theory, and especially not from the perspective of the power grabs I wanted to focus my research on. This strand of literature was also a dead-end for the purposes of my dissertation.

This prompted me to create the conceptualization myself, and my dissertation consequently has two very distinctive parts. Part 1 is a theoretical argumentation, creating the framework that I will use for my research not just in this dissertation, but also in the future. It will define, describe and classify the consolidation of power, which is the central concept of this dissertation. It is the conceptualization of the trend that prompted me to start the research: the ever more prevalent power grabs by politicians. It is related to the literature of political power, but even more so to the strand of theory where I missed this perspective: democratization and de-democratization.

The consolidation of power is defined as an action that meets four criteria: an actor that possesses power purposefully uses that power to increase its long-term influence or to preserve the power that it already has. This definition is detailed in Part 1, and is compared to the similar definitions of similar concepts in the literature. Following the definition, it is classified utilizing a parallel with the classification of democratic consolidation: positive consolidation increases power, negative consolidation preserves it, while neutral consolidation organizes and embeds it. After presenting these three types of consolidation, the relationship between democracy and power consolidation is discussed in detail. I conclude that all three types can be consistent with democratic principles and can be successfully achieved within democratic constraints, even though each of them could contribute to the erosion of the quality of democracy.

Part 1 contributes a theoretical framework that is related to field of democratic theory but is distinctive, because it focuses on the power of individual actors instead of the state of the democratic system as a whole. It is a contribution in and of itself, but is also a conceptualization that can be used for empirical research, as I aim to do in the second part of this dissertation.

The empirical investigation in Part 2 contributes to a very different strand of political science. It evaluates the efficacy of electoral engineering, i.e. changing electoral rules for partisan gain. In the beginning of Part 2, I explain why this is a tool of power consolidation and why it is suitable for empirical analysis. Then I turn to the literature of electoral reform, describing the potential partisan and non-partisan motivations for altering the electoral system. This is followed by my empirical analysis.

I created a novel dataset that covers of every democratic election from the world between 1974 and 2017. On that dataset, I tested whether electoral reforms actually benefited those who initiated them; did they manage to consolidate their power using electoral reforms as a tool of power consolidation. I conducted this test by answering three different questions. Do reformers improve their performance after their reform compared to their result before the changes were introduced? Do reformers perform better than non-reformers do? And finally, do reformers have a better chance to be reelected than non-reformers?

These tests were done via various statistical analyses. My main dependent variable was the partisan bias of parties; i.e. seat share divided by vote share. The more seats a party could gain with a given vote share, the more the electoral system favored it. My first two hypotheses were thus operationalized as follows:

H1: Parties that enact electoral reforms have a more favorable partisan bias at the election after the reform took effect than they did in the election before it.

H2: Parties that enact electoral reforms have a more favorable partisan bias at the election after the reform took effect than other, non-reformer parties do.

H1 was tested using several different methods. First, simply comparing the descriptive statistics of partisan bias for reformers before and after the reform. Then, a lagged dependent variable OLS regression, where bias at the previous election was among the independent variables in the analysis, while partisan bias after the election was a dependent variable. Finally, I also conducted a differences-in-differences (DID) test, which looked at how the 'treatment' of electoral reform affected the change in partisan bias for reformers and non-reformers.

H2 was tested via OLS regressions, where the dependent variable was partisan bias at the election after the reform, and the main independent variable was whether the party was a reformer or not.

The third hypothesis was operationalized without using the metric of partisan bias, but it was tested using by simply looking at the reelection rates of reformers and non-reformers:

H3: Parties that enact electoral reforms have a better chance of remaining in power after the reform took effect than other, non-reforming government parties do.

This hypothesis was examined through sheer descriptive statistics as well as a binary logistic model, which had reelection as the dependent variable, and the main independent variable was whether the party was a reformer or not.

Throughout the analysis, size of the party (as larger parties tend to have a better partisan bias than smaller ones do) and fixed effects were controlled for. Additionally, I also looked at the potential effects within different regions and for different types of reforms, such as large-scale and small-scale reforms, or reforms that pushed the system in a majoritarian, or a more proportional direction.

The answer to all three of these questions is a hesitant no. I rejected all three hypotheses because there was no statistically significant advantage for reformers; their performance is indistinguishable from that of non-reformers. These results would suggest that electoral engineering is not an effective way to consolidate power either because electoral reforms are generally introduced for non-partisan reasons (and thus the claim that politicians are doing everything to gain power can be rejected), or because they fail to account for all possible contingencies that might occur, and their miscalculations result in ineffective engineering. Either way, at least one of the assumptions that I felt were taken for granted, namely that all political acts are intended to gain power and that politicians are very effective at designing these acts to maximize their power, is wrong in this case. There are a lot of ways to make sense of the null findings, and I discuss them in Part 2. No matter what the exact reason for them, they could be an important contribution to the vast literature of electoral reforms.

While conducting my analysis, I also tested an additional hypothesis about the motivations of electoral reformers. Based on the findings of Andrews and jackman (2005), I hypothesized that reformers were more likely to introduce reforms that made the electoral system more proportional if their partisan bias was comparatively low before the reform. The rationale for this is that proportional reforms 'even the playing field' with regards to partisan bias, i.e. the more proportional a system becomes, the smaller the difference between the partisan bias of different parties. Consequently, parties that were comparatively hurt by the previous system (lower partisan bias) are rationally motivated to diminish the differences of partisan bias. The flipside of this argument is that majoritarian reforms are introduced by parties that have a relatively high partisan bias, because they are the beneficiaries of the electoral system, and by creating a more majoritarian system, they will increase the differences even further to capture further electoral gains.

H4a: The higher the partisan bias of a leading reformer party at the election preceding electoral reform, the more likely it is that it will adopt a majoritarian reform; and the lower the partisan bias of a leading reformer party at the election preceding electoral reform, the more likely it is that it will adopt a proportional reform.

I added a modified version of this hypothesis that posited that it is not popularity itself that is important, but the change in popularity between the two elections. Parties with decreasing popularity are afraid of losing the next election, and are consequently inclined to create more proportional systems to mitigate their electoral losses, while parties with increasing popularity can confidently make the electoral system more majoritarian, since they are very likely to win, and majoritarian systems enhance the electoral gains of winners.

H4b: The more the vote share of a leading reformer party increases, the more likely it is that it will adopt a majoritarian reform; and the more the vote share of a leading reformer party decreases, the more likely it is that it will adopt a proportional reform.

The analysis for these hypothesis was done via descriptive statistics and a binary logistic regression model on the population of reformers, where the dependent variable was proportional reforms, and the main independent variable was vote share at the previous election for H4a, and the change in vote share between the election before the reform and the one after it for H4b. The models did not indicate any significant result for this hypothesis either. The direction of reforms cannot be explained by the partisan bias or the change in popularity of reformers.

The null results have wide-ranging implications. Electoral engineering is apparently ineffective in general, and there could be many reasons for its failure. Firstly, reforms can often be motivated by non-partisan reasons, such as satisfying public demand or ideological considerations on behalf of the reformers. Secondly, reformers could be miscalculating the effects of reforms, as there is a lot of uncertainty surrounding future electoral results and the reaction of voters to the reforms. Both of the explanations are substantial, as they contradict widely accepted beliefs in the literature and especially in the public discourse.

Part 2 thus belongs to the strand of literature that empirically analyzes electoral reforms. It could be considered a contribution in and of itself, but it is also part of the larger framework of research into the empirical analysis of power consolidation. The dissertation as a whole has a combined theoretical-empirical nature, which is pivotal because these areas of political science have operated almost independently of each other, especially with regards to the analysis of the phenomena that belong under the umbrella of power consolidation. There are various theoretical descriptions that deal with similar concepts, and there are plenty of empirical investigations focused on subjects that are related to power consolidation, yet the two are rarely connected to each other.

This dissertation is the first attempt at bridging that gap. It builds a theoretical framework that can be tested empirically and offers a blueprint for empirical analysis. This is only the beginning of a larger project. The next steps of research could focus on expanding the theoretical claims made in Part 1, it could be a further and more in-depth examination of the efficacy of electoral engineering or could be an inquiry into the effectiveness of another tool of power consolidation. Either way, in addition to answering certain theoretical and empirical questions and connecting two different strands of political science literature, my goal is to create avenues of further investigation into the question that got me motivated to write about these topics: are politicians effective at pursuing more and more power?

In addition to the relevance for political science, the answers this dissertation and further research finds to this question is important to politicians and observers of politics alike. Politicians who might be looking to consolidate their power could be curious what tools are actually effective at doing so, and what circumstances improve the chances of successful consolidation. Even more importantly, those who are worried about the consolidation of power could focus on those tools and those circumstances that work in practice and disregard attempts that are generally ineffective. All in all, the findings of this dissertation could be stimulating for political scientists, politicians, and general observers of politics.

As mentioned above, there are two parts in this dissertation. Each part has several chapters, and the chapters are divided into sections. I will refer to the three structural levels as such throughout the dissertation: parts are the highest level and there are only two of them, chapters are the second level (example for notation: 1.2), and sections are the lowest level (example for notation: 2.3.4).

PART 1

THE THEORY OF POWER CONSOLIDATION

1.1 Introduction

1.1.1 The relevance of the topic

The topic of this dissertation has recently received significant attention from political journalists and social scientists alike. The news often covers and scientists often analyze politicians and parties who have attempted power consolidation, power grabs or similar acts; what is more, these acts occur in liberal democracies (such as the US[1]), and hybrid regimes (such as Turkey[2]) alike, demonstrating that this phenomenon transcends borders and political cultures. Despite the growing interest in the concept, it has not been described in detail by political scientists; the term 'consolidation of power' has been used without adequate conceptualization, as if it were self-evident.

The aim of this part of the dissertation is to address this issue and define and conceptualize the consolidation of power, so that I have a theoretical and conceptual background for my dissertation. In addition to the general conceptualization, Part 1 will also present a possible typology of the phenomena that belong to the concept of the consolidation of power, describing the various types of consolidation and identifying their features and illustrating with examples. The typology is created by drawing a

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[1] North Carolina's jarring GOP power grab feels familiar. https://www.washingtonpost.com/opinions/north-carolinas-jarring-gop-power-grab-feels-familiar/2016/12/20/50d530aa-c619-11e6-85b5-76616a33048d_story.html, 31 October 2019

[2] Recep Tayyip Erdogan's purge becomes a naked power grab. https://www.ft.com/content/aed570e6-a410-11e6-8b69-02899e8bd9d, 31 October 2019

parallel between power consolidation and democratic consolidation, the latter being a well-established concept of political science with a well-known typology that can be adapted to power consolidation. This allows me to create a framework for the consolidation of power that can be used in the future, making the understanding and comparison of different cases of power consolidation easier.

1.1.2 The structure of Part 1

Part 1 contains eleven chapters after this introductory one. The second one briefly summarizes the well-established definition and typology of democratic consolidation. The third chapter introduces the definition of power that I will use throughout this part and the reasons for why I chose it over other frameworks. Then, the fourth chapter includes the definition of the consolidation of power as well as details on each criterion of this concept. The fifth one provides then an overview of concepts of political science and other fields of social science that are similar to the consolidation of power and arguments for why they are insufficient to describe and analyze the set of phenomena I am dealing with in this dissertation. Chapter 6 will present the similarities between democratic and power consolidation. Then, in Chapters Seven through Nine, the various forms of power consolidation will be presented using the typology of democratic consolidation as a starting point, describing each type in detail, while the tenth chapter will characterize the relationship between the different types presented beforehand. Finally, Chapter 11 will analyze the relationship between the consolidation of power and democracy, addressing the issue of whether or not the two are compatible with each other and if so, under what circumstances before the concluding remarks of the twelfth chapter.

1.2 Democratic consolidation

In order to describe the consolidation of power and create a typology for it, I first draw a parallel with democratic consolidation. This will be helpful as democratic consolidation has been a central concept of political science since the third wave of democratization began, and there are various similarities between the two concepts as I understand them; similarities on which I will reflect as I define and classify power consolidation.

This is done so that the consolidation of power is approached from a familiar perspective. It is thus only a theoretical crutch; it is not the central topic of Part 1. Nevertheless, familiarity with democratic consolidation will make the understanding and classification of the consolidation of power considerably less complicated. This section thus serves the purpose of aiding the main theoretical contributions that I will begin to present in Section 1.4.

According to the literature, "a consolidated democracy is one that is unlikely to break down" (Schedler, 2001: 66); in other words, consolidation means that "democracy has become the only game in town" (Linz and Stepan, 1996: 5). Consolidation is a process through which the democratic structure becomes more resilient and less prone to anti-democratic attacks. This concept has become especially important for students of Latin American and Eastern European democracies that have had issues with the implementation of the institutions of liberal democracy. Democratic consolidation is believed to be a possible solution to these issues.

1.2.1 Types of democratic consolidation

As it has become such an important concept during the 1990s, it has been defined in countless different ways. First, as students of democratic theory well know, there is no consensus on how to define democracy itself – on the contrary, more and more different definitions come up for this form of government. To only list a few, there are the minimalist (Przeworski, 1999), the pluralist (Dahl, 1982), the participatory (Wolfe, 1985), the liberal (Wolterstorff and Cuneo, 2002) and the deliberative (Elster, 1998) concepts. Depending on which democratic definition one uses, the goal of consolidation becomes different. Most definitions of consolidation focus on the institutional set-up of liberal democracy, and regard the establishment and safeguarding of those institutions as the most fundamental part of it, but that is far from being a consensus.

Other than the disagreement about which definition of democracy to use, students of consolidation often treated democratic consolidation as a process of emulating established (Western) political systems and their norms. This meant that more and more (often conflicting) features have been added to the never-ending list of elements that comprise democratic consolidation. As a result, one cannot know exactly what an author is referring to when he/she is using the term 'democratic consolidation' or 'consolidated democracy'.

In order to mitigate this confusion, Andreas Schedler (1998) identifies five main types of democratic consolidation that can be reduced to three main types. The first one of these is negative consolidation, which is defined as the prevention of the reemergence of an authoritarian regime or the erosion of democratic quality. This is the most basic type of consolidation, ensuring that whatever democratic progress the political system has achieved is preserved and the regime at the very least survives in its current state (which may very well be imperfect in the eyes of democratic theorists). Schedler (1998: 96) considers "eliminating, neutralizing or converting disloyal players" as the "primary task" of negative consolidation – which means that here, he is concerned with the defense of the institutional structure, not democratic norms in society, but it could be extended as the protection and stability of those norms as well. This type is consistent with the "unlikely to break down" definition, and is subcategorized by Schedler into preventing drastic, sudden collapse ("breakdown") and slow decay ("erosion"). The various formal and informal, short-term and long-term, direct and indirect ways to deal with 'disloyal' actors in a democracy is also a focus of Levitsky and Ziblatt (2018).

Once a certain set of institutions is established and regression to an authoritarian regime or a lower quality subtype of democracy is ensured, neutral consolidation (i.e. the organization of democracy) can begin. Organizing democracy may include various measures that make the political system more durable and more ready for further improvement without actually changing its democratic subtype. This version of consolidation is not particularly concerned with establishing or safeguarding institutions per se, but is focused on creating and spreading democratic culture as well as the legitimization and normalization of democracy (Schedler, 1998: 100-101). It is a perpetual process in that it can never end: no matter how advanced a democracy is, it always needs organizing to adapt to changing circumstances averting erosion and facilitating further advancement. However, it cannot begin until at least a certain level of negative consolidation (i.e. the establishment and of basic, stable democratic institutions) has been achieved, after which they can happen simultaneously.

Finally, positive consolidation has to deal with the improvement ("deepening" or "completing") of democracy (Schedler, 1998: 97-100). Democracies (even looking past the various differences in definitions I pointed to earlier) can be qualified in countless ways (electoral, liberal, illiberal, delegative and advanced are just a few examples), and most authors would argue that these subtypes can be ranked normatively. The goal of positive consolidation is to transform the political system from a normatively inferior subtype into a superior one. It should be noted that normatively superior types of democracy are also considered less fragile and more stable, therefore, positive consolidation can enhance negative consolidation. Positive consolidation is more difficult to achieve than the negative one, and usually necessitates a longer period of time as well. This is due to the fact that it combines the improvement of formal and informal institutions, and cannot be attained with a mere top-down approach (as basic negative consolidation can be), but it needs a bottom-up commitment as well, which necessitates an adjustment in the attitudes of citizens. Positive consolidation completes democracy as it transforms a mere electoral way of decision-making to a liberal democracy with extensive and effectively functioning institutions that ensure the rule of law and other liberal principles. Deepening democracy is an even further step that advances democracy from a form of government into a 'way of life' that permeates the entire society.

1.2.2 Embedded democracy

Not only does consolidation have various different types, political scientists have introduced other concepts that have similar meanings or are closely related to the process of democratic consolidation. One such concept is embeddedness. According to Wolfgang Merkel (2004), embeddedness is crucial for consolidation; embedded democracies are less vulnerable and have a higher quality than unembedded ones.

Embedding a democracy is similar to organizing it. It is mainly a variation of neutral consolidation, even though it might include elements of negative and positive consolidation as well – it does not advance democracy from one stage to another, but makes preventing erosion or breakdown as well as democratic progress easier.

Merkel describes four different types of embeddedness. The first one, internal embeddedness means that the various partial regimes (he identifies five: the electoral regime, political liberties, civil rights, horizontal accountability and effective power to govern) of liberal democracy are interdependent and "mutually embedded". That means that certain partial regimes help the functioning of other ones, and at the same time, this embeddedness ensures that neither partial regime infringes on the functional spheres of other regimes. This embedded system provides "potentially conflicting sources of power", where none of the regimes can gain a dominant position that could become a threat to democracy and creates an environment that is unfavorable for the emergence of defective democracies (Merkel, 2004: 43).

External embeddedness is a larger category that Merkel divides into three subtypes. One of these subtypes is embeddedness in the socio-economic environment. This requires meeting certain economic and social criteria a country must meet to expect it to be a stable democracy as these are prerequisites of a democratic system. These criteria include: a high level of economic development (at least in historical comparison, as more and more countries around the world are surpassing the threshold necessary for successful democratization), a relatively low level of economic and educational inequality and a plural social structure. If these requirements are met in a country, that provides an opportunity for most of its citizens to become active members of democratic society who are interested in preserving the democratic system. Those in extreme poverty (either because the country as a whole is not developed enough or because material resources are unequally distributed), those that are insufficiently educated, and those that are excluded as a result of a closed social structure cannot be expected to be actively involved in public affairs, and if these groups make up a large part of society, then democracy will effectively be run by a minority, which could lead to democratic erosion or breakdown (Merkel, 2004: 44-45). Furthermore, social mobility, economic freedom and an education system that focuses on establishing democratic values are also conducive to democratization, showing additional reasons for the importance of socio-economic embeddedness.

The third type of embeddedness Merkel (2004: 45-46) highlights is embeddedness in civil society. This entails a civil sphere that is pre- or apolitical where social life can flourish without infringement from the state, as well as civil associations where citizens can "practice" democracy and critical discourse, as well as accumulate social capital. These civil associations should promote democratic values and should ensure that their members stand against any "tyrannical ambitions" and be a watchdog in the democratic system. Embeddedness in civil society is thus crucial to the establishment of a stable, consolidated democracy.

Finally, embeddedness in the international environment is also paramount for democratic survival and improvement. In order to achieve international embeddedness, a country needs to be a member of political organizations that support democracy, such as the European Union. Military alliances such as NATO are not sufficient for this as the foreign policy goals of democratic countries can be shared by autocratic ones, and democratic values can fade into the background when that is the case. The same is true of strictly economic organizations that might admit non-democratic states even though their core members are democratic. Political organizations where accession is conditional on accepting democratic values provide an incentive for democratization if membership is beneficial (either due to the prestige of the organization or because of ancillary economic or foreign policy benefits), and members can be a watchdog for each other, making democracies less vulnerable (Merkel, 2004: 46).

1.3 The concept of power

1.3.1 The pluralist framework

In order to describe the consolidation of power, I first need to make clear what I understand by power, since numerous definitions of it have been given over the years which have substantial differences between them. I will use the pluralist definition of power, originally established by Robert Dahl (1956; 1961). In his understanding, power is a purely political concept that can be described analyzing formal procedures (such as examining votes in parliament). This is in contrast with broader definitions of power that encompass phenomena outside of the formal political arena.

The classic definition of power in pluralist theory is as follows: "A has power over B to the extent that he can get B to do something that B would not otherwise do" (Dahl, 1957, 202-203). It is (as admitted by Dahl and as pointed out by others) too broad, but is a useful starting point. Dahl later specified his definition so that it can be used to analyze power relations in modern (in his view, pluralist) democracies when he defines power as the ability to influence policy-making in case of disagreement or clear, open, verifiable conflict based on revealed preferences (Dahl, 1961: 330). In his framework, influence can be measured as the ratio of successes and defeats in law-making, that is, the number of occasions when the actor in question was on the winning side of a vote divided by the number of all votes that have taken place. The definition and its operationalization are actually describing two different notions, as the first one is concerned with potential power (or, as spelled out in the definition, an ability), while the operationalized version is actual exercised power that can be observed and analyzed empirically, but potential power can also be compared and analyzed with a priori, deductive methods (for example with the power indices offered by game theory).

Pluralists distinguish four major characteristics of power (Dahl, 1956: 203-204). First, power has a *base* (or source), all the opportunities and possibilities that allow an actor to alter the behavior of another one. These are mostly formal opportunities, usually laid out in laws and regulations that describe what actors in certain position are allowed and prohibited to do, and form the boundaries of power within which an actor can move – the wider the boundaries, the more latitude an actor has. Second, power has *means* (or instruments) that are used to utilize the base. Even though the base clearly outlines the opportunities to influence how others act, actors can choose to use the base in a number of ways. One of these ways is actually seize the opportunity, but other means might include threats and promises of utilization. The means are therefore closely linked to the base, and it is much more difficult to expand them than it is to gain additional sources of power. Third, the *scope* of power consists of the possible responses the actor exerting power can receive from the other one whose actions are to be influenced by the utilization of power. The power of an actor is increased if the scope is limited, as that means that there is a smaller set of possible responses that can be used to counter the exertion of power. Finally, the *amount* of power is the probability of success from the perspective of the actor attempting to alter the behavior of the other. If the action always changes in accordance with the wishes of the actor exerting power, the amount is one (i.e. absolute power), if it never does, then it is zero, but usually it is between those two extreme values.

The distinction between these four characteristics can be especially useful in the empirical evaluation of the consolidation of power. The claim that power is consolidated does not specify what exactly happened, because it is not clear which of these aspects are affected by the consolidation attempt. Power, even in the instrumental understanding provided by the pluralist school, is an abstract concept that could be operationalized in contradictory ways. Clarifying which aspect of power a research focuses on makes operationalization easier and more straightforward.

In the pluralist school, there is a great emphasis on how resources affect power, especially its sources. The more relevant resources an actor possesses, the more power it can attain by gaining additional bases, or increasing the amount of power within a given base. While resources can be concentrated, in modern democracies, they are scattered throughout society (consider votes as a resource – every citizen has one at each election, making it an almost perfectly scattered resource, at least with regards to potential power). The potential resources of power can include formal powers guaranteed by law for a position an actor holds, number of members in a group, the amount of money an actor has at his/her disposal, time spent on influencing policy-making for a given issue, etc. In the case of larger-scale policy-making, no individual has enough resources to shape policy-making alone, therefore, pluralist theory mostly focuses on the power of groups, not specific individuals.

1.3.2 Other frameworks and the consolidation of power

While the consolidation of power could be a concept that is compatible with other definitions of power, there are several reasons why I choose the pluralist definition. First and foremost, my focus is on political phenomena, and it is only natural to choose a definition that comes from a purely political science background and uses the concepts and notions of this discipline. Power consolidation has been described from wider, more general perspectives appealing to sociology, history and other areas, but a purely political science-based definition has been missing from the literature (see Chapter 1.5 on similar concepts).

This narrowly-defined concept proves to be useful to define and create a typology for the consolidation of power. It has clear, identifiable lines of when an actor or group possesses power and when it does not, and it provides a framework that allows for relatively simple comparison between the power of one actor and another, or the power of a certain actor at a certain point in time or at another one. This is crucial to analyze the consolidation of power, as without the possibility to compare, it is impossible to tell whether power has been consolidated or not.

Furthermore, the pluralist concept is the most suitable for my dissertation because it allows power to be operationalized and measured using various metrics, enabling scholars to ascertain whether consolidation attempts are successful or not and to what degree. This is particularly significant because empirically evaluating the effectiveness of consolidation could enhance the legitimacy of this new concept, branching out from political theory to other fields of political science. Other concepts of power are less suitable for operationalization and hence would not allow scholars to analyze the new concept empirically, which I intend to do.

Other possible theories of power that could have been chosen include the one used by some sociologists and historians such as Tilly (1975), Turner (2004) and Mann (1984). They are concerned with the power of institutions such as the state, civil society, estates, etc. Even though the power of these institutions can be consolidated as shown by their analyses, it is not the type of consolidation I, from a political science perspective, am concerned with. It is rather actors within those institutions that consolidate power from the perspective of this research. While this kind of power might be observable, identifiable and comparable, the focus and the perspective is quite different. Nevertheless, relations between institutions might change as a result of power consolidation, and therefore, some phenomena that fall under this sociological-historical definition might fit my criteria as well.

The framework offered by Neomarxist theories (e.g. Poulantzas, 1986; Miliband, 1969) that treat classes as the actors that possess and struggle for power are closer to what I am concerned with: a certain class can consolidate its power vis-à-vis other ones. However, this approach is also inadequate to thoroughly analyze the phenomenon that I identify as consolidation, as a consolidating actor does not have to act along class lines, and it might be difficult to identify whether consolidation has taken place or not. Moreover, not unlike sociologists, Neomarxists also emphasize structure over agency, which is not helpful to conduct research from my actor-centred perspective.

Elite theories of power (e.g. Mills, 1963) that regard different elite groups (or one homogenous elite) as the wielders of power are also incapable to precisely grasp the notion of consolidation. While political actors that attempt consolidation might represent an elite group and their consolidation might come at the expense of rival elite groups or the non-elite (as elite theories would expect), this also might not be the case. In this respect, these theories are similar to the Marxist framework, as one could look at consolidation from their perspective, but that is not the only possible way for consolidation to take place.

The wide-ranging postmodern or post-structuralist theories of power, such as the ones proposed by Foucault (1977), Deleuze (2002) and Derrida (2005) are also unsuitable for my analysis for several reasons. These theories describe power as something that is difficult to objectively verify, let alone measure or compare, hence, while the concept of the consolidation of power could certainly be applied to them in some fork, they cannot help me in my aim of defining a measurable and easily identifiable concept. Moreover, these theories have a scope that go well beyond that of narrowly-defined political science, which contrasts my goal of describing, categorizing and analyzing strictly political phenomena.

This is certainly not an exhaustive list of possible theories of power, but even the brief overview of these prominent examples demonstrate why the pluralist concept suits my research better than other ones. It will be especially useful when creating a typology of power consolidation, identifying the successful and unsuccessful cases of consolidation, and is also applicable for empirical research.

1.4 Defining the consolidation of power

Using the pluralist concept, I define the consolidation of power as follows: a political actor (it could be a person or a group) possessing power uses said power to increase its power (or at least preserve it at the current level) in the long run. Therefore, in order for power consolidation to occur, the following four conditions need to be fulfilled:

1) The consolidating actor possesses power
2) Deliberate action on behalf of the consolidating actor
3) The power of the consolidating actor is utilized in said deliberate action
4) The aim of this action is to increase the power of the consolidating actor in the long run, or to prevent its decrease in the long run

All four conditions need to be fulfilled in order to identify a phenomenon as power consolidation. In the sections below, I will elaborate on what I mean by all four of them, one by one.

1.4.1 The consolidating actor possesses power

According to the first criterion, actors without power are incapable of consolidation. These actors can obviously increase their long-term power through deliberate action, however, they have no power to consolidate. Hence, these actions are not consolidation attempts, but attempts to establish power.

On the one hand, the pluralist framework presupposes that power is widely scattered in society, and there are not many powerless actors (these would include minors for example), therefore, most citizens and especially most politically involved groups are potentially capable of power consolidation, however small influence it has. On the other hand, as a result of the choice of the pluralist framework, institutions (such as the state) cannot consolidate power, as they do not possess any like individuals or groups do. In other frameworks, for example structuralism (Parsons, 1961), institutions would be the main subjects of consolidation, but from a perspective that focuses on the activities and decisions that shape the political arena, they cannot be considered actors.

1.4.2 Deliberate action on behalf of the consolidating actor

The second criterion ensures that consolidation cannot be an accident. Consolidation does not have to take place even if an actor increases its/his/her long-term power if the action that resulted in the increased power was not initiated with the at least partial aim of consolidation. Intent is difficult to discern, and assuming that actors in politics are in general power-maximizers, it is safe to assume that most actions that improve the long-term power situation of the actor that conducts them are intended to result in consolidation, unless there is sufficient reason to believe that is not the case. The opposite would mean that they do not pursue to have a larger effect on policy outcomes, which would contradict one of the basic assumptions of pluralist theory, i.e. that actors aim to get actual policy as close to their policy preference as possible. Exceptions could include cases where the actor is aware that it cannot have long-term power, e.g. a representative cannot seek reelection due to term limits, or if the actor has a policy goal that is only realizable in the short run, e.g. preventing a specific event, such as averting a city to host the Olympic Games, but these cases are rare. Therefore, in general, intent does not need to be analyzed in order to identify power consolidation, it can be assumed that one of the goals of political actors is consolidation.

However, if long-term power increases as a result of actions of another actor, (e.g. mistakes of the opposition parties make the government more popular for a long period of time), it cannot be considered consolidation as it is not the result of a deliberate action on behalf of the consolidating actor. Additionally, power cannot consolidate itself, as consolidation is executed by actors through deliberate action, it is not an event that takes place without explicit action by individuals or groups. This is in contrast to what someone working within a Foucauldian framework would say, where power relations are generally not altered as a result of purposeful actions by those who wield it, but are the outcome of larger underlying forces acting in society; and power can essentially act itself. The pluralist framework does not allow for this though.

This criterion is also necessary to eliminate cases where the power of the actor only increased because it was lucky. This is an issue Barry (1980) raises in general about power: "getting what you want without trying" is not a sign that the actor in question is powerful, but rather that it is lucky (paraphrased by Dowding, 2016: 105). Dowding (1991) further develops this idea by introducing the concept of systematic luck as an important feature of power relations.

While these authors are discussing the relationship of luck and power in general, it is also applicable to the consolidation of power. Accidental expansions of power can be considered increases in power, but not consolidation. Removing this requirement would result in an unnecessary widening of the concept that would incorporate phenomena that are not related to political activities, the central focus of my perspective.

1.4.3 The power of the consolidating actor is utilized in said deliberate action

The third requirement necessitates that power needs to be used in order for consolidation to take place. That means that while even less powerful individuals or groups can attempt consolidation, they are less likely to do so, as their influence is already miniscule. Consolidation is generally attempted and completed by more powerful actors, such as parties in government, who have enough power resources to utilize to even attempt consolidation, and they are therefore the principal subjects of power consolidation analysis.

1.4.4 The aim of this action is to increase the power of the consolidating actor in the long run, or to prevent its decrease in the long run

The fourth condition is probably the most important one, and it should be emphasized that consolidation is aimed to increase or preserve *long-term* power, meaning that actions that have the goal of increasing short-term power are excluded from this definition. The boundaries between the two timeframes are difficult to determine in abstract and should be defined on a case-by-case basis, but in this dissertation, I consider any action that has an effect beyond the current term of the consolidating actor as a possible consolidation attempt. That means that if the action is not expected to substantially affect power relations after the upcoming election, it is deemed to have a short-term focus and should not be regarded as a consolidation attempt.

Often, short-term increase of power does coincide with long-term increase (which would be an example of consolidation), but there are examples when the two do not occur at the same time. Sometimes, short-term increases in power affect long-term power negatively, meaning that short-term power increase can be counterproductive to power consolidation. For example, if a president extends his/her authority and/or limit the latitude of his/her opponents through unilateral action, that could result in considerable backlash from the electorate. Consequently, while his/her power is increased as a result of this action, this is only a temporary effect, as the voters will punish him/her for this at the ballot box at their first opportunity to do so. His/her long-term power will shrink as a direct result of the expansion of short-term power. It is also possible to reduce short-term power in order to achieve consolidation. For example, a party winning an election can decide to not to fill all the positions it legally could with its own members to appear more conciliatory, and as a result avoid a decline in its popularity, therefore losing short-term power, but improving its chances to gain reelection and hence a better shot at preserving its power in the long run. Thus, the sacrifice of short-term power could lead to the consolidation of power as well[3].

1.4.5 Partial consolidation

Power consolidation can also be partial, meaning that an actor stabilizes or increases its influence in a specific policy area or a particular political side. For example, an actor can remain in charge of military decisions or healthcare policy for a long period of time, regardless of its authority in other areas. This can be the case of parties running on platforms that concentrate on a single issue (such as green parties) or non-governmental organizations, activists or special interest groups focusing on a specific area. This simply demonstrates that one does not necessarily need to look for consolidation at the highest level of national or even subnational politics; it can happen in any area without a substantial effect on other areas.

Similarly, an actor can come to be the only viable option of a certain political side. For example, the Labour Party in the United Kingdom has consolidated its position on the left-wing of the British party system in the interwar period. While there have been other left-leaning parties that have supporters and compete in elections, such as the Green Party, they are virtually deprived of a legitimate chance of gaining meaningful influence as a result of the consolidation of Labour's position on that political side (and the electoral system that favors the partial consolidation of power). Hence, these actors are the only viable option on their respective political sides, as Labour is on the British left. This type of partial consolidation does not guarantee a certain level of influence, but preserves the opportunity for gaining power in the first place. In pluralist terms, actors in this scenario consolidate their potential power instead of their actual one. Potential power is not easily observed through the results of the policy-making process, but access to that process certainly constitutes a part of potential power.

[3] For more on this scenario, see the description of the concept of power investment in Section 1.5.3.

1.4.6 Some notes regarding the definition

Consolidation inherently includes stabilization and a comparatively long time horizon by the consolidating actor. If the power increase is only temporary, it cannot be considered an example of consolidation. Consolidated power can also become 'the norm', meaning that most actors are taking it for granted that a certain actor dominates a particular area of policy-making. That can especially be the case if power is stabilized through institutionalization, entrenching the position of one actor. In this case, questioning this position can be considered the same as challenging the political system (i.e. in current Western cases, democracy) itself. That level of consolidation can be considered almost absolute, as power is stable for a long period of time and is unlikely to be disrupted.

It is important to note that using this definition in the pluralist framework puts policy-making in the center of the consolidation: the consolidating actor intends to ensure that it becomes more and more likely that it determines policy outcomes. There are numerous different strategies that allow it to do that, among them avoiding decisions in contentious issues where the status quo is in accordance with the will of the consolidating actor (in line with the concept of nondecision, introduced by Bachrach-Baratz [1970]), or by suppressing potential conflicts (as described by Lukes [1974]), and the focus is on expanding the resources that allow for additional bases and/or means, a larger amount of power, or a more limited scope of the opponents of the consolidating actor in the long run.

The issue is that these strategies are, as highlighted by the authors who introduced them, very difficult to objectively identify and examine. One of the main reasons why I elected to use the pluralist framework is the opportunity for empirical analysis that the clearly observable power relations in that framework allow for. These more obfuscated strategies that the pluralist framework allow for obscure the veracity of the claim that these empirical analyses can reveal all the relevant issues within the power structure.

Since my focus is on empirically observable power, I will mostly disregard these very valid criticisms of the framework I selected, but it is important to keep in mind for broader inquiries of power relations that whatever is uncovered by the analyses created within this conceptual system, it does not include the suppressed conflicts of politics that are not openly debated and challenged. Consolidation by nondecision can be a passive way of consolidating that is difficult to observe empirically, and I will keep the focus on the active ways to consolidate power as a result.

Another prominent issue is a result of the relatively broad definition of a political actor that the pluralist understanding of power requires. This approach generally understands groups as the agents wielding power, and essentially treating groups as singular entities that could use its resources at hand freely to achieve the clearly identified and known goals it sets out for itself.

This assumption has several issues, recognized by the pluralists themselves. Groups have issues of collective action (Olson, 1965): their members might not agree on their goals, and more importantly, the mobilization of resources is generally problematic due to the free rider problem. If the goal is truly shared by the members, then all of them enjoy the payoffs if the goal is achieved, even if they do not contribute to the achievement of it. That means that they are not individually incentivized to mobilize their resources for the group, which could lead to a suboptimal amount of the resources being mobilized, upending the pluralist assumption.

This is an issue collective political actors have to constantly face: parties have to develop strategies to handle the problems of collective action, and in most cases, they are successful in acting as a single, unitary actor in politics. Using the US example, Cox and McCubbins (2007) discuss the various strategies parties use to keep their representatives, their members and indirectly, their voters aligned. These include individual rewards as incentives, the trade-off of goals (i.e. members fight for the goals of each other understanding that the other members will in turn help them achieve their own goals), action by political entrepreneurs who mobilize enough resources themselves.

No matter the specific strategy chosen, in general, parties are quite effective at minimizing the negative effect of collective action problems, as proven by the consistently high level of party discipline observed in both Westminster democracies (Kam, 2009) and in consociational ones (e.g. Castanheira and Noury, 2007). Therefore, treating parties as singular actors is an issue, but in practice, it is an assumption that is acceptable for the purposes of this dissertation.

1.5 Concepts similar to the consolidation of power

1.5.1 Related work in the history of political philosophy

The consolidation of power, as defined in the previous chapter, has fascinated scholars of various disciplines for centuries. For example, when Plato describes how tyranny necessarily forms as a result of the disarray of democracy (Plato, Book VIII), he describes a form of power consolidation to a certain extent. A leader in a democracy seizes the opportunity presented by the chaos democracy necessarily brings about and consolidates its power to the extent that the democratic system becomes tyrannical.

Ever since Plato, many other philosophers dealing with political issues have been describing and analyzing various forms of power consolidation, but I will not attempt to present them. This dissertation does not have a focus on political philosophy or the history of thought, and as such, cannot and does not intend to offer a fair overview of this rich tradition.

Nevertheless, it would be remiss if I did not make an exception to mention the work of Niccolò Machiavelli, who was essentially dealing with power consolidation in his main work, The Prince. The underlying theme of the entire book is how to consolidate power in an effective way; Machiavelli is providing practical advice to a contemporary ruler on how to stabilize and preserve his current power as well as increase it in the future through military conquest, improved reputation, shrewd alliances, well-timed and well-placed financial contributions among other tools. While the term 'the consolidation of power' is rarely if at all used to describe the subject of the analysis of Machiavelli or other scholars of political phenomena, that is essentially what they were doing within the context of both the political realities and the academic standards of their time.

1.5.2 The definition of the consolidation of power in other scientific fields

Modern social sciences have also dealt with similar phenomena. Some scientific fields have even used the term 'consolidation of power' in different contexts, but these concepts differ from mine considerably. Sociologist Jonathan Turner (2004) deals with that concept when he is identifying the basic social forces in society. One of these forces is power, which has four bases: the coercive base (i.e. use of force or the threat of it), the administrative base (i.e. the bureaucracy issuing directives reflecting the will of the powerful and monitoring the enforcement of those directives), the material incentive base (i.e. the manipulation of material rewards and punishments to enforce conformity) and the symbolic base (i.e. the use or manipulation of symbols to enforce conformity). "The consolidation of power denotes the mobilization of all four bases of power by the actors" (Turner, 2004: 232). This definition does not describe the phenomenon with which I am dealing, as it identifies consolidation with a way power is mobilized or used. From the perspective of this dissertation, it is irrelevant whether the consolidating actor uses one or all of the bases mentioned by Turner. Moreover, the specific actor is not the focus of consolidation in this framework, as it is in mine – demonstrating the dissimilar perspectives of political science and sociology.

Turner contrasts consolidation with centralization, which "denotes the extent to which decision-making and issuing of directives is concentrated into a relatively small proportion of actors in population" (Turner, 2004: 232). He writes that while consolidation helps centralization, high centralization is not conducive to consolidation as it focuses on one or two bases. His definition of centralization is closer to what I call consolidation, even though it is far from being identical with it: a consolidating actor usually centralizes power by his actions, but that is not always the case, because this definition does not include the time component that I consider a crucial element of consolidation. Centralization could result in only a temporary, short-term power increase, which (as I laid out in the previous Chapter 1.4) can even be unfavorable for effective consolidation.

Indeed, for historians and sociologists in general, consolidation of power is similar to the concept of centralization. Historian Charles Tilly (1975) is describing the consolidation of state power, defining a process that took place in Europe after 1500, allowing states to become territorially defined, centralized and possessing monopoly over coercive power. Michael Mann (1984) adds that in this process of consolidation, the key was the increase (and in many cases, the establishment) of infrastructural power, i.e. "the capacity of the state to actually penetrate civil society, and to implement logistically political decisions throughout the realm" (Mann, 1984: 189), making state power a part of everyday life for every citizen. In this sense, consolidation of state power is similar to the concept with which I am concerned in that one player is increasing its long-term power systematically. However, in this case, the player is the state itself, not a political actor. In my definition, consolidation of power presupposes that this type of consolidation has already taken place, as I am analyzing modern, mostly centralized states.

1.5.3 Related concepts in political science

Scholars who have drawn a parallel between economics and political science, describing political concepts using well-established terms from economics, have described power as being similar to money (e.g. Parsons, 1963; Deutsch, 1968). In this approach, the consolidation of power is akin to the investment of money. In this view, power (as money) can be either consumed (used for short-term gains), or invested (not used short-term, in order to gain even more in the long run). This approach has a very diverse literature, which combines very different approaches, including that of Parsons, who generally considers the power of institutions, functions and systems, not that of political actors. That is already a different perspective from mine, but there are frameworks where the analogy between power and money is used in a stricter political sense, and in those, power investment is quite similar to the central concept of this dissertation.

An actor that invests power restrains itself and does not influence a certain decision in order to increase its long-term power (Deutsch, 1968: 44-46). This investment can come in a number of different forms. Walter Korpi (1985: 37-41) identifies four different types of power investments: creating mobilization channels (empowering other actors, making them easier to mobilize later), institutionalization of power (creating institutions that allow for more effective use of power later), power conversion (instead of capitalizing on the possession of power for short-term gains, using it to create alternative, more efficient sources of power, such as ideologies), and anticipating reactions (perhaps the most important type: the investing actor restrains itself anticipating that it will draw favorable reactions, resulting in increased power in the long run).

While this interpretation of the investment of power closely resembles my definition of power consolidation, it is not suitable to replace it. One of the reasons for it is that in order to discuss the consolidation of power, it is not necessary to accept the analogy between money and power; while my concept can be used in other theoretical frameworks as well. Moreover, there are issues with the analogy that compel me to reject it. The quantity of money in a community can be grown by several ways, including a more effective way of production, more time used for economically useful activities, etc. Transactions of money can and usually do result in benefits for all parties involved. On the other hand, the total quantity of power is fixed. Power transactions always have a winner and a loser – one actor becomes more powerful, the other more powerless. Thus, the natures of the two concepts is substantially dissimilar, hence, while the analogy has certain benefits when it comes to uncovering certain dynamics, but it cannot be applied in general.

Furthermore, the investment concept has an implicit statement that is not universally true, namely that for the preservation or increase of long-term power, short-term power needs to be decreased (the investing/consolidating actor needs to restrain itself). This could very well be true in many cases, and is important to note, as most other concepts that are similar to mine disregard this very scenario and conflate short-term and long-term increases in power. Separating the two is imperative partly because of the phenomena described as the investment of power. However, restraint is not indispensable for consolidation. What is more, the use of one's short-term power to the fullest extent could lead to consolidation (e.g. eliminating opponents through violent acts). That does not involve any restraint on the part of the consolidating actor, yet, consolidation does take place. Indeed, these cases are among the ones that are most often referred to as the consolidation of power. Hence, the investment concept does not cover all possible scenarios of consolidation.

It could be argued that because current power is risked in such an attempt, it is actually an investment: that invests all the power it has in order to immediately increase power that he/she hopes to stabilize as well. It can be compared to an investment that is extremely risky, but in case of success, provides great returns within a very short period of time. However, even this kind of investment necessitates the investor to part with money for a certain amount of time, while this is not the case when power is used for consolidation. Moreover, the literature of power investment emphasizes the element of restraint, and therefore, connecting consolidation to this concept would be inappropriate. Therefore, while the analogy with investment is applicable to certain cases of power consolidation, it is unfit to describe others, and hence can only be considered a type of consolidation, not an alternative concept to consolidation itself.

Legal theory has unsurprisingly focused on the legal, and most importantly, the constitutional aspects of power consolidation. For instance, David Landau (2013) has introduced the concept of abusive constitutionalism. This concept describes a process where the legislators modify the constitutional foundations of the legal system with the aim of increasing their own power (instead of solving issues the previous system was not suitable to address): the new, altered system is created to ensure (or at least be conducive to ensuring) that those in power remain in power for the long run. Landau focuses on democracies and hybrid regimes, but other legal scholars have dealt with similar phenomena in authoritarian regimes as well. For example, Mark Tushnet (2015) has discussed the various legal tools authoritarian regimes use to entrench themselves. Tom Ginsburg (2014) has also edited a volume that demonstrates through several case studies how a new constitutional order can stabilize authoritarian systems after the initial power grab. While these phenomena are related to power consolidation, their legal focus make them unfit substitutes for that political concept; they do not encompass all the different processes that could be considered consolidation. Therefore, these legal concepts can be considered examples of power consolidation, but are not suitable to describe all of the phenomena that belong to the central concept of this dissertation.

Political scientists have mostly dealt with power consolidation through the analysis of specific cases, but these cases have rarely been included in a larger theoretical framework. Students of fragile and young democracies are an exception to this; they have created several concepts to describe processes where political actors gain power by eroding democratic quality and create new concepts to describe these changes. As a result of these actions, numerous democracies have become defective: in some respects, they have lost their democratic character, becoming electoral authoritarian systems (Schedler, 2006). The same systems have also been called competitive authoritarian systems by Steven Levitsky and Lucan Way (2010). Other concepts that are used to describe similar phenomena also include democratic deconsolidation (Foa and Mounk, 2017) and democratic backsliding (Bermeo, 2016).

Other case studies have pointed out how presidentialization (as introduced by Poguntke and Webb, 2005) became a global phenomenon and have provided a chance to opportunistic politicians to increase their power. For instance, Aurelia George Mulgan (2018) has described the process where the presidentialization of Japanese politics allowed Abe Shinzo to first entrench his position in his own party, then later, using that power, greatly expand his influence in more and more domains of the political arena of the country. Gabriela Tanasescu (2014) used the examples of Russia and Romania to point out that semi-presidential systems are extremely prone to presidentialization (more so than parliamentary ones), and this feature has been exploited by certain actors who have extended their influence in domains where they previously had no competence or were even constitutionally prohibited.

Some scholars have noted how building or leaning on an economic network can prove to be useful to entrench political positions, and therefore an analysis of power relations needs to be extended to the economic arena as well as to the political sphere. Lanskoy and Myles-Primakoff (2018) point out that Vladimir Putin has the ability to dominate Russian politics due to the establishment of an economic system where the main economic actors need to rely on him. This system provides him with almost unlimited economic resources, which serve as the foundation of his power. This is a variation of state capture (as described by Hellman, 1998), where political actors initiate the establishment of the economic network that seizes the state.

All these concepts by various political scientists describe examples of power consolidation, focusing on one specific aspect (the constitutional system, erosion of democratic quality, presidentialization, interwoven economic and political networks), but none provides a comprehensive overview of all the phenomena that are related to the consolidation of power. These contributions make it easier to understand certain types of power consolidation, and are therefore useful tools when dealing with specific cases of power consolidation. However, establishing a theory and typology of power consolidation could foster a deeper understanding of the already established concepts, placing them in a larger framework.

1.6 The parallel between democratic consolidation and power consolidation

My definition of the consolidation of power has a lot in common with the concept of democratic consolidation. Both consolidation of democracy and power are concerned with the long-term preservation and/or improvement of an existing object that is desirable for those that intend to preserve and/or improve it, making consolidation a "teleological concept" (Schedler, 1998: 95). They both have an object (i.e. democracy or power) that is imperfect in its current state, but it has an ideal, perfect version, i.e. an unshakable and perfectly functioning democratic system, or an unchallengeable form of power that is absolute. The objective of consolidation is to move the current, imperfect version of the object towards the desired, ideal state. That ideal state may never be reached, the goal is to make progress towards attaining it nonetheless.

It is imperative that in both cases, the object needs to be established before consolidation begins in an incomplete form (i.e. an imperfect democracy or limited power). A powerless actor cannot start to consolidate its power even if it is attempting to increase its long-term power, and the democratic movement under an authoritarian regime cannot consolidate democracy even though it is trying to bring about a more democratic system.

While the two concepts are similar, they are hardly identical; there are several major differences between the two. Obviously, the objects are not the same, and have significant conceptual differences. The most important of these is that while democracy is a structure that describes the entire political system, and therefore does not belong to any particular actor within the system. There might be actors that are interested in improving democracy and other actors who do not agree with that position, nevertheless, democracy does not belong to only those who promote it. On the other hand, as defined in the pluralist framework, power belongs to specific actors (individuals or groups), therefore, the consolidation of power can be understood from the perspective of the consolidating actor instead of the entire structure.

Consolidating an entire structure is not the same as consolidating something a specific actor possesses. In the first case, multiple actors can be interested and can benefit from consolidation, in the latter, it is generally only one actor that stands to gain from it. However, pervading a decision-making structure and consolidating that very structure can be an effective way of power consolidation. For example, Schabert (1989) describes how creating and stabilizing a structure of decision-making preserved and deepened the influence of one individual (the mayor, Kevin White) and his allies for over a decade. In this case, the consolidation of power is achieved through the establishment and stabilization of a structure, yet, this phenomenon is markedly different from democratic consolidation. The consolidated system in this case, while affecting all relevant actors in the political arena, is created by and benefiting a single actor. Should that actor become less prominent for any reason, the system loses its reason for existence. In case of a democratic system, a consolidated structure stands to remain in place no matter the particular players involved and the change in their situation. The very idea of consolidation in the democratic case is to stabilize and improve a way of decision-making, but when power is consolidated, it cannot be separated from the consolidating actor.

Another difference between the two concepts can be found when the definitions used to describe democratic consolidation are applied to the consolidation of power. The two short definitions of a consolidated democracy I quoted in the Section 1.2.1 seem very similar at first glance, but under scrutiny, it becomes clear that they have subtly different meanings, and extremely different consequences. This is not apparent in the case of democratic consolidation: the goal is to make democratic regimes stable and improve their quality, thus eliminating alternative forms of government, i.e. "making it the only game in town" (Linz and Stepan, 1996: 5) is synonymous with making it "unlikely to break down" (Schedler, 2001: 66). They both seem to be adequate definitions of negative consolidation, and also describe some aspects of positive and neutral consolidation. Although they have somewhat different viewpoints, as the definition of Linz and Stepan seem to describe consolidation from the perspective of the activities that are necessary for the phenomenon to take place[4], while Schedler approaches it from the desired state consolidation should achieve, they seem to describe the same process.

Contrastingly, in the case of the consolidation of power, these two definitions can describe entirely different phenomena. Making the power of a political actor "unlikely to break down" does not necessarily mean the elimination of alternative power wielders. It can be achieved in a fair competition, where the one actor simply outperforms its rivals on a consistent basis in the struggle for power. On the other hand, if the goal of the actor that is consolidating is making itself the only source of power ("the only game in town"), then it is trying to eliminate all its potential rivals. The

[4] Some may argue that in a liberal democracy, democracy is not actually the only game in town. Liberal democratic regimes have certain aristocratic/meritocratic institutions (e.g. courts), and may even have monarchic elements as well (e.g. royal veto power). These non-democratic institutions exist to curb the potential excesses of 'pure' democracy, and complement the democratic features. In the literature of democratic consolidation, however, these formally non-democratic components are treated as essential parts of a democracy, hence, they are included in the "game" that should be unrivaled.

distinction between these two types is significant: while the former is very much compatible with the idea of liberal democracy (and can be achieved through good governance), the latter is an explicit infringement of democratic rules. In Chapter 1.11, I will further expand on the relationship between democracy and the consolidation of power, and the difference between these two definitions is the foundation of that analysis.

Despite the important differences between the two concepts, there is an underlying basic similarity between then. Therefore, I will expand on my initial definition of power consolidation with the help of the literature on democratic consolidation summarized earlier. This will enable me to create a typology of the consolidation of power, which in turn leads to a deeper understanding of the central concept of this dissertation. The various types of consolidation are described in the next three chapters.

1.7 Negative consolidation

Due to the similarity between democratic and power consolidation, I will create the typology of power consolidation based on the typology of democratic consolidation presented above: negative, neutral and positive consolidation, and will distinguish various subtypes within each type.

1.7.1 Competence-based preservation of power

In the case of negative consolidation, the goal of the actor possessing power is the long-term preservation and stabilization of power, preventing the decrease of power it already possesses. Under democratic conditions, this generally means that it uses its power to improve its performance in upcoming elections, ensuring to stay in power. In a competitive political system, the need for negative consolidation is one of the factors prompting the governing parties to be responsive: if they provide good and competent governance, responding to the needs of the electorate, the voters will be satisfied with their performance and will vote to keep them in power[5]. In order to do that, the government will attempt to anticipate what policies will satisfy the electorate, assuming that by executing those policies, its approval will increase and that will lead to reelection[6]. In this case, rivals are not limited or hindered, instead, a situation is created where the current wielder of power is likely to keep its position for the foreseeable future by being competent – that is precisely how democracy is supposed to work. I call this subtype the *competence-based preservation of power*.

There are countless examples for this type of negative consolidation. By definition, each and every reelection is a case of it. Generally, the competence-based preservation of power is highlighted if there are several reelections in a row by the same politician or party, which warrant special attention. Typical examples for parties that were able to consolidate their power in this fashion include the Swedish Social Democratic Party between 1932 and 1976, or the Liberal Party in Japan that (with the exception of two brief periods) has been in power since 1955. If we take look at not only the national scene, but also regional elections, there are even better examples: in the United States, most states are dominated by one of the two main parties (for example, in Utah, Republicans have been in control of both houses of the Legislature and the governorship since 1985), and in Germany, the Bavarian Minister-Presidents have been exclusively members of the Christian Social Union since 1957, and the party has also had control of the local legislature during this period (between 2008 and 2013, they were forced to enter a coalition with the Free Democrats but they still dominated the Landtag). Giovanni Sartori (2005: 172-193) called parties that achieved this

[5] Assuming their preferences are mainly shaped by government performance, and also assuming that the main aim for governments is to remain in power.

[6] This process is described in detail by Friedrich (1963). However, it does not work in case of term limits, when the incumbents ar barred from competing at elections after a certain amount of terms, as in that case, even the best governance does not result in negative consolidation. That is why it is better to understand this model in terms of parties instead of candidates, as parties can achieve negative consolidation even in cases where a term limit prevents incumbents from doing the same.

consolidation (i.e. the ones that won at least three consecutive competitive democratic elections) predominant parties. This is also part of the incumbency advantage (see for example Erikson, 1971) – parties and politicians in power can signal to the electorate that they are competent by proving they are capable of good governance, while their challengers do not have the same opportunity.

It is noteworthy that subnational levels of government, including municipal administrations, provide more examples of this type than national ones. This is probably due to two different factors. On the one hand, the smaller an electorate, the smaller the pool of potential candidates, meaning incumbents have a greater advantage as they might not even have viable opponents. As a result of this, smaller levels of competence might be enough to achieve negative consolidation, as the challengers are not as formidable. Additionally, subnational elections are usually considered second-order elections (Reif and Schmitt, 1980), meaning that voters do not pay as much attention to them as national ones. The lack of interest puts challengers in a difficult situation, as they struggle to become well-known. These two reasons can also interact with each other: there are fewer competent challengers at the subnational level due to the perceived lower prestige, setting the bar of competent governance lower for subnational incumbents, making negative consolidation easier to achieve.

1.7.2 Limiting competition

However, another subtype of negative consolidation is markedly different from the one described above. If negative consolidation means that the consolidating actor completely eliminates all of its rivals, institutionally or otherwise excluding even the possibility of them increasing their power (as the "only game in town" definition would suggest), then consolidation is necessarily an authoritarian tool that cannot be successful in a democracy. Generally, when the term consolidation of power is used in scholarly studies, it is used in this sense. Most often, these papers study how political actors who seized power through democratic, semi-democratic means or a coup made their system less democratic and hence more authoritarian, ensuring that they remain in power. The Nazis leading Germany from a democratic to a totalitarian regime in the 1930s is probably the prime example (see e.g. Epstein, 1962). Other similar processes that have been labelled as a consolidation of power are Nasser's rise in Egypt (Thornhill, 2004), Hamas' control over the Gaza Strip after 2007 (Milton-Edwards, 2008), and Bashar al-Asad's early years as the leader of Syria (Becker, 2006). I call this subtype *limiting competition*. As seen from the list of examples, the history of hybrid and authoritarian regimes provides numerous cases of this subtype of power consolidation.

Once again, it is important to differentiate when this process happens through the institutional and/or violent means, or when it is a result of shrewd political moves. The examples of power preservation cited above demonstrate that it is possible for an actor in a democratic environment through fair competition to be essentially "the only game in town" for a certain period of time. The difference in the means of achieving that status is crucial to identifying which subtype a specific act of negative consolidation belongs to.

1.8 Positive consolidation

Both subtypes of negative consolidation overlap with positive consolidation. As positive democratic consolidation "deepens" or "completes" democracy, positive consolidation of power deepens or extends the power of the consolidating actor. When positive consolidation of power takes place, not only is it ensured that the actor that wields power will not relinquish it, but it increases its power, getting hold of new sources of power that have not been under its control beforehand.

1.8.1 Deepening power

There are two subtypes of positive consolidation, based on what aspect of power is increased. The first one is *deepening power*. In this case, the consolidating actor increases the amount of its power, i.e. the probability of power being successfully exercised, or the likelihood of effectively influencing other actors using a certain means and within a certain scope. For example, if a political party had two seats in a committee that has a fixed number of members and the tasks of which remain unchanged, and then it possesses three after a consolidation attempt, then it has more influence in the affairs than before; hence its power is deepened. The most common example of this subtype is increasing the vote share a party has in the legislature (i.e. winning more seats in an election than the one the party had beforehand).

1.8.2 Extending power

The other subtype of positive consolidation is *extending power*. In this case, the aim of consolidation is to influence domains that the consolidating actor previously did not affect, i.e. gains additional bases for its power. For example, if the party in the above example instead of gaining an additional seat in a committee on which it already had members puts a member on a committee on which it previously had no members (and hence no influence in the matters decided by that committee), it extends its power to the domain with which the committee with the new member is concerned. A common example of this within a democratic framework is when a party gains control of a ministry it did not lead before, e.g. a member of the party becomes the minister of defense, while previously, the party had no substantial effect on military policy.

1.8.3 The relationship between the two subtypes of positive consolidation

The two subtypes can naturally coexist, and they can even enhance the effect of each other and accelerate their respective development. As power deepens within a certain domain, it becomes more and more influential within that sphere of policy-making, its policy positions will affect wider and wider areas as changes in the various domains affect other spheres. For example, as an actor deepens its power in the area of environmental policy, it will indirectly affect other domains, such as industrial policy in an ever larger fashion. Thus, since the more power one has in a certain sphere, the more and more its power is intertwined with other areas and interests, the deepening of power can easily lead to the indirect extension of power.

Furthermore, the extension of power by definition increases the bases of power of the consolidating actor, which also means that its resources become more diversified. As an actor acquires more and more bases of power, its overall power is more secure, since if it loses one of those bases, it still has others that it acquired during the extension of power. This stabilizing effect is generally conducive to negative consolidation (I will further elaborate on the relationship between positive and negative consolidation later in Chapter 1.10), and it also makes the deepening of power more achievable. An actor with more bases of power is less protective of one specific power source and can be more confident in venturing to deepen its power, because it has less to lose if it fails – it can always fall back on its other bases of power, and it will remain powerful even if the deepening proves to be unsuccessful and loses all of its influence in the domain where it attempted to increase the amount of its power.

In addition to this inflated sense of confidence, extended power can also have other advantages for deepening power, the main one being the flipside of the benefit of deepened power has when an actor attempts to in broaden the sources of its power. When an actor gains influence in more and more intertwined policy areas, it becomes easier and easier to deepen its power in any one of those, because indirectly, it already did by default when it extended its influence to domains that are related to one another.

The ever-increasingly intertwined nature of policy areas thus makes it difficult to distinguish these two subtypes in practice, proving that there is a possibility that they can coexist and are far from being mutually exclusive.

1.9 Neutral consolidation and embedding power

As for neutral consolidation (i.e. ‘organizing democracy’), ‘organizing power’ is just as important for consolidating of power as negative or positive consolidation is. When the consolidating actor organizes its power, neither does it make it less likely to lose, nor does it increase the power it possesses, but it adapts to the changing circumstances and prepares for negative or positive consolidation attempts. No power structure can remain unaltered for a long period of time: it is either adapted or it breaks down. Therefore, neutral consolidation of power is crucial for consolidating actors, regardless of the regime type in which it is acting (democratic or authoritarian). Neutral consolidation is a slower, every-day process that furthers and is in most cases a prerequisite of positive or negative consolidation.

Neutral consolidation is therefore a rather difficult concept to identify in practice, especially in the pluralist conceptual framework that focuses on identifiable conflicts in policy-making. Neutrally consolidated power does not make it more likely that the interests of the consolidating actor affect the final policy outcome nor does it preserve the already existing levels of power it has, at least not in a direct way. It aids those processes though and is crucial, yet all but invisible when measured by the pluralist methodological framework.

This is where the concept of embedded democracies, as outlined in Section 1.2.2 can prove to be helpful. Embeddedness is a useful and much more identifiable concept that is generally used synonymously with neutral consolidation and it has a clear typology created by Merkel (2004). Therefore, I will base my description of neutral power consolidation on this analogy, and will describe *embedded power* instead of neutrally consolidated power, as it is a concept that is much more compatible with the pluralist framework than the elusive notion of the negative consolidation of power.

1.9.1 Internal embeddedness

Just as in the case of democratic embeddedness, power can be embedded both internally and externally. Making the various aspects in the power structure interdependent, i.e. *embedding power internally* makes the consolidation of power much easier to accomplish. This is related to the description of intertwined policy spheres in Section 1.8.3 that make the coexistence of the two subtypes of positive consolidation natural. Embedding power internally is the flipside of this relationship though. While in that section, I assumed it as a given that some policy areas are naturally intertwined, and in many cases they certainly are, internally embedding power is the process of the consolidating actor actively interweaving domains of policy-making.

When power has numerous interrelated bases, there is no single point where it could be 'attacked': in order for one aspect of power to be seized by other actors, a whole structure of power need to be broken down, and that is more difficult than taking over the systems one by one, and that results in the structure becoming less vulnerable overall. This makes negative consolidation more likely than if the bases were not intertwined. Additionally, as outlined in the Section 1.8.3, intertwined areas of influence can also be conducive to positive consolidation.

Another way to think about internal embeddedness is the institutionalization of power. It is also a way of connecting the various aspects of the power of the consolidating actor, and creating an interdependent system out of them. This can happen within one sphere or between multiple domains of policy-making. Even within one domain, a single actor, e.g. a party can have different bases of power, e.g. multiple representatives with somewhat differing opinions. Institutionalization in this case would comprise of establishing a process that would ensure that these bases act in concert, such as negative or positive incentives for party discipline. The same can happen across policy areas, guaranteeing that the power bases of the consolidating actor do not conflict with each other and all work towards the goals of the actor. Institutionalized power is considerably easier to preserve or increase in the long run.

For example, this kind of embeddedness was crucial for the establishment Nazi regime, where the various institutions within the power structure were strongly interdependent. Even within the same policy area, local, national and party organizations had interdependent responsibilities. This interdependence, i.e. internal embeddedness ensured that the influence of any particular actor within the system was always checked by another actor within the system, and the only way to resolve these issues was to appeal to the goals of the higher authorities of the power structure, and ultimately, the Führer himself (Arendt, 1951). This discouraged actors to challenge the system itself, limited the independence of the separate power bases and directed policy-making at every level towards the goal of the consolidating actor, thus enabling both negative and positive consolidation.

1.9.2 Embedding power in the socio-economic environment

External embeddedness is equally important for the consolidation of power. The three types of external democratic embeddedness are useful in the case of embedding power. When the socio-economic context, civil society and the international environment all favor the existing power structure, consolidation is clearly easier than when they do not.

The first type to consider is *embedding power in the socio-economic environment.* This entails creating an environment where social and economic conditions are favorable for the consolidation of power. That could simply mean improving the economy of a country through good governance, increasing the likelihood of reelection.

This would only embed power temporarily, power could be more deeply embedded through other means. For example, a government can ensure that social and economic groups that could potentially support it are content and loyal by granting special privileges to them, e.g. a pro-business party can introduce significant tax cuts to businesses in hopes of business owners other voters influenced by them would support them, or a left-wing government could expand in a considerable way to garner the votes of workers. When enacting these measures, the government is looking to solidify the support of a certain group and is not looking to achieve 'the public good' (as they do in the first example when good governance simply creates a tide that rises for all), and these policies may be harmful on the whole for society, but could be an effective way to prepare for negative or positive consolidation.

Consolidating actors can go even further and embed power socio-economically by elevating smaller groups or even individuals with disproportionate influence to positions of privilege. For example, union leaders could be essentially bribed by involving them in the policy-making if they promise to ensure that union members will support the government at the ballot box. This act does not even consider the well-being of the groups that support the government, let alone the entire nation, but only privileges a select few, yet could enhance consolidation attempts.

Another form of embedding power could be to make economic or social systems subject to or dependent on the political system. Taken to the extreme, this would mean that the state would become a 'kleptocracy' – the entire economy of the country is subject to the ruling political class and only serves their own interests, may those interests be political or economic (Lanskoy and Myles-Primakoff, 2018). Thus, the complete economic system can be mobilized as a base of power, and thus could prove to be an asset for power consolidation. The flipside of this scenario is when economic players pervade the political system and it becomes a base of their power that they essentially control. This is called state capture, the dynamics of which is aptly described by Hellman (1998).

Consolidating actors need to pay attention to the socio-economic context as a hostile environment can make it impossible to consolidate power either negatively or positively. Groups that feel social exclusion or underrepresentation, or voters who suffer economically are more likely to challenge power than those that have either ambivalent feelings about these issues or have a positive view of their own social and economic position. Embedding power in the socio-economic environment can not only help the consolidating actor in an active way, as described above, but can also prevent countermeasures against the consolidation of power even before the attempt is initiated.

1.9.3 Embedding power in the international system

Another type of external embeddedness is being embedded in the international system. *Embeddedness in the international environment* means that the power structure is supported by foreign actors, either tacitly or explicitly. Actors from other countries or international organizations can provide assistance in the form of financial or military aid or simply improving the reputation of the consolidating actor by endorsing it in a campaign, signing treaties and documents that reflect positively on the consolidating actor or allowing it to join prestigious 'clubs', i.e. international organizations where membership is coveted and considered beneficial. On the other hand, lack of embeddedness in the international environment poses significant issues for power consolidation: if the relevant international players oppose those who possess power, they have at least one more opponent with which to be concerned during consolidation, and their internal rivals might become more dangerous to the power structure due to the financial, military or symbolic assistance provided by international players. Thus, international embeddedness is crucial to the efficacy of power consolidation.

This is especially true in the case of smaller countries that rely on their larger counterparts with regards to defense, economy and other aspects. For example, the consolidation of power by the Communist parties in Central and Eastern Europe after the Second World War was made possible by the direct assistance of the Soviet Union – without diplomatic, military and economic pressure by the Soviet government, it would have been impossible. However, once that pressure and support vanished after 1990, the Eastern Bloc collapsed. Many governments in this regions realized soon afterwards that international embeddedness is crucial to them remaining in power, and joining the international organizations of the West (NATO, EU) became a primary goal for them, almost regardless of their ideological orientation (Lasas, 2010).

While international embeddedness is less important in larger countries, it can prove to be helpful in achieving consolidation. For example, the improving international reputation and the positive developments of the Cold War throughout the 1980's helped improve the popularity of the Republican Party in the United States, and the Party used this opportunity to consolidate its power. Generally speaking, how a country, a party or a politician is perceived internationally often plays a part in the decision-making of the electorate of most countries in the globalized world, especially if there is a large contrast between candidates in an election. This proves that even though domestic relations usually take precedence in political science analysis, or alternatively, studies rely only on the international relations aspects of a specific issue, the international embeddedness of domestic influence can rarely be neglected when analyzing the consolidation of power on the national level.

1.9.4 Embedding power in civil society

The third type of external embeddedness, *embedding power in civil society* is a more complicated concept as it might be achieved in a number of different ways. Civil society can foster or impede consolidation, and ensuring that their reaction is as positive as possible can be crucial to the success of consolidation attempts.

Even if the level of domestic political opposition, the attitudes of major social and economic groups and the international environment are all favorable to the consolidation of power, if there is a potential grassroots resistance in civil society, the consolidation attempt might be destined for failure. On the other hand, civil society can be an important stabilizing force for the power structure by actively supporting it, and can help the power of a certain actor 'become the norm', which is extremely conducive to the effective consolidation of power.

The consolidating actor has several options to make civil society more receptive to power consolidation. Firstly, it can choose to hinder hostile elements of civil society that are opposed to consolidation by introducing negative or positive incentives to these components of civil society. That could include legally curbing the activity these organizations to ensure that their active opposition is not as fierce as it otherwise would be. This could prove to be disadvantageous as these measures could cultivate support for these organizations and could result in backlash for the consolidating actor. An example of this kind of activity would be the treatment of certain non-governmental organizations in Hungary between 2010 and 2020[7]. Their activity and funding was significantly limited by the government that deemed them political organizations because of their opposition to certain measures, a number of which could be considered attempts to consolidate power.

Antagonistic elements of civil society could also be compromised with, potentially diminishing the fervor of their opposition. This would mean that at least some of their wishes are granted, essentially appeasing them. This would not make these

[7] See for example the summary of the Helsinki Commission Report from 2020: https://www.csce.gov/sites/helsinkicommission.house.gov/files/Shrinking%20Civil%20Society%20in%20Hungary%20Designed%20FINAL.pdf Last accessed on December 31st, 2021.

organizations supportive of the power structure, but their opposition would become less active and therefore less harmful from the perspective of the consolidating actor.

Another option is not suppressing the antagonism of civil society, but to encompass potentially supportive elements of civil society into the power structure. For example, the church or non-governmental organizations could be involved in the political decision-making. In turn, the influence of these groups can be used to improve the perception of the consolidating actor, thus making the consolidation of power more likely to succeed.

For example, this technique has been used to great effect in Poland. The Polish Catholic Church is extremely influential among certain groups of the electorate, and its support has decided elections in the past. Parties often seek the support of the church, and at certain points, the two can become indistinguishable to the extent that if someone is a devout Catholic, one can tell his/her political preferences with relative certainty based on what the position of the church is. That is beneficial for parties that have the support of the church, included their members in their ranks and attempt to consolidate power, as they can rely on the support of the most major player in civil society (Zuba, 2010).

1.10 The relationship between the different types of power consolidation

The various types of consolidation can coexist, one can be the prerequisite of the other, can enhance the effect of one another, can impede the development of each other, or could be completely independent. The dynamics can vary on a case-by-case basis, but there are general trends that I will present in this chapter.

1.10.1 Negative and positive consolidation

Positive consolidation can be achieved with or without negative consolidation. Most often, it is achieved after or along with negative consolidation, as the consolidating actor generally builds on (extends or deepens) power that it manages to preserve. When power is already negatively consolidated, that is, it can be safely assumed that it cannot crumble even in the long run, an actor might be more emboldened to consolidate power in a positive manner as well. In the case of deepening power, positive consolidation by definition includes negative consolidation as the power wielded by the actor is increased in the same domain, meaning that the power previously possessed in that very domain is naturally preserved (and deepened). In general, negative consolidation is a soft prerequisite of positive consolidation and in most cases precedes it.

However, there could be instances where the extension of power occurs without successful negative consolidation. Consolidation is aimed at long-term power, a phenomenon in the future that is by definition uncertain, meaning that the success of any consolidation attempt is dubious. Therefore, the consolidating actor might attempt extending its power believing that its current power is negatively consolidated, when in fact it is not. For example, a party that is dominating the political arena in a region might focus its resources during a municipal election campaign on other regions, believing it could extend its power there as well. It might succeed, gaining influence in new regions, however, due to the lack of resources focused on the region it already dominated, it might lose power there, hence not achieving negative consolidation while successfully consolidating its power positively.

Alternatively, an actor can look to extend its power while purposefully neglecting negative consolidation. For example, when Recep Tayyip Erdogan elected to run for the presidency of his country instead of remaining Prime Minister in 2014, he voluntarily forewent the opportunity to consolidate power negatively in order to extend his power (gain the influence the President has instead of preserving the power the position of Prime Minister provided him). He did so thinking ahead; as he and his party planned to broaden the competences of the President by making Turkey a semi-presidential system, which he achieved a few years later[8]. Hence, political actors might not be concerned with preserving the power they already possess when they are looking

[8] Turkey's parliament set to approve sweeping new powers for president. https://www.theguardian.com/world/2017/jan/19/turkeys-constitutional-reforms-set-for-approval-in-parliament, 15 November 2019.

to increase their influence in the long run. However, the lack of negative consolidation generally does not bode well for the efficacy of a positive consolidation attempt.

Positive consolidation is inherent to the second subtype of negative consolidation: those who ensure that they remain in power by creating a more authoritarian regime increase the power that they were given under the system that made them the ruling actor (again, the Nazis are a clear example). These actors do not intend to simply preserve the power they already possess, they use that power to hinder opponents to create an environment where even more influence is available to them (and they can successfully consolidate power positively as a result). Hindering or eliminating rivals creates empty spaces of power that the consolidating actor attempts take, therefore, negative consolidation is a prerequisite for positive consolidation. Again, this is the process that is usually talked about when the consolidation of power is discussed in either scholarly or journalistic articles.

When discussing this relationship, it is important to remember the timeframe of consolidation. Attempts that focus on increasing or preserving power in the short run are not considered as cases of consolidation, as explained in Section 1.4.4. Examples of positive consolidation attempts without already stabilized power can easily be confused with attempts that have no regard to long-term power and have a pure focus on short-term influence. These might be called consolidation attempts, especially by the media that generally has a short-term focus itself, but according to my definition they are not considered as such and are not a subject of the inquiry in this dissertation.

Finally, the case where positive consolidation precedes negative consolidation needs to be discussed. After a successful positive consolidation attempt, the newly acquired power can be stabilized through negative consolidation, and this describes an often observed cycle: power is established, then it is consolidated negatively, then it is consolidated positively, then again negatively, etc. However, the more influence an actor possesses, the more challenges it has to prepare for, and therefore, successful positive consolidation by definition makes negative consolidation more difficult in general. In other words, the deeper an actor is in the above-mentioned cycle, the more arduous it is to remain in the cycle and consolidate power further and further.

1.10.2 Embeddedness and negative or positive consolidation

Neutral consolidation is mainly a preparation for either negative or positive consolidation, and is therefore always intended to work in concert with another type of the central phenomenon of this dissertation. Depending on the circumstances of the situation, embeddedness might be a prerequisite of consolidation, as especially external embeddedness could be necessary to create an environment where preservation, stabilization or expansion is even conceivable.

Nevertheless, even if power is embedded, successful negative or positive consolidation is not guaranteed. Just because the environment is, or at least seems to be favorable to the consolidating actor, success depends on a number of other factors as well, and cannot be taken for granted. Furthermore, unembedded power can be consolidated. For example, even an actor with virtually no international support can preserve its influence in the long run if foreign actors do not exercise enough resources to avoid successful consolidation.

Moreover, embeddedness can even prove to be detrimental for the effectiveness of consolidation attempts, or rather, the lack of embeddedness could be used in the quest for consolidation. This is particularly true in the case of external embeddedness: a hostile international environment, an antagonistic civil society or the opposition of powerful social and economic groups can be used to create a narrative that the actor is treated 'unfairly' and can only rely on the support of the electorate, or 'the people', instead of 'special interests' and 'foreign agents'. The narrative reason for why consolidation is necessary can thus be the lack of embeddedness.

As the flipside of this, actors with embedded power could be accused by their opponents of not engaging the electorate, and thus could end up being the reason their power crumbles instead of stabilizing. This strategy is frequently by populist politicians, who attack the very notion of embeddedness and identify it as a political evil to be dismantled. This shows that while embeddedness is often a prerequisite for successful consolidation, not only does it not guarantee effective stabilization or expansion, it could even be damaging for the consolidating actor.

1.11 Democracy and the consolidation of power

Now that the typology is established, it is important to address the relationship between democracy and the consolidation of power. This is necessary because of the similarity and parallel with democratic consolidation, and also because both political scientists and journalists generally use the term 'the consolidation of power' when discussing detrimental trends to democracy. Most of them consider the consolidation of power as either a harbinger of democratic erosion or an apparent sign that the political system in question is either authoritarian or a hybrid regime, but not a full-fledged (liberal) democracy.

This claim is not baseless. One of the most important features of modern democracies is the separation of powers, and political scientists often emphasize the importance of a relatively equal power distribution as a key characteristic of democracies in contrast with power concentrated in the hands of smaller groups in nondemocratic regimes (e.g. Vanhanen, 2000: 252-253). Coincidentally, this is also the assumption of the pluralist framework that I use. Extrapolating from this claim, if one actor increases and stabilizes its power in the long run, i.e. consolidates its power, the regime becomes less democratic, because power is less dispersed among the various groups in society. Furthermore, if the various sources of power become interdependent, which is the definition of internal embeddedness, one of the core principles of modern democracy might be violated. That happens if the intertwined sources come from different branches that should be independent, either due to a constitutional rule or based on widely accepted convention. This leads to the logical conclusion that in a functioning (liberal) democracy, attempts to consolidate power are unsuccessful.

What is more, one could argue that even the occurrence of unsuccessful consolidation attempts would signal the deterioration of the quality of democracy, because actors who have an aim to undermine the democratic framework should be prevented from getting into a position of power by the democratic institutions before they could even attempt consolidation. Therefore, democracy seems to be incompatible with the consolidation of power: either power cannot be consolidated in a regime or it is not fully democratic.

I would argue that this statement is not entirely correct and power can be consolidated in any political regime. As the various types of power consolidation were presented above, the examples were from liberal democracies, authoritarian systems and hybrid regimes alike, proving that it is possible to consolidate power (in different ways) in all of them. No scholar would question that Sweden, Japan, the United States and Germany are among the most advanced liberal democracies in the world, and yet this type of consolidation could occur in these countries, meaning that it is compatible with democracy. While power consolidation can be a threat to the democratic system, it can exist without endangering it, and in certain cases can be an integral part of democratization itself. In the sections below, I will discuss the relationship between democracy and each type of power consolidation.

1.11.1 Negative consolidation and democracy

The consolidation of power is especially congruent with democracy in the case of the competence-based preservation of power, the subtype based on the "unlikely to break down" definition presented in Section 1.7.1. This form of negative consolidation is a perfectly acceptable process in a functioning liberal democracy, where the governing party is creating an environment where "its power is unlikely to break down", i.e. it is unlikely that it loses an election. As the consolidation is strictly negative, power does not extend to new, previously unavailable domains. Therefore, power relations that are a result of a democratic process remain unaltered throughout consolidation.

These long-ruling parties are generally called dominant or predominant parties (e.g. Key [1949], Blondel [1968: 168], Sartori [2005: 172-193]). The literature of dominant parties does not offer a consensus whether this type is compatible with democracy or not. Schlemmer (2006: 116) believes that dominance contradicts the basic tenets of democracy, and Golder (2000: 104) likewise suggests that democracy is based on the alteration of power, if one actor possesses power for a lengthy period of time, then that political system cannot be a democracy by definition. Bogaards (2005: 32) offers a softer version of this indictment, arguing that the emergence of dominant parties necessarily erode democratic quality, but it is still possible that dominance could come about in a lower quality version of democracy.

On the other hand, Pempel (1990) argues that dominance, i.e. long-term successful negative power consolidation is a rare, but sometimes beneficial feature of even liberal democracies. He cites the decades-long dominance of the Social Democrats in Sweden, the Liberal Democrats in Japan, the Labor Party in Israel and the Christian Democrats in Italy as examples where dominant parties emerged without harming the quality of democracy. This means that negative consolidation is indeed compatible with democracy, even though it is, to cite the title of Pempel's volume, an 'uncommon' version of democratic regimes.

Furthermore, Arian and Barnes (1974) argue that dominant parties can in certain situation improve the quality of a democracy, especially in the case of young democracies, but also in developed, consolidated ones. If the dominant party is committed to democratic principles, its stable position is positive for democracy in that country.

To sum it up, there is a lively debate in the literature whether negative consolidation is compatible with democracy or not, but is certainly possible in some accounts. I consider negative power consolidation to be potentially consistent with democratic principles, even though it might still harm its quality.

This subtype of negative consolidation can be achieved in a number of different ways. As mentioned earlier, effective governance is one tool that could lead to it. Another example would be the regular redistricting and reevaluation of the electoral system that inevitably happens in every democracy periodically due to the ever-changing distribution of the population (the number of eligible voters within a district might change, etc.). The governing parties usually have an outsized influence in the process of this reevaluation in even the most advanced democratic regimes. Independent commissions setting up electoral rules are in the world and the ones that exist have a limited purview, with all their competences based on decisions made by the legislature, ensuring the indirect influence of the governing parties. As a result, electoral rules are generally designed to favor the perpetuation of the *status quo*, and are often used to ensure negative consolidation, and because this process is inevitable, it is not considered to be a major threat to democratic quality unless the new electoral rules become extreme. Moreover, redistricting generally fosters democracy, as it is the only way to ensure that every vote is equal. The example of electoral redistricting shows that as long as negative consolidation attempts do not include hindering opponents, they can be compatible with democracy. The way the shaping of electoral rules is related to the consolidation is power will be discussed in depth in Part 2 of this dissertation.

Furthermore, the opportunity to preserve power is one of the main reasons effective governance can be expected from governments in democratic countries. As described in Section 1.7.1, the aspiration to gain reelection is supposed to lead to responsiveness on behalf of democratic leaders that need to meet the requirements of the electorate to have a reasonable chance of remaining in office. Should negative consolidation be considered absolutely incompatible with democracy, there would be no incentive for governments to not be rent-seeking, and more rigorous regulations would need to be enforced to prevent them from behavior that is not to the benefit of society. Extremely restrictive term limits could institutionally eliminate this form of negative consolidation, but would necessitate the introduction of procedures that would likely be costlier to enforce than the incentive of potential reelection. The opportunity to negatively consolidate power can thus be an integral part of liberal democracy and can improve its quality.

On the other hand, the other subtype of negative consolidation, i.e. limiting competition is completely irreconcilable with democratic principles. Institutionally or otherwise barring other actors to access certain bases of power does not allow for the plurality necessary to cultivate a democracy in high quality, and can only exist in non-democratic regimes. However, it does not make a political system authoritarian automatically: hybrid regimes can be considered imperfect democracies where certain spheres are governed in authoritarian way, i.e. the power of an actor or several actors is negatively consolidated there through institutional means. That could allow for other areas of power that are ruled in accordance with the principles of liberal democracy, but the regime cannot be called democratic on the whole in this case, because these very principles are violated in specific areas of policy-making.

1.11.2 Neutral consolidation and democracy

Just like negative consolidation, neutral consolidation does not increase the power of the consolidating actor and does not extend it to new domains, and is therefore not in direct contradiction with democratic principles. Democratically gained power can be organized and adapted without violating the rules of democracy; in a sense, that is what is expected out of the wielders of power. Static and inflexible behavior is not considered to be a trait of a successful democratic political actor, and it also demonstrates a lack of responsiveness to changing preferences by the electorate and to the developments in the external environment. Therefore, to a certain degree, neutral consolidation of power is an expectation in liberal democracy and can be achieved without breaching democratic norms. However, there are differences between the various types with regards to compatibility with democracy.

Internal embeddedness can be a natural part of democracy as well. Harmonizing already existing sources of power can simply be a sign of effective functioning on the part of the actor in question, and could merely be used as a tool to prevent chaos within the ranks of the consolidating actor. After all, if the various people representing the different bases of power for a group (e.g. MPs for a particular party) do not act in concert, the group will not be able to have the actual power it could possess based on the potential power under its control, eventually leading to the crumbling of the power sources itself. Therefore, a certain level of internal embeddedness is necessary even in the short run, not to mention to possibly retain long-term power. However, there are certain sources of power that are barred from being intertwined, either by constitutional statutes or widely accepted convention. For example, the legally separated branches of power should not be internally embedded in a liberal democracy, e.g. members of the judiciary should not base their decisions on the interests of the political actors who appointed them or who could appoint them to more prestigious positions. Therefore, while internal embeddedness could violate democratic principles, a certain level of it is essential to effective governance even in a liberal democracy.

Socio-economic embeddedness is generally expected of the wielders of power: providing services and creating regulations that make certain social or economic groups satisfied is the very essence of democracy, according to pluralist theory. The actor that can appease the groups with largest influence on the outcome of elections will be the governing party, meaning that socio-economic embeddedness is almost a prerequisite of reelection. Satisfying the needs of certain groups and creating a prosperous general socio-economic environment is thus a crucial part of liberal democracy. However, when certain groups and individuals receive undue privileges, or in the extreme, merge the political and economic spheres as described in Section 1.9.2, embedding power in the socio-economic environment becomes intrinsically undemocratic.

Embedding power in the international environment can also happen both in democratic and undemocratic ways. Ever since Kant (1795), it is a widely accepted claim that democratic countries tend to help each other against undemocratic forces, meaning that international embeddedness is crucial to the democratic forces in order for democracy to survive, especially when the regime is young, unconsolidated and fragile. Furthermore, struggling democracies can be supervised by 'democratic clubs', and membership and endorsement from these clubs could be the incentive for governments to not violate the principles of liberal democracies.

On the other hand, embedding power internationally can have the opposite effect as well: for example, when undemocratic governments use international assistance to crush democratic forces, international embeddedness is the reason for the inability of democracy to flourish. In many cases, it is precisely the international environment that allows the existing authoritarian or hybrid regime to remain in power, as the interests of other countries are best served by a nondemocratic government. This demonstrates that internationally embedding power is vital for both democratic and nondemocratic actors, and it depends on the nature of the international environment whether it hurts or fosters democracy.

Embedding power in civil society is sometimes a part of regular democratic decision-making. For example, when making a certain legislation, governments often consult with the relevant non-governmental organizations about the exact direction the new measures should go, what are the needs, grievances and experiences of the affected communities are, etc. This could be a regulated, systematic cooperation that ensures that the NGOs have an influence and improve the credibility of the government as well as potentially solidifying the support of voters that trust in the particular organizations involved. The organizations might also be a channel of communication between the government and the electorate, ensuring responsiveness on the part of the government and making political decisions easier to understand for the public. Additionally, compromising with organizations with opposing viewpoints can also be a natural process in a pluralist system. This could be healthy for democracies, and in certain cases is expected from governments. To a certain extent, neocorporatism is an institutionalized form of both socio-economic embeddedness and embeddedness in civil society.

Contrarily, if antagonistic elements of civil society are hindered, democracy cannot function in an ideal way, as diverse civil society is conducive to creating a democratic environment of high quality. Furthermore, when embeddedness reaches the level that certain groups in civil society receive undue privileges, democratic principles are violated. Hence, this type of embeddedness can be achieved in both a democratic and an undemocratic manner.

1.11.3 Positive consolidation and democracy

The most problematic type of power consolidation with regards to democratic quality is positive consolidation. Extending or deepening power by definition alters the established power relations in the political structure, and can easily violate democratic principles: even if it does not destroy democracy completely, it erodes its quality in most cases. The very idea of a democratic institutional system is to check the power of the various actors, not allowing it to grow over a certain threshold. These are generally the examples colloquially referred to as the consolidation of power.

Nevertheless, it is conceivable that in certain situations, positive consolidation does not decrease the quality of a democracy. If power is increased, but does not cross the threshold that it is not allowed to go over, it can be a natural part of the democratic process. For instance, if a party only controls one policy area in one government, but through shrewd political moves and/or increased public support forces its way to control other areas as well, or even form a one-party government, it is not necessarily undemocratic. Changes in the power structure are regular and are an essential part of a democratic system, and there bound to be winners in the long run – winners who successfully consolidated their power positively. It is a fine line that needs to be drawn when the threshold that determines whether positive consolidation can be considered within democratic norms or not, and it is largely based on convention and differs from one political culture to another, and therefore needs to be evaluated on a case-by-case basis.

Moreover, positive power consolidation could even ensure the efficacy of the democratic system and might be an indispensable part of democratization. Checks and balances can in certain situations stifle decision-making, creating a gridlock that none of the actors prefer, denying the opportunity for a government to deliver on its promises and the will of the electorate to be carried out. In most cases, this gridlock is a feature of the system and is designed to protect vulnerable minorities from the tyranny of the majority, but in some cases, all parties are hurt and dissatisfied by it. One historical example would be the institution of *liberum veto* (any member of the legislature could veto any legislation) in early modern Poland, which paralyzed the country and resulted in the weakening of the state as it failed to adapt to the changing circumstances and ended up being overrun by surrounding, less democratic empires (Calhoun, 1992: 53-54). In these rare cases, altering democratic institutions in a way that increases the power of certain actors can be a solution that ensures the long-term viability of the democratic system and prevents the inevitable dissatisfaction with democratic decision-making that can lead to the breakdown of democracy.

Greene (2009) argues that dominant parties (i.e. negative consolidation of power) emerge when there is a public need to expand the capabilities of the state. Certain issues in certain eras require public action, which requires the expansion of the state, which in turn often results in positive power consolidation (the extension of power to be precise) for the government. In other situations, the shrinking of the state is considered to be the most prudent way to handle economic issues, which works against power consolidation. Thus, positive consolidation could simply be a result of economic necessities and does not damage the quality of a democracy.

For example, in the 1960s and the 1970s, the general trend in Western democracies was that public services were extended to more and more groups and in more and more areas. For instance, in the US, the Great Society program was launched. At the same time, many Western democracies (e.g. Italy, Sweden, France, West Germany) had a dominant party that consolidated its power through these extensions. Conversely, when this trend was reversed in the 1980s, when neoliberalism, spearheaded by Margaret Thatcher and Ronald Reagan, emerged as the preeminent ideology to organize Western societies, dominant parties became a lot less common in the West (Greene, 2009: 5). Therefore, positive consolidation can indeed be a part of democracy if the economic situation is conducive to it, and it is sometimes undemocratic *not* to consolidate power positively, since the needs of the people require the expansion of state capacities, and that naturally brings about positive consolidation.

This correlation also explains why authors in the 1970s were much less condemning about the various forms of power consolidation than they were in the 1990s. For example, most of the literature on dominant parties in the 1960s and 1970s described them as a regular form of a democratic regimes, then in the 1990s, dominance became an antonym of democracy (Rajnai, 2021: 42). Hence, in this case the trends in political science mirror the trends in politics.

1.11.4 Power consolidation and democratic consolidation

This is the point where the relationship between democratic consolidation and the consolidation of power needs to be examined. In most cases, these two concepts describe processes that pull the system as a whole in opposite directions: the consolidation of power generally decreases democratic quality, while democratic consolidation by definition increases or stabilizes it.

Nevertheless, negative, neutral and positive power consolidation could in certain cases contribute to democratic consolidation, especially when it occurs in fragile, young democracies, where nondemocratic actors maintain control over certain domains of politics, posing a threat to the survival of democracy. This is often the case in the sphere military, where authoritarian decision-making is more widely accepted than in most other policy areas. These nondemocratic elements put democracy at a constant risk, as the danger of a coup cannot be ruled out and even if it never occurs, the simple possibility of it might compromise democratic decision-making. Latin America provides numerous examples of anti-democratic elements in the military threatening democracy (Lehoucq and Perez Linán, 2014: 1110). In such a case, extending the power of democratic actors to the domains controlled by nondemocratic ones can be considered a positive development for democracy, and this is true until all "disloyal players" are "eliminated, neutralized or converted" (Schedler, 1998: 96).

Early democratization and the establishment of democratic institutions is thus naturally a consolidation of power by democratic actors. However, these consolidation attempts are generally not aimed to increase the power of a specific actor, but to decrease the influence of a particular player and extend the realm of democratic decision-making. Ideally, they would not benefit a single group of actors that initially acquire an elevated level of influence as a result, but prove to be beneficial for all actors committed to democracy. But as they increase the power of specific actors beyond one electoral cycle, they fit my definition of positive power consolidation, and hence should be treated as such.

Andreas Schedler (1998: 96) considers "eliminating, neutralizing or converting disloyal players" a "primary task" of democratic consolidation, while the emphasis in power consolidation is also on eliminating, weakening and hindering opponents – who could very well be proponents of democracy. In the case of democratic consolidation, disloyal players are actors whose aim is to restore the authoritarian regime, to establish a new one, or to erode the quality of democracy. This means that one of the key elements of negative democratic consolidation is the neutralization of the actors who are attempting to achieve the anti-democratic version of the consolidation of power. This is not true *vice versa*: the aim of power consolidation is not the reversal of democratic consolidation, it is merely a means to preserve or gain more power; consolidating actors generally are only concerned about regime types insofar as to how they affect their own power. If retaining democracy is conducive to them preserving, extending or deepening power, they will be motivated to foster democratic consolidation.

Arian and Barnes (1974: 593) argue that in undeveloped countries, it is impossible to create a democracy without a strong party that is committed to establishing and strengthening the institutions of democracy from above, otherwise the previously mentioned “disloyal” actors will certainly undermine it. Giliomee and Simkins (1999: 342) add that the dominant party is necessary not only to create the institutional setting of a democracy, but also to reinforce democratic values among the public and to improve the economy to a level that makes the widening of the middle class possible. They consider the cultural and economic development a prerequisite of creating a stable democracy, and consequently, in their view the consolidation of power by the party that establishes democracy is a natural part of democratic consolidation.

As proven by examples in this chapter, democratic consolidation should not aim to prevent all possible forms of power consolidation, as power can be consolidated in a ways that compatible with (liberal) democracy. What is more, certain forms of it could be necessary to sustaining or establishing democracy, while other types describe phenomena that are a natural part of democracy (such as incumbents intending to retain their position, prompting them to be responsive). Therefore, it is important to identify in each case which types of power consolidation are compatible with democracy and which are not, instead of labeling each actor attempting the consolidation of power a threat to democracy.

Political culture can be crucial in determining whether a specific form of power consolidation fosters democracy or damages it. Every political act has its own cultural context, and the same step could be undemocratic in one setting and conducive to democracy in another one. This is why general claims of whether the consolidation of power or even a type of it is democratic are not helpful, the circumstances matter in each case. This chapter outlined some of the factors that should be examined when the impact of power consolidation on democracy is evaluated.

Nevertheless, the goals of power consolidation and democratic consolidation are quite different and are potentially conflicting. Democratic consolidation is generally about dispersing power among more and more actors within society, while power consolidation is aimed at concentrating power under the control of one specific actor. This prompts me to hypothesize that despite the exemptions I laid out above, it can be generally accepted the more consolidated a democracy is, the less probable it is that power can be successfully consolidated in it by any actor. Furthermore, it could also be presumed that as a democratic regime becomes more consolidated, the fewer genuine attempts of power consolidation we can expect from actors operating within the confines of that regime. These hypotheses could be useful when analyzing the various tools of the consolidation of power empirically, which is the main objective of Part 2 of this dissertation.

1.12 Conclusion

Part 1 of this dissertation introduced the consolidation of power as a new concept to be used in political science and as the theoretical background of Part 2. While the phenomena described by this concept and other notions akin to it have been dealt with extensively by various fields in the social sciences and by political science in particular, it has not been comprehensively defined before in the form that I understand it.

I first presented the well-established concept of democratic consolidation and the pluralist theoretical framework that I use for the definition of power. Then, based on the understanding of consolidation and power within the literature of political science, I defined the consolidation of power as a phenomenon where a political actor possessing power uses said power to increase or preserve its influence in the long run. After a brief overview of existing concepts akin to the consolidation of power in the social science literature, I created the typology of power consolidation with the aid of the well-established literature and classification of democratic consolidation, drawing on the similarity of the two concepts. The typology is summarized in Table 1. Finally, the relationship between democracy and the consolidation of power was analyzed in Chapter 1.11.

In addition to the novel theoretical insights Part 1 offered, this conceptual framework will help conduct the analysis in Part 2 and will provide the basis for most of the hypotheses proposed there. This will demonstrate the practical applicability of my understanding of the consolidation of power for empirical analysis of a wide range of political phenomena that are rarely analyzed together or compared to each other, despite them essentially being different tools to reach one specific aim, which I identify as the consolidation of power.

Part 2 will only show one example of this kind of analysis, but the efficacy an endless number of tools could be examined in order to find out whether they really lead to increased or stabilized power for the actors that initiate it or not. Too often, due to the relatively long time that could pass between the initiation of a consolidation attempt and the point when its results become apparent, attempts are considered effective on the one hand and harmful for democracy on the other. My framework allows for the careful analysis of the effectiveness of various consolidation tools to test the first assumption, and the typology can help in determining whether the second assumption is correct or not in a specific case. Part 2 is thus a mere precursor of what the conceptual framework of Part 1 could be used for.

Table 1: Comparison of the various types of power consolidation

Types	Negative consolidation		Positive consolidation		Neutral consolidation/embedding power			
Subtypes	Competence -based preservation of power	Limiting competition	Deepening power	Extending power	Embedding power internally	Embedding power in the socio-economic environment	Embedding power inter-nationally	Embedding power in civil society
Aim	Retaining power the consolidating actor already possesses		Increasing power		Preparation for positive or negative consolidation; organizing, adapting, stabilizing power			
Tools	Responsive behavior, good governance	Hindering opponents	Increasing power within a domain the consolidating actor already influences	Influencing a domain in which the consolidating actor previously did not possess any power	Making bases of power inter-dependent	Integrating social and economic groups into the power structure	Gaining the support of inter-national actors	Impairing or compromising with antagonistic parts of civil society; integrating supportive elements of civil society into the power structure

PART 2

THE EFFECTIVENESS OF ELECTORAL ENGINEERING AS AN INSTRUMENT OF POWER CONSOLIDATION

2.1 Introduction

2.1.1 The necessity of empirical analysis

As laid out in Part 1, power can be consolidated in numerous different manners using various tools. In Part 2, I set out to test the actual effectiveness of one of these potential tools of consolidation, namely reforming the electoral system for expected partisan gain.

The aim of this undertaking is to demonstrate in practice what I already described in theory. Part 1 offered examples illustrating the theoretical distinctions I was making, but none of those examples were analyzed in depth, nor could it have been ascertained whether those examples represent a typical case of a specific type of consolidation or if they are extreme occurrences of this phenomenon. The definition and typology in Part 1 can be useful to interpret political phenomena observed in real life, but without extensive empirical analysis, it is difficult to say whether a certain type of power consolidation is achieved relatively frequently or if it is a rare occurrence in practice. For example, it could be possible that positive consolidation is something that is rarely achieved successfully, whereas negative consolidation could be a phenomenon that is common in politics, or the other way around. Part 2 should contribute to gaining more knowledge about these practical matters and therefore a better understanding of power consolidation in general.

In addition to the academic relevance, these insights could be useful for both those aiming to achieve power consolidation and those striving to prevent it. Distinguishing types of consolidation that are realistic and unrealistic can shift their focus on the types that are genuine opportunities or threats and disregard improbable attempts at consolidation.

Empirical analysis can also shed light on the circumstances that aid specific types of successful consolidation. For example, certain types of consolidation could be more prevalent in certain regions, time periods, regime types, etc. Identifying the key attributes that foster specific types of consolidation and those features that hinder it could also narrow the focus of those analyzing, attempting or preventing the consolidation of power.

Even more important than determining the practical applicability of the various types is uncovering the same with regards to the different tools of power consolidation. Typology is a larger concern for those studying the phenomena that constitute the consolidation of power, but the efficacy of specific tools is sure to be on the mind of the actual actors attempting to consolidate power and those trying to impede consolidation as well as that of external observers. Choosing to use a specific means of consolidation depends on both the availability of that instrument, the perceived ability of the consolidating actor to utilize it and the overall likelihood of success of that particular tool.

Part 2 will be useful in determining the latter in the case of one particular tool, offering a blueprint for the evaluation of other tools in the future. Understanding that a specific means of consolidation does not work in practice could cause actors aiming for consolidation to refrain from using it and opting for alternative methods, while opponents of consolidation could more comfortably disregard consolidation attempts that are overall unlikely to prevail; focusing resources on those instruments of power consolidation that have through empirical analysis been proved to be effective in general.

In addition to the general practicality of a certain tool of power consolidation, the circumstances under which it is more likely to succeed could prove to be of interest for students, proponents and opponents of power consolidation alike. A specific instrument could be effective in one environment and could be inefficient in another, meaning that some actors could be inclined to use it, whereas others could opt for alternatives. Thus, understanding what kind of environment is favorable to the effectiveness of a specific consolidation tool could be crucial, and I aim to add to that knowledge in Part 2.

Nevertheless, this dissertation only allows me to examine one tool of power consolidation in practice. It is not going to be a general evaluation of consolidation and hence not a finished endeavor, but only a beginning that offers a blueprint for further research into other tools. One of the main objectives of Part 1 was to create a concept that is suitable for empirical analysis, and Part 2 intends to show that it can indeed be done, and that the consolidation of power is an adequate framework for doing so. Continuing on this road beyond this dissertation will be important to attain a fuller grasp of what the consolidation of power looks like in practice, but Part 2 will serve as the introduction to this series of empirical analysis. As I intend this examination to be a blueprint for further research, selecting the right tool to analyze first is crucial. I will explain my reasons for selecting electoral engineering for this pioneering analysis in Chapter 2.2.

2.1.2 The structure of Part 2

Part 2 will be structured as follows. In Chapter 2.2, I will lay out why analyzing electoral engineering should provide to be fruitful subject for gaining a more general understanding of how the consolidation of power works in practice and elaborate on the relation between the two. In Chapter 2.3, I will summarize the literature of electoral engineering and how it relates to evaluating the effectiveness of power consolidation. In Chapter 2.4, I will posit hypotheses that allow me to evaluate the effectiveness of electoral engineering for consolidating power. Afterwards, in Chapter 2.5, I will lay out the operationalization, the data and the methods I will use to test those hypotheses. This will be followed by a summary and interpretation of the results of the analysis in Chapter 2.6. Finally, I will sum up my findings, provide potential explanations for the patterns observed, and offer ideas for potential further research in Chapter 2.7.

2.2 Selecting electoral engineering as the subject of the empirical analysis

2.2.1 Criteria for the type of tool that is useful and practical to analyze

Uncovering the effectiveness of various types and tools of power consolidation is thus an important challenge for both academic and practical reasons alike. Nevertheless, as there are countless different tools of power consolidation, I cannot attempt to gain a comprehensive understanding of the efficacy of all of them or even sampling the menu of consolidation instruments in one dissertation. What I can endeavor to achieve though is examining one tool that is prevalent, can be used in multiple types of power consolidation and the effectiveness of which can be reasonably ascertained through empirical analysis. That would be representative of the type of inquiry the consolidation of power requires, and would thus be a great precursor of further research in addition to having merit on its own.

This would contribute to the general evaluation of the efficacy of the consolidation power in several different ways. Obviously, the assessment of that particular tool can provide to be useful when that instrument is used or is being considered to be utilized, and if it is a widely-used one, the results could be applied in a multitude of cases. Additionally, the analysis could serve as a prototype for later investigations that complement it and could consequently further the research of this particular area of political science.

Furthermore, if the tool to be chosen for the analysis is applied under a diverse set of different circumstances and in pursuit of various types of power consolidation, it can provide valuable insight as to what kind of environments are favorable to consolidation and what types of consolidation are more likely to be achieved than others. Naturally, this insight would not be comprehensive, but it could hint at larger trends and could inform the design of the analysis of other consolidation instruments that are to be conducted in further research.

Hence, the tool I will select needs to have been used for a long time, comparatively often, in a wide variety of settings with different political parameters, and with the aim of achieving multiple different types of power consolidation. At the same time, it should be reasonably feasible to assess the effectiveness of the selected consolidation instrument via empirical analysis. These are the criteria I will keep in mind when selecting the subject of the analysis in Part 2 of this dissertation. This will allow me to produce results that are not only relevant in assessing the phenomenon that I analyze, but could contribute to a broader understanding of the dynamics of the consolidation of power as well.

2.2.2 Electoral engineering as a tool that meets these criteria

I chose electoral reform for expected partisan gain, also known as electoral engineering, as the subject of Part 2. I will provide an overview of the literature of this topic in Chapter 2.3, but before turning to that, I will present why I chose this particular tool for deeper analysis; i.e. how it meets the criteria described in the previous section.

Firstly, electoral engineering is suitable for empirical analysis because it is a change in institutions, and as such is a visible, identifiable and interpretable phenomenon. The features of an electoral system are publicly known, and changes to its attributes are also relatively easy to identify: if the characteristics for one election were different from the one before, the system was modified between the two elections. The degree of change can be determined and classified as well.

The fact that electoral reform is an institutional change clarifies when the consolidation attempt took place. There is a clear watershed moment when the system is altered that can be looked at as the point where consolidation was attempted. In the case of non-institutional changes, observing and analyzing the effects of the attempt becomes more difficult as there is no evident juncture to identify as the point when consolidation took place, and as a result, consolidation can be more gradual.

The direct aim of electoral engineering is also relatively narrow: improving electoral performance. There are various indirect consequences of electoral engineering, but the direct goal that the consolidating actor has in mind is clear and relatively easy to measure. Moreover, electoral results can be compared to performance in alternative systems, including the previous one that the actor just altered. This is not true in the case of most of the other tools of power consolidation, where the aims can be wide-ranging and interrelated, making the evaluation of the efficacy of those instruments much more difficult. In the case of electoral engineering, the criterion of success is relatively straightforward: did the new electoral system aid the electoral performance of the consolidating actor or not? Determining the threshold of success in this case has its own issues, but compared to the aim of other tools, electoral performance is a constrained and measurable object appropriate for a comparative analysis, making the evaluation of effectiveness a feasible task in the case of electoral engineering.

Not only is electoral reform for partisan gain a tool that is suitable for empirical analysis from the perspective of power consolidation, it is an extremely common tool as well. It dates back to at least the early 19th century, when the term ‘gerrymandering’ originated, denoting the process of redrawing the boundaries of electoral districts to maximize partisan electoral performance (Martis, 2008). There is no such thing as a ‘neutral’ electoral system, i.e. the choice of the system always favors certain actors and is disadvantageous for others. This always incentivized political actors to potentially use electoral reform as a means to consolidate power, as electoral results serve as the most important source of power in modern democracies. Hence, this tool has a long history; it is essentially as old as democratic elections are. This ensures the historical depth I was looking for in the tool of consolidation that is appropriate to be subject of empirical analysis.

Furthermore, electoral systems cannot stay perfectly constant for a long time due to changes in the structure of the electorate and the development of the political system, making electoral reform a periodic necessity in most cases. That makes electoral reforms prevalent and widely practiced, providing ample data points for a comparative investigation. Moreover, the prevalence is not limited to certain types of political regimes: established democracies are just as prone to it as young, unconsolidated ones, and electoral engineering can be observed in virtually every region of the world where democratic (or semi-democratic and even non-democratic) elections are held. That adds geographical and cultural diversity to the available data in addition to the historical variety, and this diversity and the multitude of cases ensured by the prevalence of this consolidation instrument are all critical to the viability of the analysis conducted in Part 2.

Electoral engineering is thus suitable for a comparative empirical investigation, due to both its nature and its wide-ranging use. The effectiveness of it can be tested due to the large number of diverse cases that can be analyzed, and due to the comparatively constrained aim and easily observable nature of electoral reform. That leaves one criterion that electoral engineering still needs to meet in order to be applicable for the empirical analysis, namely that is should be used to achieve multiple types of power consolidation. Before I turn to that, I will need to establish precisely how electoral engineering can serve as a means to consolidate power, which I will do in the following section.

2.2.3 Electoral engineering as a tool of power consolidation

Electoral reforms are by nature processes where bias play a part, and as the electoral systems designed during them can influence electoral results in a substantial way, and those results in turn have an enormous effect on power relations, it is not difficult to see how they can be used by an actor to preserve or increase power. Hence, the consolidation of power is surely in play when analyzing electoral reforms, and the shaping of electoral rules is often under scrutiny for the very reason that it is considered an effective means to consolidate power. The exact way in which I define electoral engineering as a consolidation instrument will be formulated in this section.

As for the time horizon required by my definition of power consolidation, electoral engineering is by definition concerned with long-term influence. The power relations at the time of proposal or enactment are unaffected by the changes, but the power relations after the next elections are altered significantly. Since I defined the long run as anything happening after the elections after the consolidating acts were carried out, electoral engineering can only be considered as an activity with a long-term focus. Naturally, the reform process could have short-term consequences (e.g. backlash from the public for initiating reforms that were uncalled for), but these are not the main concern of the consolidating actor. The main goal of electoral engineering is to affect the results of the upcoming elections and other elections to be held later, and that by definition matches the time horizon required for power consolidation.

The consolidating actor is one with power that uses its leverage in an attempt to increase or preserve long-term power. In this case, the actors need to have an influence in the design of the new system. These are almost always the political actors who possess legislative power, without their contribution, no electoral reform has been passed (Renwick, 2010: 16). Their influence is in most cases direct: they control the mechanisms that can enact changes in the electoral system, and in the vast majority of cases, the legislature needs to explicitly approve the modifications.

Other actors, such as members of the judiciary, activist and pressure groups, or international players can also be indirectly influential, as well as trends in public opinion (Renwick, 2018: 119), but they can in almost all cases only indirectly influence the process. Furthermore, the mechanisms themselves are generally established in the legislature, which means that even if other actors are allotted direct influence (e.g. an independent committee is established to design the rules, experts are consulted, the judicial branch is granted veto power, etc.), that is indirectly given by the legislature, and can always be rescinded, providing indirect influence to legislators in this case.

Consequently, it is fair to assume that the consolidating actors are the parties in control of the legislature. Even if the reforms are not initiated by them, they need to approve the changes in order for them to take effect. That means that no reform can be passed against their will, which makes it fair to assume that their interests are at least partially represented in the final outcome. I will elaborate on this when I operationalize my research, but it should suffice here that in the case of electoral engineering, the consolidating actors are the parties controlling the legislature.

These parties are attempting to consolidate their power through the electoral reform process. Their first goal could be negative consolidation: they are already in a position of power and aim to preserve the control they have previously obtained. This could mean that they would like to protect the share of the votes they already possess, or they could aim to simply remain in the majority, which could include a loss of a certain amount of seats. This could be achieved through either or both subtypes of negative consolidation: electoral engineering can happen in accordance with democratic norms or in direct contradiction with them.

Another goal of consolidation through electoral engineering could be to increase influence by creating a system that favors the ruling parties more than the previous one did. That could add to their power without gaining additional votes – the same share of votes could result in a larger share of seats, benefiting the consolidating actor. This would be an example of positive consolidation, and deepening power in particular. The actors are not trying to extend their influence over domains they previously have not controlled, but are attempting to increase their authority in an area they have already been powerful in: the legislature. Indirectly, a firmer grip on the procedures that allow for laws to be passed or rejected could lead to an extension of power, but the direct and explicit goal of electoral engineering is either negative consolidation or deepening power.

This means that this tool satisfies the criteria of being able to be utilized to achieve multiple types of power consolidation, which is rare. It would be even better if all three different types of consolidation could by analyzed through one tool, but this is almost impossible, and cannot be done in the case of electoral reforms. neutral consolidation can only be an indirect consequence of electoral engineering. A more favorable electoral system does not make the various power bases of an actor more interdependent, it does not change the social or the economic environment, and it is unlikely to gain the support of relevant international actors or constituents of civil society. The latter is not impossible though: maybe the new system is not only beneficial for the consolidating actor but also converges with norms promoted by international actors or specific influential groups of civil society. This however can generally only be a result of a benign coincidence; the assumption is that the main aim is not winning the support of these actors (in that case, it cannot really be called electoral engineering), but to create a more advantageous electoral system to preserve or increase legislative power.

As stated above, the consolidating actors are not looking to secure a new base of power when using this tool, but they are concerned with an already existing one – their share of seats in the legislature. However, it could be argued that they are striving to ensure that they do not lose this source of influence in its entirety, and as a result attempting to negatively consolidate a base they already possess. That would mean that the goal of electoral engineering is to prevent losing all seats in parliament: if there is one representative left, the base is still available to the party, but the amount (i.e. the probability of successful exertion of power) is diminished. This is a rare scenario, so in general, it could be said that it is not the base of power actors are attempting to consolidate through electoral engineering.

It is therefore the amount of power that the consolidating actors are focusing on when they are shaping electoral rules. Preserving the majority (negative consolidation) or increasing the seat share (positive consolidation) ensure that the probability of success in the legislature remain high or even increase. That is the aspect of power this tool is concerned with, and it is important to keep in mind when analyzing other consolidation instruments that those could behave differently due to the different aspect of power they are associated with.

Overall, it could be stated that electoral engineering meets both the criteria of an instrument of power consolidation and my criteria for the specific tool that is suitable for the empirical inquiry I am conducting in this part of the dissertation. I will thus proceed by analyzing the phenomenon further by surveying the literature of the subject.

2.3 Electoral engineering

2.3.1 The general importance of electoral engineering

Electoral engineering has long been a central topic of political science. Politicians, observers of politics and political scientists alike have become interested in how different variations of electoral systems affect the outcomes of elections. Each different electoral set-up is beneficial to certain political actors and is disadvantageous for others; there are no completely impartial systems. As a result of this fact, politicians have often attempted to influence the formation of electoral systems so that they would produce desirable results.

While it was considered to be a somewhat neglected and under-researched subfield of the vast area of the study of electoral systems as late as 2005 (Shugart, 2005: 27), it has since been one of the most investigated topics in the field due to interest from both academic and general audiences (Renwick, 2018: 113), and its literature has grown substantially as a result.

Political science has through theoretical arguments and the analysis of existing systems ascertained how the various electoral systems affect results in general (Shugart and Taagepera, 2018; Colomer, 2018) and also in specific cases (e.g. Jacobs, 2018; Zittel, 2018). The evaluation of the political effects of electoral systems, and especially electoral system reforms, has always relied on the assumptions gathered from this strand of political science literature. Since this literature is quite exhaustive, and our knowledge on the impact of electoral systems on electoral results and political consequences is vast, it is often presumed that using the extensive resources provided by this wide-ranging literature, the actors initiating electoral reform will create a system that will be beneficial to them in the future. Even journalists often accuse politicians for pushing electoral reform only when and in the shape it fits their interests[9].

2.3.2 General approaches to the study of electoral engineering

Norris (2004) has provided the first general international overview of electoral engineering in her seminal book *Electoral Engineering: Voting Rules and Political Behavior.* In it, she attempts to merge two schools of political science: rational-choice

[9] For example, the government was accused of insisting on electoral reform for partisan reasons in Poland in 2020: https://www.politico.eu/article/polish-pis-rams-through-electoral-system-changes/ Accessed on March 3, 2021.
On the other hand, in Canada, the two major parties (the Conservatives and the Liberals) are accused of stifling reform in order to entrench themselves in positions of power, and suppress minor parties such as the Greens or the New Democrats: http://www.themanitoban.com/2019/11/electoral-reform-in-canada-is-becoming-a-battle-between-two-unprincipled-parties/38466/ Accessed on March 3, 2021.

institutionalism and cultural modernization. 'The core theoretical claim in rational-choice institutionalism is that formal electoral rules generate important incentives that are capable of shaping and constraining political behavior' (Norris, 2004: 7). The formal institutions themselves will change electoral outcomes 'mechanically', and actors will adjust their behavior due to their expectations of how the rules will affect the results; the latter is the 'psychological' impact of electoral institutions (Duverger, 1954).

Political actors will respond to these incentives because they are seat-maximizers (though they have other considerations as well), and will adapt to the system in their choice of candidates, platforms and allies. Voters will also respond to the consequences of the electoral system as well as to the strategies adopted by the political actors. Therefore, electoral engineering is capable of not only shaping the political arena, but also the behavior of citizens.

On the other hand, cultural modernization theory claims that the shape formal institutions take only reflect the result of informal changes within society, and therefore the focus should be on these informal norms. The political behavior of both politicians and voters is determined by factors such as societal modernization (including economic development and changes in the societal framework), political culture, and the socialization process.

The informal institutions established by these processes shape the behavior of actors in the political arena, and they will be less responsive to changing formal institutions, and even if they adapt to the new rules, they will do so slowly, making short-term shifts in political behavior unlikely. Furthermore, since changes in formal institutions such as electoral rules are mostly a reflection of changes in informal norms; therefore, even if actors seem to respond to the change in formal rules, they are actually following the trends in informal rules (Norris, 2004: 16-17). Observing changes in the formal electoral framework is thus almost always superfluous, and electoral engineering cannot work without more substantial changes in the underlying informal rules of society.

Due to its focus on changes in clearly set formal rules, this dissertation follows the rational-choice institutionalism tradition. Reforms in formal electoral rules are easy to detect and serve as great basis for analysis. Cultural modernization theory should not be forgotten though, the role of informal institutions can indeed be very influential in electoral outcomes and political behavior, and could explain the differences of the effects of electoral reform among different countries or offer alternative explanations if rational-choice institutionalism fails to deliver any.

2.3.3 Non-partisan motivations of electoral reform

Norris and numerous other authors are mostly concerned with normative issues the specific choice of an electoral system could address, such as which systems promote accountable and or efficient governments (e.g. Kam et al, 2020), large turnout and extensive political participation (e.g. Selb, 2009), amelioration or acceleration of political polarization (e.g. Matakos et al, 2016), diverse representation (e.g. Lublin, 2017), etc.

These considerations are often cited as arguments for or against a specific reform, and such normative and ideological motivations can indeed drive the electoral reform process. In this section, I will sample the empirical evidence in support of these motivations playing a role, while also demonstrating how partisan self-interest still played at least a partial role in each of these cases, as generally noted by the authors themselves.

Blais et al. (2005) examine electoral reforms during the interwar period in Western Europe, the first spike in the adoption of proportional systems. They found that these reforms were generally accepted by a wide consensus due to the fact that proportional systems were considered more equitable than the previous majoritarian ones. The failed historical experience of the Weimar Republic shattered this consensus and thus halted the emergence of PR systems after World War II. They conclude that common sense reasoning can drive electoral change more than partisan motivations. However, even according to the results of this study, self-interest was still involved in the enactment of these reforms: the political reality of countries adopting these systems were mostly characterized by confusing electoral results and optimal electoral strategies have not yet been found, thus making proportional systems seem more convenient for political actors due to their simplicity and predictability.

Simplicity and predictability for the political actors (candidates and parties), that is. Proportional systems are often more complicated and more difficult to understand for the public than majoritarian ones are, but for politicians, predicting the future composition of a legislature based on polling data is easier in a proportional system with large district magnitude than in a majoritarian one, where seat share could be very different from vote share, and therefore polling needs to be extremely accurate to forecast results. Candidates know that their seat is safe if they are high up on a list of a mid-to-large party, they do not have to worry about potentially losing in their district. Furthermore, the relative power of parties does not shift dramatically if their popularity increases or decreases incrementally, but a couple of percentage points in a couple of districts could make all the difference in a majoritarian system. This is the sort of complexity that reformers intended to reduce by switching to proportional systems according to Blais et al. (2005).

Bol et al. (2015) argue that international patterns are among the main catalysts of electoral change. Analyzing post-war European electoral systems, they conclude that certain directions of electoral change usually spread across several countries within a limited time period, i.e. there are 'fashionable' and ill-favored system characteristics at any given time. Positive or negative experience in other countries, as well as potential pressure from the international environment can accelerate the reform process, which therefore should not be examined in isolation, but in international comparison. However, the authors emphasize that international spillover only supplements the major motivation of electoral reforms: the seat-maximizing intent of political actors (Bol et al, 2015: 401).

Norris (2011) argues that based on survey data from New Zealand, electoral reform can be motivated by the 'democratic aspiration' of the electorate. The idea that majoritarian systems are not equitable and do not foster fair representation became prevalent in the country by the early 1990s, compelling politicians to change the system to a mixed-member proportional system. However, as Lamare and Vowles (1996) point out, public opinion was not unanimous by any means; partisan allegiances and ideological values that can be tied to party preference can help explain why certain voters favored the change and others did not. Furthermore, they argue that party interests did in fact play a significant role in the shaping and adopting of the new electoral rules. They conclude that public opinion can only be a partial impetus for electoral reform, and since public opinion is significantly shaped by political actors, the self-interest of politicians cannot be ignored even in cases where public opinion seems to be a major driving factor of electoral change.

All in all, while empirical evidence suggests that there could be numerous different motivations and reasons for electoral reform, these generally only supplement or mask the partisan interests that play the most significant role in electoral engineering. Therefore, I focus on partisan motivations in my analysis, and in the following section, I turn to the overview of the literature that analyzes partisan motivations of electoral reforms.

2.3.4 Partisan motivations of electoral engineering

As described in the previous section, while there could be a myriad different reasons driving electoral reform other than the self-interest of the parties in control of the legislature, these generally only complement the underlying seat-maximizing intent of political actors. As Benoit (2004: 374) aptly put it: "A change in electoral institutions will occur when a political party or coalition of political parties supports an alternative which will bring it more seats than the status quo electoral system". The flipside of this claim is formulated by Shugart (2008: 14): "If the existing system is performing poorly by some 'objective' criteria, yet the party in power prefers to keep the system, there is no reform". As this is the general consensus of the field, and it also fits my theoretical framework of actors consolidating their power through electoral engineering, I will accept this as a basic assumption of my empirical investigations.

Accepting that the primary motivation of electoral system change is the desire of political actors to maximize their power does not resolve all complications of selecting an approach to analyze electoral engineering. Renwick (2018: 120-121) points out several dilemmas within the power-maximizing assumption strand of the literature.

The first dilemma is concerned with who the power-maximizing reformers exactly are. Are the actors initiating reform individual members of parliament who seek reelection and increased personal influence or parties that expect collective payoffs from electoral change? I answer this question clearly by choosing to use parties as the main actors at play. On the one hand, this is due to the fact that I work in the pluralist tradition, which generally assigns power to groups rather than individuals. On the other hand, MPs alone cannot pass legislation: they need the support of several of their peers to enact changes, and it is therefore prudent to assume that the collective priorities of the parties that are most prominent in designing electoral reform.

This brings up potential issues of collective action (Olson, 1965), insofar as the goals of individual members can deviate from that of the party as a whole, and coordinating a large number of representatives can prove to be challenging. Pilet (2008) demonstrates using the case of the Belgian electoral reform of 2001 that the collective nature of parties can help understand their motivations for supporting or opposing a reform. For example, the relationship between backbenchers and party leaders was one of the concerns of the reformers, and surprisingly, intra-party animosity was generally more influential in the design of the reforms than considerations of inter-party competition.

Emmenegger and Petersen (2017) go as far as to argue that large-n cross-sectional analyses of electoral reform cannot find meaningful results due to the complexity of the reform process and the large number of players involved, even within one party. Nevertheless, party discipline has been consistently high both in Westminster democracies (Kam, 2009) and in consociational ones (Castanheira and Noury, 2007), proving that parties tend to manage to aggregate these diverging interests and vote cohesively as a unit in the end. This prompts me to consider parties as singular homogenous actors despite the concerns the collective action problems might raise.

Another important dilemma is the one discussed in depth by Shugart (2008), namely the distinction between ‘outcome contingency’ and ‘act contingency’. Outcome contingency in the case of electoral reform would be the expected mechanical effect of the proposed new system: a party generally advocates for reform if the new system appears to be more beneficial than the current one. This is the type of contingency generally assumed by the rational-choice literature of electoral reform, and my approach focuses on this as well.

However, this is not the only concern for power-maximizing parties when considering electoral change. Act contingency is the amalgamation of the potential consequences from the act of attempting to change the system. Public opinion might be perceived to be in favor of a reform, in that case, a proposal to modify electoral rules could result in a spike of popularity for the party, resulting in improved electoral performance. On the other hand, the electorate could react negatively to a proposed reform, generating electoral backlash that could hurt the electoral performance of the reformer party. Other act contingencies include the psychological effects of the reform, i.e. the adaptation to the new rules by the voters. Due to act contingencies, electoral reform does not only affect how votes are translated into seats, but could also increase or decrease the vote shares of the actors initiating the reform.

Act contingency is more complicated to assess than outcome contingency, and it is vastly different in each case of electoral reform or lack of electoral reform. While it is an important consideration of the electoral reform process, I will not focus on act contingency in my analysis, because it requires in-depth qualitative analysis. Moreover, even in a well-conducted exhaustive case study, evaluating it requires speculation about potential reactions and the intent of reformers.

I am attempting to gain a general understanding of the effectiveness of partisan electoral engineering, and that can only be achieved by looking at multiple countries, which rules out the possibility of in-depth case studies. Thus, I am focusing on the observable and measurable outcome contingencies. Act contingencies should not be forgotten though and might explain some of the findings my model cannot, and that could be cleared up later by qualitative case studies that take act contingencies into account.

It should also be noted that while there is considerable literature on the effect of certain features of electoral systems, there are always unintended or unexpected consequences, and electoral reform can even bring about the opposite of the result that its proponents aimed to achieve. This can be a result of improperly assessing either outcome or act contingencies or both. The reason for this is the considerable uncertainty political actors have to deal with when designing institutions, especially in unstable situations, such as volatile political competition or young, unconsolidated democracies (Lijphart, 1992; Geddes, 1996). Even the mechanical effect of certain systems is not perfectly deciphered by experts, and the relative popularity of parties in upcoming elections proves to be even more difficult to predict under such circumstances (Shvetsova, 2003).

Andrews and Jackman (2005) offer evidence that actors in interwar Western Europe and in the new democracies of Central and Eastern Europe in the 1990s acted strategically when creating and altering electoral institutions: they were clearly attempting to maximize their influence through electoral engineering. Yet, the majority of them failed in doing so and have not benefited from electoral reform the way they expected to as a result of the uncertainty reformers faced when designing institutions.

Nevertheless, the authors claim that this was due to the volatile situations in these periods: the interwar period saw the unprecedentedly rapid expansion of suffrage in Western Europe, changing the electorate significantly and depriving politicians of the information that came to be available to later legislators in more stable democracies. In the case of Central and Eastern Europe, the institutions were new and it was impossible to predict how the electorate will react to them and what its voting patterns was going to look like. All in all, the lack of information caused reformers to act against their own interests. In order to eliminate this type of extreme uncertainty, I will not analyze elections before universal suffrage was generally instituted and will also focus on changes to an established electoral systems, not the introduction of a completely new one in a freshly created democracy.

Uncertainty exists in less volatile situations as well, as shown by several examples in the literature. Fahey (2018) examines the phenomenon that in the United

States, certain states introduced term limits and staffing regulations for state legislators, expecting these to reduce the advantage of incumbents in elections. However, he demonstrates that the reforms actually increased the incumbency effect, as due to the introduced constraints, quality challengers were disincentivized to run and only competed for empty seats with a much higher probability of winning, making elections less competitive.

Another instance of uncertainty in established democracies is offered by Bronner and Ifkovits (2019). They show that while the governing coalition of Austria hoped to strengthen its support in 2007 by lowering the age of voting eligibility from 18 to 16, the reform had the opposite effect: voters who became eligible as a result of the reform have been supporting parties more than older voters did, decreasing the overall support of the governing parties.

These examples serve as a reminder that the consequences of electoral reforms cannot be predicted with complete certainty, and even if parties introduce reforms with their self-interest in mind, they cannot be sure to benefit from the system they propose, it is thus worthwhile to test the actual efficacy of electoral reforms, because uncertainty might cause power-maximizing actors to err and introduce changes that they thought were beneficial for them, but in practice turned out to be harmful instead.

Another confounding aspect of partisan motivations is the delicate distinction between electoral and policy payoffs. Parties could push for electoral reforms for ideological reasons even if the proposed system is expected to hurt their electoral performance. This could happen as a result of anticipating act contingencies: if they did not support reforms that are ideologically consistent with their platforms, their voters might deem them untrustworthy and shift their party preferences.

This is the phenomenon that Bol (2016) investigated. Analyzing major cases of electoral reform in OECD countries between 1961 and 2011, he concluded that parties usually support electoral reform if it fits their policy platform and if they expect to benefit from it. He essentially provides evidence for both policy-seeking (i.e. ideologically motivated) and power-seeking (i.e. seat-maximizing) behavior (Müller and Strøm, 1999).

This finding suggests that normative considerations can indeed play a role in introducing electoral reform, but also demonstrates that self-interest cannot be neglected. In fact, in the analysis of Bol (2016), the seat-maximizing model had a higher explanatory power than the generic ideological one, implying that even though both factors are at play, the former is a more adequate point of reference if one has to choose between the two. Nonetheless, ideological and policy preferences are additional potential factors that might help explain results that the power-maximizing model cannot account for.

2.3.5 Different approaches to analyzing the partisan motivations of electoral engineering

There are several ways to investigate partisan motivations of electoral change. Several scholars approach electoral engineering by examining its effect on democratic quality, particularly how it can enhance democratic erosion. 'Electoral authoritarianism' is a system where electoral rules are engineered in a way that ensures that only the incumbent government to have a realistic path to victory, thus entrenching its position while retaining a democratic façade through regularly held elections (Schedler, 2006). The creation of an electoral system that ensures the perpetuation of the dominant position of the ruling actors. As discussed in Chapter 1.11, the relationship consolidation attempts have with democracy can be quite complex, and since I am looking to gain a general understanding of the effectiveness of electoral engineering, I am not investigating how this kind of power consolidation affects the quality of democracy, and do not limit my research to consolidation attempts to ones that decreased it.

Social choice approaches "seek to characterize voting rules in axiomatic form or study normative properties of voting rules in general" (Grofman, 2016: 525). Scholars in this tradition analyze rules and their consequences in an abstract way, without a lot of concern for normative consequences electoral systems have on their respective political systems. Whereas these analyses are useful for gaining hypotheses about how different types of electoral reforms affect electoral outcomes, they are too general, and not concerned with the benefits and detriments to specific parties, instead calculating the advantages and disadvantages of hypothetical actors. I am focusing on the actual practical applicability of electoral engineering as a tool of power consolidation, and these abstract methods can consequently not be an adequate approach to my empirical inquiry.

Another way to investigate electoral change is through in-depth case studies that can uncover the motivations and processes that led to electoral reform and the effect it had on the political arena. The most comprehensive collection of examples is the IDEA Handbook of Electoral System Design (2005). There are numerous case studies that use this approach and their findings can supplement that of the general literature of electoral reform.

Brady and Mo (1992) argue that based on the case of the change of electoral law in South Korea in 1988, the reform process is mainly a bargaining mechanism through which the original interests of one or maybe all actors are dissolved, and the outcome may not be to the liking of any party participating in it. Therefore, even though partisan motivations are indeed the primary drivers of electoral change, the final shape of the reform might not reflect the interests of the reformers.

In his inquiry of electoral reforms in Belgium, Pilet (2005) finds that the reform process in each instance was significantly influenced by the consociational nature of the Belgian political system, and as a result, contrary to previous expectations and the intentions of some actors, electoral reforms consequently strengthened consociationalism in the country. This finding points to the strength of cultural modernization theory, because underlying social norms ended up prevailing over the rational self-interest based motivations of the reformers.

In a later study of the support of proposed changes to the Belgian electoral system, he also pointed out how actors could oppose reforms that are supposed to improve their electoral performance due to being uncertain whether the actual outcome of reform will mirror that of a simulated one. If a political actor is more or less content with its current amount of power, it will not seek to increase it through uncertain methods (Pilet, 2008). This points to how the evaluation of risk is a part of the calculus for reformers in addition to simulating how alternative electoral systems would affect their power in the legislature.

A case study with a series of interesting and consequential findings was conducted by McElwain (2007) on Japanese electoral changes. He argues that while due to intra-party and constitutional constraints, large-scale electoral reform did not take place in post-war Japan until the 1990s, the dominant Liberal Democratic Party was able to strengthen its position between 1955 and 1993 through micro-level reforms, such as changes in campaign financing, district magnitude, electoral formula and electoral thresholds, which all favored incumbents.

Similarly, Tan and Grofman (2016) find that even in the absence of large-scale electoral reform, electoral engineering has had a particularly long history in Singapore. They identify changes in campaign finance regulations and in the methods of candidate selection as the main tools used in that country to great effect.

This suggests that reforms of the smallest scale could have significant effects on the political arena, and sometimes miniscule modifications are even more important than major alterations, as the latter often does not reflect the interests of any reformer due to the extensive bargaining process that usually precedes it, as proven by Brady and Mo (1992), or as a result of the strong norms in society that could supersede any partisan motivations per the findings of Pilet (2008).

Case studies are useful for inferring and testing hypotheses about the consequences and dynamics of changes in the electoral system, but not suitable to gain a general understanding of the effectiveness of electoral engineering, which is my goal in this dissertation. Comparative cross-country studies are more apt to do that. Over the course of the last twenty years, numerous analyses have been published using this framework.

Calvo (2009) conducts such an analysis on early electoral reforms in Western Europe (before World War II). He demonstrates that while other authors, such as Rokkan (1970), Boix (1999) or the previously cited Andrews and Jackman (2005) famously considered these reforms at least partially ineffective from the strategic standpoints of the ruling parties of that era, the rational self-interest-based model does in fact explain the changes to the Western European electoral systems in this period. He does so by taking into account not only seat-maximizing, but also seat-minimizing strategies: some of the right-wing parties of the time had the goal of combating the Communist threat, and they supported electoral reforms that ensured the minimization of the influence of radical left-wing parties. Furthermore, even in the cases where the Communist threat does not provide a rational explanation for the seemingly irrational behavior of certain parties, the emergence of territorial parties does: as a result of expansive enfranchisement, new parties gained support, and some of them had a strong enough local support to compete under majoritarian electoral rules, but not enough votes to be a contender under proportional ones. That prompted certain parties who, based on their vote share, were supposed to support majoritarian systems to promote proportional ones.

In another cross-national survey of electoral change, Riera (2013) looks at the effect of electoral reforms on subsequent elections in 60 countries between 1945 and 2010. He first focuses on how intra-party relations were affected and how the party system changed as a result of changes in the system before turning to the evaluation of the efficacy of electoral engineering. Discussing the partisan gains and losses electoral reforms induce, he concluded that supporting permissive reforms (i.e. ones that make the system more proportional) helped parties if party system fragmentation is low, while restrictive reforms (i.e. ones that push the system in a majoritarian direction) are effective if the opposite is true. The approach taken by this study is the one closest to the one I will adopt, but is different from several important methodological aspects. I will detail some of these differences as I outline my methodology in Chapter 2.4. Overall though, my general approach strongly mirrors that of Riera (2013).

2.3.6 Quantifying the impact of electoral engineering

There have been plenty of attempts to quantify the partisan bias of electoral systems. The proportionality of electoral systems can be measured by the Rae Index (Rae, 1967), or its modified and more accurate versions, the Rose Index of Proportionality (Rose, 1984) and the Gallagher Index (Gallagher, 1991). These indices are widely accepted and used but are not suitable for this investigation, because they look at the electoral system as a whole and not the effect the system has on the electoral performance of specific parties.

In the United States, due to the country having a first-past-the-post system at virtually every level of government, political science has generally focused on the effects of redistricting, and a lot of studies (e.g. Niemi, Grofman, Carlucci and Hofeller [1990]; Johnston [2002]) deal with district shapes and political geography as a result.

Grofman, Koetzle and Brunell (1997) describe the three factors contributing to partisan bias in district-based systems. The first one of these is 'wasted votes', i.e. the votes in excess of what is necessary for the election of a candidate. The second one is the differences of turnout among districts. The third one is malapportionment, which is a significant difference between the number of eligible voters in certain districts. The authors provide tools to measure the independent effect of each factor.

McGhee (2014) argues that the most widely used metrics that evaluate the partisan bias of single-member district systems are inadequate due to the assumption of symmetry and introduces his own measure that shows smaller effects of redistricting than previously offered standards of measurement. Widely used as they are, these measures are specific to single-member plurality systems and in many cases to two-party competition, and therefore cannot be generalized to each of the political contexts I aim to include in my dataset.

Other scholars focus on the predicted effect of adopting a certain electoral system by trying to establish ways to identify what the expected results under a proposed or adopted system would be before an election is even conducted under its rules. These expected results are then generally compared to some kind of an ideal that is deemed to be 'fair'; if the two converge, then partisan bias is not significant, if they do not, partisan bias is an issue. Expected results are calculated based on certain characteristics of the electorate, such as registration within a party, income-level, ethnic composition, etc.

For instance, Wang (2016) uses this approach when he calculates the "unrepresentative distortion" of districts in certain US states. Studies that focus on district shapes are often similar in their design by comparing actual districts to ideal or fair districts. Methods that focus on the observed partisan bias of electoral systems usually also have an explicit or implicit ideal they compare results to. That ideal is more often than not identified as proportionality for every relevant actor, i.e. the partisan bias of the system should be similar across the board. However, in this dissertation, I am not interested in the question of fairness in general, but with the issue of which actors benefit from electoral reform, and these metrics are hence not suitable for my inquiry.

The simplest way to measure if a specific party benefited or was harmed by the partisan bias of a system was introduced by Tufte (1973). He proposed the basic idea that number of votes compared to gained seats should be adequate to measure this effect: certain parties need fewer votes to gain an additional seat their peers do, these parties are beneficiaries of the partisan bias of the electoral system. Formally put:

$$b = \frac{s}{v}$$

Where *b* stands for partisan bias, *s* stands for the share of seats allocated to the party by the electoral system at the election in question, and *v* stands for the overall vote share the party received at the election in question. That means that the more seats a party is allotted for the same vote share, the higher its partisan bias becomes. Furthermore, the smaller vote share it requires to gain the same amount of seats in the legislature, the higher the partisan bias is. This metric is used in various studies (e.g. Calvo [2009]) that posit that parties are looking to maximize *b* in this equation.

It is reasonable to assume that this is indeed the metric what electoral engineers are having in mind when designing electoral reforms. The purpose of electoral systems is to convert votes into seats, and therefore the relationship between the two is what electoral systems affect the most directly, at least through the mechanical effect. Seats in the legislature are the source of power for parties, and if we treat them as power-maximizers, which the consolidation framework does, then they are attempting to maximize their seats. One way to do that is to increase their vote share (see the paragraph below), but another is to make sure that a given number of votes turn into as many seats as possible through the increase of partisan bias.

On the other hand, Riera (2013) focuses on the change in popular support (change in vote share), not on how favorable the electoral system is to parties (change in partisan bias). Despite the important psychological effects of electoral institutions and the act contingencies of electoral reform, vote share is largely not determined by the electoral system, but partisan bias is almost solely a result of it. Therefore, the latter is more appropriate for the evaluation of the effectiveness of electoral engineering.

Votes are determined through effective campaign communication, turning out the base, selecting the issues voters are most responsive to, etc. Electoral systems only play a very small role in that, even though electoral reforms might become salient campaign issues themselves, affecting vote share in the process through the psychological effect, as discussed in the literature review. This is not easy to forecast, and it would not be prudent to assume that reformers are focusing on the psychological effect when designing electoral systems that align with their partisan motivations. If they intended to gain votes, introducing new electoral rules would be an odd way of doing so. It is reasonable to assume that the more predictable mechanical effect is the focus of reformers, even if they are looking to prevent any backlash through the psychological effect.

An analysis that treats vote share as the dependent variable would only look at the psychological effect, and would do so poorly, since the psychological effect is affected by numerous other issues. Partisan bias on the other hand almost perfectly encapsulates the mechanical effect, while admittedly ignoring the more complex psychological effects. Naturally, this is not a complete picture either, but treating partisan bias as the main dependent variable is suitable for assessing whether the objective of reformers when designing the reforms, i.e. obtaining as many seats as possible for the votes that they can garner, is achieved successfully or not. That is the aim of this analysis.

2.4 Research question and hypotheses

2.4.1 Research question

My aim in this dissertation is to evaluate the effectiveness of electoral engineering. Simply put, I would like to ascertain whether the reformers benefit from changing the electoral system, and if they do, how significant those benefits are. Therefore, my general research question is as follows:

RQ: Do electoral reformers generally design electoral systems that benefit them?

Over the course of the rest of this chapter, I will formulate three distinct hypotheses, the testing of which will help me find an answer to this very question, as well as a couple of additional hypotheses that will aid the interpretation of the results of H1-H3.

2.4.2 The first hypothesis

Firstly, I will not compare observed results to a certain ideal or fair distribution, but I will compare them to previous results the same party had in preceding elections. The basic idea is that parties initiate electoral reform to improve the partisan bias of the system to their advantage. They are not content with the bias the electoral system in place provides, and introduce modifications so that they can enjoy a higher bias in the new system. Had they expected a decrease in partisan bias as a result of the changes, they would not have proposed the alterations in the first place. If their partisan bias increases after the system is altered, they are successful, if it decreases, they are not. Therefore, the first hypothesis can be formulated as follows:

H1: Parties that enact electoral reforms have a more favorable partisan bias at the election after the reform took effect than they did in the election before it.

2.4.3 The second hypothesis

Furthermore, it could be argued that parties that expect electoral payoffs from electoral reforms do not only think in terms of improving on the partisan bias they used to have, but also aim to design a system that benefits them more than it does other parties in general. They might not care about whether they improve upon their partisan bias or not, but certainly would like to have an outsized one compared to their peers. If they have a higher partisan bias than the non-reformer parties, they might consider their reforms successful even if their bias decreased compared to previous elections. For example, this could be the case if they had extraordinary biases when they ascended to government that they deem to be impossible to repeat. Thus, their standard is not this previously attained disproportionate bias, but simply a higher bias than that of their rivals. My second hypothesis derives from this idea, and is formulated below:

H2: Parties that enact electoral reforms have a more favorable partisan bias at the election after the reform took effect than other, non-reformer parties do.

2.4.4 The third hypothesis

Additionally, it is possible that electoral reforms are not designed to enhance the partisan bias of reformers in subsequent elections, all they could be aimed at is power preservation. The goal of reformers in this case is not to improve their electoral bias, as they are satisfied with their current levels. Moreover, all they care about is ensuring that they will continue to possess governing power, additional seats, and a consequent increase in the amount of power is not a worthy payoff for them, but the continued control of the legislature, which guarantees them a commanding position in the political arena, is. Thus, even if their partisan bias is not significantly better than the one they had at preceding election or the one attained by their rivals under the new rules, if they can remain in government, they could consider their electoral engineering to be successful. Therefore, my third hypothesis is formulated as follows:

H3: Parties that enact electoral reforms have a better chance of remaining in power after the reform took effect than other, non-reforming government parties do.

2.4.5 Additional questions to be answered by the analysis

In addition to finding an answer to my research question through the testing of the three hypotheses, I will also attempt to uncover the motivations to introduce different types of electoral systems using the method proposed by Andrews and Jackman (2005). They suggest that reformers with relatively low partisan biases at preceding elections will favor proportional electoral reforms, while those with larger biases will adopt majoritarian changes to the electoral system. In other words, the larger the partisan bias of a reformer party at the election before the reform was adopted, the more likely it is that the reform is in a majoritarian direction (Andrews and Jackman, 2005: 82).

This hypothesis is in accordance with my theoretical framework and could prove to be useful to assess the results of testing my own hypotheses. The idea behind this hypothesis is that majoritarian reforms generally increase the differences of partisan bias, while proportional ones shrink it. That means that parties with a high partisan bias can expect an even higher one as a result of the reform. Reformers with relatively low partisan biases are more prone to lose subsequent elections and can therefore be defending against those losses by adopting a more proportional system that will even partisan biases out.

H4a: The higher the partisan bias of a leading reformer party at the election preceding electoral reform, the more likely it is that it will adopt a majoritarian reform; and the lower the partisan bias of a leading reformer party at the election preceding electoral reform, the more likely it is that it will adopt a proportional reform.

I will also test this hypothesis in a modified form. I hypothesize that parties that gained a lower vote share at the election after the reform than the one before are aware of their declining popularity and are introducing reforms to counter the seat losses this decline prompts, compelling them to favor proportional reforms. Therefore, the more the vote share of a reformer party decreases at the election after the reform, the more likely it is that it will be a proportional reformer, and vice versa.

H4b: The more the vote share of a leading reformer party increases, the more likely it is that it will adopt a majoritarian reform; and the more the vote share of a leading reformer party decreases, the more likely it is that it will adopt a proportional reform.

2.4.6 Summarizing the hypotheses

Essentially, I evaluate the effects of electoral system change had on reformer parties using three different benchmarks: their own partisan bias from the previous election, the partisan bias of other, non-reforming parties, and the probability of reelection of other, non-reforming incumbent government parties.

In addition to these three benchmarks, an alternative counterfactual could be comparing the electoral performance of the reformer party in the newly adopted system to the one it would have attained had the rules remained unchanged and the old system stayed in place. I opted not to use this approach in this dissertation for two reasons. First, due to the psychological effects and act contingencies of electoral reform, it cannot be assumed that the exact same vote totals and territorial vote distribution would have happened had the rules remained unaltered.

Additionally, rival parties can react to the changes and adapt to them better than the party that initiated the reform. If one assumes that political actors anticipate the psychological effects of electoral engineering, simply looking at the difference of the mechanical effect of the actual election and a hypothetical election conducted under the old rules is an erroneous approach: political actors adapt to the change of rules and the rules themselves were created with the anticipation of this adaptation. The old system is therefore not an actual alternative to the new one after the acceptance of the electoral reform. Using this hypothetical benchmark would require to assume that political actors and voters either do not react and adapt to changes in electoral rules, or that reformers do not anticipate these changes when designing new systems. I cannot accept either of these assumptions, therefore, for the purposes of this inquiry, it is more prudent to use only real-life outcomes as benchmarks instead of hypothetical ones.

Furthermore, testing a hypothesis based on this counterfactual is not possible using the dataset I use for testing H1, H2 and H3, i.e. whether reformers have a better partisan bias after the reform than beforehand, whether reformers have a better partisan bias than their competitors, or whether reformers have a better chance of being reelected than non-reformers do. It would require precinct-level voting data and the exact boundaries of electoral district of each election I include in the analysis. As far as I know, such detailed data is not available on a large enough scale. A suitable dataset could only be construed from datasets that only include relatively recent elections conducted in a relatively few countries, and mainly the most developed ones. My aim is to gain a general understanding of the effectiveness of partisan electoral engineering as a means to consolidate power, which can be done through cross-national analysis of a diverse set of elections consisting of a large number of cases from various different political contexts. That is currently not possible for this counterfactual, but it could be interesting to look at it in further research using case studies.

The first two hypotheses focus on the mechanical effects of electoral reform, but ignore the psychological effects. I assume that electoral reformers anticipate these effects and design the new system expecting that the interaction of mechanical and psychological effects will be beneficial to them overall. Misanticipating these psychological effects, as well as other unintended and unexpected consequences could explain the eventual failure of political actors to increase the partisan bias of the electoral system in their favor, should the analysis point to such a failure.

The third hypothesis is different in this regard. It does not solely focus on the direct effect of an electoral system translating votes into seats, but on an overall electoral performance that includes the garnering of votes as well. This is advantageous from the perspective of encompassing both types of impact electoral reform could have on power relations, but has the downside of including effects that may not be related to electoral engineering. A party could lose its cabinet position for a number of different reasons. It could simply lose popular support for its policy positions, or its politicians could lose credibility, resulting in a diminishing vote share. Alternatively, it could leave the cabinet voluntarily due to disagreement with coalition partners. This could mean that electoral engineering actually worked, but H3 is still rejected in that specific case. That could be especially important if H1 and H2 show one result and H3 a different one.

Nevertheless, the inclusion of H3 is instrumental for several reasons. Firstly, it is reasonable to expect many actors focusing on their quite palpable government status instead of their more elusive partisan bias. This is somewhat different from including similar dependent variables related to overall electoral performance instead of the effect of the electoral system, such as identifying the changes in vote share as the primary goal of the reformers, as done by Riera (2013). Vote share can be influenced by the psychological effects of electoral engineering, but not by the mechanical ones that only affect the way votes are translated into seats. Ignoring the mechanical effects and treating changes in vote share as a sole product of the psychological effects of electoral engineering, when party popularity is affected by a plethora of other factors as well is in my opinion not the most apt way to evaluate the effects of electoral reforms. Examining reelection rates is similar to this suboptimal way of measurement, but unlike vote share, reelection is largely determined by the distribution of seats, which is influenced by the electoral system.

Another reason the inclusion of H3 is beneficial for this dissertation is that it allows me to assess the effectiveness of an entirely different type of power consolidation than H1 and H2 do. It focuses on the preservation of power at its current levels instead of increasing it, and is therefore solely about negative consolidation, while H1 and H2 are suitable for the evaluation of the efficacy of electoral engineering to consolidate power positively. As described in Section 1.10.1, the two types are related to each other in numerous ways, but are separate concepts and it is possible that a tool of power consolidation is effective for achieving one, but not the other. This could be demonstrated if the analysis of H1 and H2 indicates different results than testing H3 does. This is why electoral engineering is a great phenomenon to analyze in this framework, because positive and negative consolidation can be examined individually.

The fourth hypothesis will be important to find out whether mitigating potential losses, instead of increasing potential gains is the primary goal of proportional reformers. In addition to confirming or contradicting the findings of Andrews and Jackman (2005), it is also useful to ascertaining the actual usefulness of H1-H3 in case of proportional reformers. Those reformers could mainly be focusing on countering their otherwise inevitable decline by the reform, and thus acting well within the rational power consolidation framework, without achieving any of the goals H1-H3 assume. If H4 is confirmed, that would suggest that including proportional reforms in the analysis of H1-H3 could distort the results.

2.5 Data and methods

2.5.1 The dataset

Since I would like to gain a general understanding of the how changes in the electoral system affect the partisan bias incumbents, I will include as many cases as possible in my analysis. I therefore chose to fuse two databases to create one that tracks the changes both electoral system change and electoral results on a global scale.

In order to identify electoral reforms, I used the Democratic Electoral Systems around the World database (DES)[10] as the basis of my inquiry. The database has detailed information on electoral systems dating back to 1945. This dataset has the advantage of including detailed information on electoral systems, and due to its exhaustive nature, relatively slight changes can be detected using its data. This is important, because I am intent on analyzing small reforms as well as wholesale changes to electoral systems. Most studies (including, with few exceptions, virtually all studies cited in this dissertation) only consider electoral reforms that move the electoral system from one 'family' (i.e. majoritarian, proportional, mixed) to another, or at the very least

[10] Bormann, Nils-Christian and Golder, Matt (2013): Democratic Electoral Systems Around the World, 1946-2011. *Electoral Studies* 32, pp. 360-369.

significantly alter the relationship between the proportional and majoritarian branches in mixed systems.

Based on the findings of case studies and social choice analyses, I will attempt to examine as many changes as possible, regardless of their magnitude. For example, Grofman (2016, 526) suggests that it is important to go beyond the simple division between proportional and plurality systems and consider the differences between the various subtypes, as well as changes in often neglected features, of which he highlights mean district magnitude, and electoral tiers. Furthermore, the findings of McElwain (2007) or Tan and Grofman (2016) discussed in Section 2.2.5 indicate that even smaller, miniscule peripheral changes could make a huge difference when it comes to electoral engineering.

I therefore selected a database that with as much information as possible, and the DES is suitable in this regard. Not only does it include data from a historically and geographically diverse set of cases, it is detailed enough to include modifications to the features listed by Grofman (2016). Unfortunately, reforms as microscopic as the ones McElwain (2007) or Tan and Grofman (2016) discuss are not included even in this dataset, representing a potential explanation of any rejected hypotheses: the database excludes potentially effective reforms that only change small, circumstantial elements of an electoral system. This will be important to keep in mind when evaluating the results.

For the purposes of my analysis, I combined the DES with the Database of Political Institutions (DPI)[11], a dataset created by the World Bank. In addition to information on political institutions, the DPI also has extensive information on electoral results, which is the part I used when creating the database for my analysis. The latest version of the DPI includes information from 1974 to 2017. Since the DES goes even

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[11] Cruz, Cesi; Keefer, Philip and Scartascini, Carlos (2018): *Database of Political Institutions 2017 (DPI2017)*. Inter-American Development Bank. Numbers for Development. https://mydata.iadb.org/Reform-Modernization-of-the-State/Database-of-Political-Institutions-2017/938i-s2bw

further back in time, I needed to exclude cases that occurred before 1974 due to them not being listed in the DPI. Therefore, the timeframe of the combined database used for the empirical investigation is 1974 to 2017.

This timeframe allows for a historical perspective but does not go back to earlier periods when even democratic countries did not have universal suffrage and the use of democratic institutions was substantially different. Additionally, it excludes the uncertainty caused by the ever-growing enfranchisement of the first half in the 20th century, which, according to Andrews and Jackman (2005), was a major contributor to the ineffectiveness of electoral reforms during that period.

The DPI has data on the vote share and the number of seats of the three largest government parties and three largest opposition parties after every election, making it suitable as the basis for the calculation of partisan bias for each party. In order to make the electoral results comparable, I only included legislative elections in the database, and in the case of bicameral legislations, I disregarded the results of elections to the upper house. I only considered competitive elections[12] where there was no electoral fraud[13].

I matched the electoral results from the DPI to the electoral rules in the DES, creating the database that included both the results and the rules of virtually every democratic election that was conducted between 1974 and 2017 in the world. Due to some missing data in the original databases that I merged, I have also supplemented certain data points myself using the resources of the Inter-Parliamentary Union Parline database[14] and Nohlen's handbooks of elections per continent (Nohlen, 1999; Nohlen, 2001; Nohlen, 2005a; Nohlen, 2005b; Nohlen-Stöver, 2010). More details on the modifications and additions I made can be found in the Appendix.

[12] I defined competitive elections as the ones having a 'liec' score in the DPI of 6 or above, i.e. multiple parties won seats in the legislature, and at least one of them was in the opposition.

[13] I.e. the election had a 'fraud' score of 0 in the DPI.

[14] http://archive.ipu.org/parline/ Last accessed on 25 June 2020.

This is the dataset analysis was conducted on. The similarly extensive database that Riera (2013) uses is different from the one I will, because his data does not allow for the detailed comparisons mine enables as it does not take into account smaller modifications to the electoral system and also includes fewer cases from fewer countries from fewer regions.

Overall, my dataset covers 4,564 cases (the result of a specific party in a specific election) of 1,142 different parties participating in 1,055 different elections in 141 different countries. 324 of the 1,055 elections took place after some form an electoral reform, and there were 512 reformer parties out of 1,356 parties that were a member of government prior to an election. A large majority (4,134) of the cases occurred in systems with the highest 'liec' score (7), indicating the most competitive and democratic category of legislative elections by DPI classification. 1,345 of the cases are from majoritarian systems, 782 from mixed systems, and 2,416 from proportional ones. Of the 324 electoral reforms, 80 altered the system in a majoritarian direction, 118 made the electoral system more proportional and 126 were neutral in that regard. 37 of the reforms were large-scale, 46 mid-scale, and 241 small-scale (I will elaborate on the categorization of reforms in the following section). This shows that not only is the database geographically and historically diverse, it covers a sufficient number of cases from all sorts of electoral systems and electoral reforms, making it suitable for a comprehensive comparative analysis.

2.5.2 Operationalizing the variables of the analysis

In order to identify electoral changes, I amended the DES, so that if any feature of an electoral system (electoral system family, rule of distributing seats, number of electoral tiers, distribution formula for each electoral tier, type of mixed system, number of total seats, number of seats distributed in each electoral tier) changed between elections according to the database, I coded that election as a case of electoral change.

This means that according to my coding, any election that was not conducted under rules identical to the ones governing the preceding election was considered to have taken place after an electoral reform. This allows for the analysis of the effect of even small alterations, as compared to most studies in this subfield that focus on drastic changes to a system. Yet, this still does not include redistricting if the overall number of seats in any tier remain unchanged. In several systems, especially majoritarian ones, redistricting is the main source of electoral engineering (Makse, 2012: 226), making my database still somewhat incomplete, but it includes as many cases as it was possible to identify using the two databases I selected for my inquiry.

Due to the very different nature of the reforms in the database, I differentiated between large-, mid- and small-scale reforms. An electoral reform was considered large-scale if the family of the electoral system (majoritarian, proportional or mixed) was altered (this is what is generally analyzed in the literature when electoral reforms are discussed). It was identified as mid-scale if the system remained in the same family as before, but the method of seat distribution or the relationship between the various electoral tiers was changed[15]. In all other cases, the reform was coded as small-scale.

This distinction allows me to analyze the different types of reforms separately. There could be different expectations on how efficient reforms of different magnitude are. One could argue that large-scale reforms should have a larger impact and consequently should be more effective for electoral engineering. On the other hand, based on the literature of smaller reforms cited before, one could argue that reforms on a smaller scale could be more effective, because they could be more targeted, limiting the possibility of unintended consequences, and they are less likely to be salient issues in the public arena, minimizing the risk of an electoral backlash and act contingencies. The analysis should show which of these ideas appear to be more correct.

I also distinguished between majoritarian, proportional or neutral (i.e. reforms that did not push the electoral system in either direction) reforms based on the direction of the reform. For example, a majoritarian reform makes an electoral system more majoritarian than it used to be. That does not necessarily mean that the system became majoritarian, it could even remain proportional, but within the proportional electoral

[15] I.e. a change in one of the following variables of the DES: ‘elecrule’, ‘formula’, ‘mixed type’ or ‘multi’.

family, it can be considered to be more majoritarian than before (this could be achieved by decreasing district magnitude, for instance). This is the same distinction Riera (2013: 81) makes between permissive (proportional) and restrictive (majoritarian) reforms. Since he did not include small-scale reforms in his analysis, no reforms were coded as neutral by him.

An electoral reform was coded as majoritarian if a large-scale reform changed the family of the electoral system in a majoritarian direction (either to a majoritarian system or from a proportional to a mixed system).

In case of a small-scale or a mid-scale reform, the definition of majoritarian reforms differs based on which electoral family the reform was conducted in. In majoritarian systems, majoritarian reforms were the ones that reduced district magnitude by more than 10%. In mixed systems, if the share of seats distributed via the majoritarian elements of the system increased, the reform was considered majoritarian. If the share of seats distributed through the majoritarian elements remained unaltered, but the threshold to get into parliament increased, or the PR district magnitude decreased (the latter by more than 10%), the reform was also coded as majoritarian. In proportional systems, if district magnitude decreased by more than 10% or the threshold to get into parliament was raised, the reform was identified as majoritarian.

Proportional reforms were coded similarly, but naturally, the reforms needed to go in the opposite direction to be coded as proportional. If a reform met neither the criteria for a proportional reform, nor the requirements of a majoritarian one, it was coded as neutral. Neutral reforms generally include small changes (i.e. less than 10%) to the district magnitude.

After identifying the reforms, each reform was assigned one or more reformer parties using the DPI. The DPI includes information on whether a party was a member of government at the end of each year, and I presumed that the incumbent government parties were the ones enacting electoral reform.

In the analysis, I tested H1-H3 (change in the bias from the previous election, bias compared to non-reformer parties, and reelection chances of reformers) separately for all government parties (i.e. each member of the coalition coded as reformers, including quite small parties) and leading government parties (i.e. only the government party with the largest seat share coded as a reformer). This is an important distinction, as it is quite possible that the interests of junior members of a coalition are not as well-represented in the proposed electoral system as those of the senior members. Leading reformers could even introduce reform to strengthen their position within the coalition, meaning that the system is designed to be disadvantageous for junior coalition members. Consequently, the inclusion of smaller government parties could result in potentially distorted findings due to the lack of positive results in their case. Therefore, I conducted separate analyses with both of these different possible codings to address this potential issue.

The assumption that electoral reforms are always initiated by the government is certainly not always true, as the initiative could come from the opposition, or completely outside of the parliament (a popular initiative, recommendations from an external organization, or a legal obligation). However, as proven by the literature, in most cases, it is indeed the government that designs the new system. Even in cases where it only reacts to proposals by other actors, its approval is certainly necessary for the electoral reform to pass in the legislature, and therefore, its interests are expected to be reflected in the final form of the new electoral system. As Renwick (2010: 16) points out, no electoral reform has been passed without approval of the government, and government parties always substantially shape the features of the new system.

This is one of the findings of Walter and Emmenegger (2019). Focusing on the process of adoption of more proportional systems, examining cases in Swiss cantons, they demonstrate that even when the parties in government did not originally support the introduction of these systems, when they were persuaded or compelled by the opposition and public opinion, they successfully designed the reforms to minimize losses and maximize gains.

The assumption that government parties are the main architects of electoral reforms is widely accepted in the literature. Bol (2016: 97) only selects reform proposals that were at one point submitted to parliament by a member of government in his dataset due to the impossibility of accepting a reform that is not supported by any of the parties that comprise the legislative majority. Andrews and Jackman (2005: 82) use an assumption that is extremely similar to mine: in their model, the largest government party is the one designing the electoral reform. This prompts me to identify reformer parties as the ones in government at the time of the adoption of the reform.

For measuring partisan bias, I will opt to use the simple metric offered by Tufte (1973) and described in Section 2.2.6. This is also the metric used by Calvo (2009) and Andrews and Jackman (2005) to assess the effects of electoral reforms. It is a widely accepted measure of the very subject of my analysis. Partisan bias will be computed for each case, i.e. each result for each party at each election, allowing me to track the change in partisan bias from election to election for every party in the analysis.

2.5.3 Methods of analysis

In order to test H1, i.e. whether reformers have a better partisan bias after the reform than they did beforehand, I will first conduct several straightforward analyses of descriptive statistics. I will simply look at the share of the reformer parties that increased their partisan bias at the election right after the reform took effect. I will do this by presenting the change in partisan bias (partisan bias at the current election divided by the partisan bias at the preceding election) for reforming parties. The aim of reformers is to increase their partisan bias, i.e. have this number larger than one, as one would mean that partisan bias is identical at both elections. If the majority of reformers have a change that is higher than one, i.e. they increase their partisan bias after the reform, it suggests that the hypothesis should be accepted. I consider this an imprecise, yet telling measure for testing H1 that provides an easy-to-understand overview before the other tests.

Additionally, I will assess H1 comparatively as well, that is, I will compare the change in the partisan bias of reformers to non-reformers. This will be done in several ways. First, I will run OLS regressions[16], where the dependent variable is the partisan

[16] Ordinary Least Squares (OLS) regression is the most common type of linear regression. It models the relationship between two or more variables. It works on the

bias of the party after the reform, the independent variable is a dummy determining whether the party is a reformer party or not, and the partisan bias of the same party at the preceding election is used as a lagged dependent variable so that the model captures the change in partisan bias as a result of the electoral reform. In this case, it is not the significance and explanatory power of the entire model that will be telling, as bias at the previous election almost surely predicts bias at subsequent elections, but whether the main independent variable (being a reformer) itself is significant or not.

Another method of testing the change in the partisan bias of reformers will be a difference-in-differences analysis (DID). This is a widely used method in health sciences and other fields as well. This research design requires two groups, one receiving a 'treatment', and an 'untreated' control group. The value of the dependent variable and potential covariates are recorded for both groups at two different points of time: once before the 'treatment', and once after it. If the value of the dependent variable in the 'treated' group has changed significantly differently than it did in the 'untreated' group, the difference will be attributed to the 'treatment effect'. For more details on this method, see Wing et al. (2018).

In my case, the dependent variable will be partisan bias, the 'treatment' is electoral reform, the 'treated' group will be electoral reformers, and the 'untreated group' will be non-reformer parties. For reformers, partisan bias is recorded at the election preceding the reform (before 'treatment') and at the election following the enactment of the reform (after 'treatment'). For non-reformers, partisan bias at the election in question will be considered a case in the 'untreated after' group, while their partisan bias at the preceding election will be classified as a case in the 'untreated before' group, regardless of whether a reform has taken place between the two elections. The difference in the change in partisan bias will be considered the treatment effect, and

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presumption that there is a linear relationship between the independent variables (the ones used for explaining) and the dependent variable (the one that is explained by the model).

based on H1, we can expect that the 'treated' group (reformers) will have a larger increase or a smaller decrease in their partisan bias as a result of the reform.

The DID design is not without its flaws, because the two groups are not independent: many of the 'treated after' results will also be included in the 'untreated before' group, as the results after the reform are also results before another election, and in most cases, there are no back-to-back results, i.e. electoral systems are generally not changed during the electoral cycle right after a reform had been enacted. This raises questions about the violation of the assumption of independence, which is an important caveat to keep in mind, but which I will ignore during the analysis. Should the DID results significantly differ from conclusions drawn through other methods, this issue will be considered as a potential explanation.

Furthermore, I will also run independent unpaired t-tests[17], where the dependent variable is the change in partisan bias, and the two compared groups are reformer parties and other, non-reforming parties. These tests are suitable to determine if there is a significant difference between the means of these populations without any assumptions of causality. If the mean change in partisan bias is significantly higher for reformers than it is for non-reformers, H1 will be accepted.

Another way to quantify the change of partisan bias would be to use difference scores. That would mean that partisan bias at the current election would not be divided by partisan bias at the previous one, but the bias at the previous election would be subtracted from the bias at the current election. Difference scores would have a value of zero if the bias is unchanged, a positive value if the bias was increased after the reform, and they would be negative if the partisan bias of the party decreased. These would be somewhat easier to understand, because it is easier to see whether a number is positive or negative than recognizing whether it is higher or lower than one.

[17] The unpaired t-test is a statistical method of determining whether the mean of a variable (the dependent variable) is significantly different in one group compared to the mean of the same variable in another group.

Nevertheless, I opted to not to use them because the size of the bias is an important factor when analyzing the change. For example, if a party had a bias of 2 at an election, i.e. its seat share was twice the size of its vote share, and at the next election has a bias of 1.5, then its difference score would be -0.5. But the same difference score would be attributed to a party that had a bias of 1 at the election before the reform, and a bias of 0.5 after the reform. This latter change is much more dramatic, as conveyed by the variable that I opt for: its bias is halved from one election to the next, and the other party had a change of 1.5/2=3/4.

Differences scores therefore overemphasize the changes to parties that had a high partisan bias to begin with. Even a relatively small change in their partisan bias is represented as a large one by difference scores, and the method I propose (division) does not have this issue. This is particularly important, because winning parties tend to have a higher partisan bias, and these are exactly the parties that introduce the electoral reforms, since winning the elections gave them majorities in their respective legislatures. Thus, the changes to the parties the analysis is focusing on (electoral reformers) would be overemphasized by the use of difference scores, and that is why I will not use them.

These different methods should yield similar results, but I include all of them for different reasons. Simply looking at the share of reformers that improved their partisan bias is a simple, easy-to-understand overview. T-tests are important to determine if there is a significant difference in the mean change in partisan bias of reformers and non-reformers. OLS regressions are useful because they allow for the inclusion of multiple control variables later if the model needs further fine-tuning. Finally, difference-in-differences analysis is a method that is intended to evaluate the effects of an intervention on a group, which is precisely the kind of phenomenon I am investigating when testing whether the partisan bias of reformers is increased as a result of the electoral reform, but has flaws in this case due to the violation of the assumption of independence.

H2, i.e. whether electoral reformers have a higher partisan bias than non-reformers do, will be examined by way of OLS regressions. The dependent variable will be the partisan bias of parties, the independent variable will be a dummy indicating whether a party was a reformer or not. Independent unpaired t-tests comparing the mean partisan bias of reformers and non-reformers will also be run for a further check on the results of the analysis and an easier overview. Again, the dependent variable is the partisan bias of the party, the two compared groups are reformers and non-reformers. Regressions allow for the inclusion of control variables later if necessary, and in general are more widely used and more suitable to evaluate this hypothesis, but t-tests are a useful complementary way to check the results.

In order to test H3, i.e. whether reformers have a higher chance to be reelected than non-reformers do, logistic regression[18] will be used among parties that were members of the government when the election took place. The dependent variable will be whether the party in question was reelected or not, and the independent variable will be whether the party was a reformer or not. The results will show if reformers are reelected at a significantly higher rate than non-reformers are. This model also allows for the addition of potential control variables later. Additionally, I will also report some telling descriptive statistics, namely the reelection rates of reformers and non-reformers to show an easy-to-decipher way of the differences between the two groups, and calculate the Phi coefficients to see if the observed difference in means is significant or not.

H4a and H4b will also be tested via logistic regression. The dependent variable will be whether the party in question introduced a proportional reform or not. The choice of the independent variable is somewhat more complicated. Building on the work of Boix (1999), Andrews and Jackman (2005) use change in the effective electoral threshold as the independent variable, but that is a somewhat inaccurate measure for overall proportionality as it only accounts for the barrier of entry to politics. While it is true that the effective electoral threshold is generally lower in proportional systems, the

[18] Logistic (or logit) regression is similar to linear regression, but it is used if the dependent variable has only two possible values (also called a binary variable). The assumption in this case is not that the relationship is linear, but that it fits on a logistic function. The model evaluates whether the data fits this assumption or not.

two phenomena (barrier to entry and proportionality) are not the same, nor should they be measured with the same metrics.

Using standard measures of proportionality, such as the Gallagher Index (Gallagher, 1991) would not be adequate either, because they capture proportionality vis-à-vis a specific result, not the proportionality of the system in an abstract way. That means that if I used a proportionality index to identify proportional reforms, I would identify the proportionality of results, not the proportionality of electoral systems, and I would attribute the entire electoral result to be the design of the electoral reform, which would be inappropriate as well.

Therefore, I will test their hypothesis more directly by using the dummy variable of being a proportional reformer as the dependent variable in the analysis. This has the downside of not having a continuous variable as the dependent one in the model (as it would be the case if I used change in effective electoral threshold), and does not take the degree of the reform into account, but the direction is certainly well-captured by it.

In the case of H4a, the independent variable will be the one used by Andrews and Jackman (2005): partisan bias at the election preceding the introduction of the electoral system change. For H4b, the independent variable is the change in vote share compared to the previous election, i.e. vote share received at the election after the reform divided by vote share received at the election preceding the reform. If this number is greater than one, the party in question increased its popular support between the elections, if it is lower than one, than its popularity has declined during that period. A negative relationship between the independent and dependent variables would confirm the hypotheses.

In addition to the regressions, supplementary t-tests will also be conducted, with partisan bias at the previous election as the dependent variable for H4a and change in vote share the dependent variable for H4b. The two groups are proportional and majoritarian reformers. These tests will further illustrate the observed relationship.

For this analysis, only majoritarian and proportional reformers will comprise of the examined population, because neutral reformers and non-reformers are irrelevant from the perspective of the hypothesis, which is concerned with the direction of the reform. Since each party included in this population is either a majoritarian or a proportional reformer, this model will be suitable to accept or reject the hypothesis 'both ways': if previous partisan bias or the change in vote share can explain that a party will favor proportional reforms, it can also indicate that changes in the opposite direction prompt majoritarian reforms.

2.5.4 Control variables

First and foremost, I need to control for the effects that might cause issues with the independence assumption that regression tests require to produce reliable results. The problem is that electoral results of parties that compete in the same election are certainly not independent. For example, if one party receives 40% of the vote, then the rest of the parties only have 60% to compete for. Thus, the result of one party affects the results of other parties in that same election. Likewise, the seat shares are also interrelated in each election and consequently violate the independence assumption. For instance, if one party receives 60% of the seats, the rest can only compete for the remaining 40%.

That means that both components of my main dependent variable (partisan bias) are not independent for parties competing in the same election. Not only is that true, but bias itself has its own problems with independence in the same election, especially in majoritarian systems. Those systems tend to reward the winner of a given election, and whether a party wins or loses can have a significant effect on its partisan bias. Generally speaking, a party that garners 40% of the vote will have a very favorable partisan bias if its remaining best-performing rival has 30%, but will have a much lower bias if there is a party that gets 50% of the vote in that same election due to the 'winner-take-all' nature of majoritarian systems. Naturally, this is a general statement, and a lot depends on the exact features of the system and the territorial distribution of the votes, but in general, partisan bias is not independent from the results of other parties competing in the same election.

Since both components of partisan bias and partisan bias itself are all related to each other for parties competing in the same election, I need to control for that effect in my models. I will do that by controlling for fixed effects in most of the DID and OLS regression models that I conduct where the dependent variable is partisan bias. Fixed effects models are designed exactly to mitigate the kind of issues I mentioned in the above paragraphs. In my case, fixed effects will be introduced for each election, i.e. in each model each separate election will be used as a dummy variable, so that for parties in the same election the same variable will have a value of 1, and for all the other parties that value will be 0. This means that each of my models will have hundreds or even thousands of variables in it, but I will not present the results for the election dummy variables as they are irrelevant in and of themselves, but the correction they provide for the actually meaningful results are crucial for the models to be reliable.

Fixed effects of elections are not going to be used in the models that I run only within the population of leading cabinet members. There is only one leading cabinet member after each election, so there is no interdependence for different parties competing against one another, making the inclusion of fixed effects not only superfluous, but also damaging for the analysis.

Other than introducing each election as a fixed effect dummy, in order to distinguish potentially different contexts that could affect the results, I will test the hypotheses separately in various geographical regions, i.e. throughout the seven regions identified in the DES: Latin America, Middle East and North Africa, Oceania, Eastern Europe and post-Soviet states, the West, Asia and Sub-Saharan Africa. This should clear up potential cultural differences that could affect the effectiveness of electoral engineering.

Furthermore, I will also conduct separate tests base on the scale of the reform (small-scale, mid-scale or large-scale) to identify whether smaller reforms are more effective than larger ones. Finally, as mentioned earlier, I will also test the different effects of electoral reforms based on the direction of the reform (whether it was majoritarian, proportional or neutral). These distinctions will allow me to identify the contexts that make electoral engineering efficient and will be useful both for the interpretation of the results observed here and for establishing the direction further research should take.

Other than the distinctions of geographical region, scale and direction of the reform, my main control variable for the hypotheses that deal with partisan bias as the dependent variable in one form or another (H1, H2), will be the size of the party, i.e. vote share garnered at the election. It is common knowledge that larger parties receive disproportionately more seats than smaller ones, specifically in majoritarian systems (Cox, 1997), but also in proportional ones due to the existence of electoral thresholds and the impossibility of a perfectly proportional allocation of seats. Therefore, the larger a party is, the higher partisan bias it might expect in general, and party size is thus a reasonable control variable to include in the analysis.

This relationship between vote share and partisan bias is confirmed by the data, although not in an overwhelming way. Table 2 shows OLS estimates of this relationship in different electoral systems[19]. The results demonstrate a somewhat weak but significant relationship that is stronger in majoritarian systems than in proportional ones, and – somewhat surprisingly – is the strongest in mixed systems, but the differences between the electoral families are not staggering.

[19] For the purposes of this analysis and any further analysis involving partisan bias as a variable, outliers were discarded from the population. A case was defined as an outlier if it had a partisan bias above 2, i.e. its seat share was more than twice as large as its vote share was. That is about 2 standard deviations above the mean (mean is 1.03, standard deviation is 0.474). These are almost exclusively regional parties in majoritarian systems that won a handful of seats and did not contest other ones, and hence have a very low national vote share while having a few MPs. Their inclusion would severely distort the analysis of the hypotheses.

OLS models of vote share and partisan bias	N	Constant (SE)	Vote share (SE)	Adjusted R squared
All systems	4217	0.887*** (0.008)	0.005*** (0.000)	0.069
Majoritarian systems	1128	0.809*** (0.020)	0.006*** (0.001)	0.085
Mixed systems	728	0.822*** (0.019)	0.007*** (0.001)	0.102
Proportional systems	2361	0.937*** (0.004)	0.004*** (0.000)	0.058

Table 2: OLS estimates of vote share predicting partisan bias in different electoral systems[20]

[20] Legend for regression tables:

- N: number of cases.
- Constant: value of the dependent variable at the baseline level, i.e. if the value of all independent variables were 0.
- SE: standard error of the estimate, i.e. average distance of observed values from the regression curve.
- Adjusted R squared: adjusted coefficient of determination, i.e. the proportion of the variation that is explained by the model, adjusted for the number of independent variables included in the model. The higher the adjusted R squared, better the model explains the examined relationship.
- p value: probability of that the observed relationship occurred by random chance. The lower the p value, the greater the statistical significance of the independent variable in question in the model. P value levels are denoted by stars in these tables: *: p≤0.05 **: p≤0.01 ***: p≤0.001.

2.6 Analysis

2.6.1 Analyzing H1: The change in partisan bias for reformers

Table 3 shows descriptive statistics of the change in partisan bias of reformers. It shows mean and median partisan bias change, its standard deviation, how many of the reformers saw their bias increase after the reform, how many had it decreased, and finally, the percentage of cases that improved their partisan bias after the reform. The second row shows these figures for all reforming parties, while the third one does the same for only main reformers, i.e. the parties with the highest seat share in the government.

Change in partisan bias	Mean	Median	Standard deviation	Im-proved	Wor-sened	Im-proved%
All reformers	0.972	0.977	0.419	195	298	39.6%
Main reformers	0.941	0.977	0.242	114	197	36.7%

Table 3: Descriptive statistics of the change in partisan bias (current partisan bias divided by partisan bias at the previous election) for electoral reformers

The data indicates that reformers have not benefited from enacting electoral system change. A majority of them (more than 60%) experienced a decline in their partisan bias after the reform, but in general, the losses do not seem to be severe at all, as the mean and the median of the change suggests that their bias remained close to the previous one on average.

As shown by Tables 14 and 15 in the Appendix, the data is remarkably consistent across the different subgroups: electoral reformers saw their partisan bias slightly worsen on average regardless of the region of the reformer or the type of the reform introduced. It is important to note that the West, a region that consists mostly of established democracies, had the most cases and the smallest standard deviation, was not an outlier either, indicating that the pattern seems to hold in the long run and it is not just the inclusion of cases from unconsolidated, young democracies that distort the results.

The general robustness of these results mean that based on pure descriptive statistics, there is no evidence that reformers improved their partisan bias as a result of a change in the electoral system. There are several parties that did, but the majority of reformers in any subgroup did not.

Tables 4 and 5 further confirm this observation. They show the results of t-tests comparing the mean partisan bias change of reformer parties to that of non-reformers. In Table 4, the comparison is made with all non-reforming parties, whereas the comparison group in Table 5 is all non-reforming government parties.

Comparison of mean change in partisan bias	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All reformers	493	0.971	0.999	-4.771	1807.3	0.001
Other parties	2395	1.104	0.419			
Main reformers	311	0.941	0.242	-6.699	1867.9	0.001
Other parties	2577	1.099	0.977			

Table 4: Comparing the means of change in partisan bias of reformers and non-reformers[21]

Table 4 indicates that the partisan bias of non-reformers changed in a significantly better direction than the bias of reformers did (Tables 16 through 19 in the Appendix

[21] Legend for tables summarizing the results of t-tests:

- N: number of cases
- t: value of the t-statistic.
- p: p-value.

corroborate this with results of reforms of different types and results from different regions). This is interesting, but is overall not enough to convincingly test the validity of the hypothesis. This is because reformer parties, being members of government, generally have a larger partisan bias to begin with (partly due to having a higher vote share at the previous election), and it is therefore more difficult for them to improve their partisan bias in a significant way. Non-reformer parties are generally smaller, and as proven by Table 2, smaller parties tend to have a smaller base of partisan bias, which is easier to increase. This indicates that only other government parties should be treated as benchmarks; reformers should only be compared to their non-reforming governing peers, especially if there is no control for the size of the party in the model. I will act accordingly in further analyses.

Comparison of change in partisan bias	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All reformers	493	0.971	0.419	0.995	984.3	0.320
Other government parties	786	0.949	0.388			
Main reformers	311	0.941	0.242	0.719	796	0.473
Other leading government parties	487	0.928	0.928			

Table 5: Comparing the means of change in partisan bias of reformers and non-reformers

That is why Table 5 is more important for testing H1. Since the comparison is made with other government parties, the base partisan bias is similar across the groups, and the comparison is thus more applicable. The results show only insignificant differences (due to the high p-values), meaning that the partisan bias of government parties moved in a similar direction overall regardless of whether the party was a reformer or not.

This is further confirmed by the estimates resulting from the same comparison made in the various subgroups created based on the type of the reform and geographical differences, presented in Tables 20 through 23 in the Appendix. This is very much in line with the indications of the descriptive statistics, and suggests no particular electoral benefit from the introduction of an electoral system change.

The results of OLS regressions are presented in Table 6. It shows the coefficients in both the model where the main independent variable is being a reformer party (Row 2), and the one where the main independent variable is being a leading member of a government that enacted electoral reform (Row 3). Just as in Table 5, reformer parties are compared to their incumbent government peers, i.e. leading reformer parties to non-reforming leading government parties and all reformer parties to all non-reforming government parties.

OLS models of reform and partisan bias	**N**	**Constant (SE)**	**Reformer dummy (SE)**	**Previous bias (SE)**	**Vote share (SE)**	**Adjusted R squared**
All reformers	1296	0.639*** (0.231)	0.230 (0.252)	0.186*** (0.037)	0.004*** (0.001)	0.254
Main reformers	811	0.738*** (0.050)	0.016 (0.017)	0.202*** (0.037)	0.003*** (0.001)	0.061

Table 6: OLS estimates of being a reformer predicting partisan bias, controlling for partisan bias at the previous election, vote share and fixed effects (within the population of government parties and leading government parties, respectively)

The estimates validate the findings of the t-tests and the descriptive statistics: being a reformer does not have a significant effect on partisan bias change. Whereas vote share (this was already shown in Table 2) and partisan bias at the previous election (this was fully expected) have a significant and positive relationship with partisan bias, reformers do not have an outsized bias if those two variables are controlled for. That means that electoral reform does not lead to a significant increase in partisan bias. This is further corroborated by the estimates in the various subgroups presented in Tables 24 through 27 in the Appendix. These consistently show similar figures to the ones shown in the table below.

Finally, the results of the difference-in-differences analysis are presented in Table 7 below. First, all reformers are compared to all non-reforming government parties (Rows 2-3), then leading reformers are compared to the largest members of government that did not initiate electoral change (Rows 4-5). First, the number of cases in the various groups (‘treated’ and ‘untreated’, before and after) are presented, then the difference in means between the groups before and after the ‘treatment’ (controlling for vote share) are shown, and then, the DID estimator, i.e. the ‘treatment’ effect can be seen in the seventh column.

DID analysis of reform and change in partisan bias	N (before)	N (after)	Diff. before reform (SE)	Diff. after reform (SE)	DID (SE)	R^2
All reformers	495	496	0.018 (0.064)	0.023 (0.064)	0.005 (0.025)	0.41
Other parties	796	796				
Main reformers	312	316	0.057 (0.054)	0.072 (0.054)	0.014 (0.023)	0.58
Other parties	488	498				

Table 7: Estimates of the difference-in-differences analysis of change in partisan bias among reformers and non-reformers, controlling for vote share and fixed effects (within the population of government parties and leading government parties, respectively)[22]

[22] Legend for tables summarizing the results of DID analyses:

- N (before): number of cases in the 'before' group, i.e. cases before the reform took place.
- N (after): number of cases in the 'after' group, i.e. cases after the reform took place.
- Difference before the reform: difference in the mean of the dependent variable, i.e. partisan bias between the two groups, i.e. reformers and non-reformers before the reform took place.
- Difference before the reform: difference in the mean of the dependent variable, i.e. partisan bias between the two groups, i.e. reformers and non-reformers after the reform took place.
- SE: standard error.
- R^2: coefficient of determination, i.e. the proportion of the variation that is explained by the model, adjusted for the number of independent variables included in the model. The higher the adjusted R squared, better the model explains the examined relationship.

The results presented in the table indicate no significant relationship, and the model as a whole is not significant either. The differences in partisan bias before or after the reform are not statistically significant, meaning that there is no meaningful difference in how much electoral systems benefited reformers and non-reformers. This is further substantiated by the mostly similar results in the various subgroups, presented in Tables 28 through 31 in the Appendix.

Table 30 has an interesting result though: majoritarian reformers seem to enjoy a statistically significant, mild increase in partisan bias. This is a result that could be expected, because majoritarian reforms are expected to widen the differences of partisan bias between parties. If majoritarian reformers designed the new systems because they were confident in winning the upcoming election, their plan seems to have worked based on this. Nonetheless, since all other ways of examination showed no significant benefit for even majoritarian reformers, I treat this result with caution.

H1 can therefore be rejected based on the results of all four methods applied in this section. Reformers do not experience a detectable and statistically significant increase in their partisan bias at the election after the reform was enacted. Neither are they truly harmed by being reformers, at least the data does not indicate any significant negative effect of introducing an electoral reform on partisan bias change either. Based on the analysis, in general, electoral engineering simply does not seem to be an effective way to increase the seats-to-votes ratio for a political party.

2.6.2 Analyzing H2: the partisan bias of reformers compared to non-reformers

Table 8 summarizes the results of the OLS regressions of partisan bias within the population of all parties. After controlling for vote share, the reformer dummy variable is not significant in the model, which suggests that reformers in general do not have a higher bias than non-reformers. For the estimates in the subgroups, see Tables 32 through 35 in the Appendix.

OLS models of reform and partisan bias	N	Constant (SE)	Reformer dummy (SE)	Vote share (SE)	Adjusted R squared
All reformers	4224	0.637** (0.216)	0.022 (0.018)	0.008*** (0.000)	0.131
Main reformers	4224	0.633** (0.008)	0.028 (0.019)	0.005*** (0.000)	0.130

Table 8: OLS models of being a reformer predicting partisan bias, controlling for vote share and fixed effects

The result that reformers in general appear not to have a significantly higher partisan bias than other parties seems in general sufficient to reject H2, but it is also interesting to look at the same results when reformers are compared to their non-reforming peers, i.e. other governing parties. The partisan bias of former government parties might in general be lower due to a backlash from voters after they are in power, making this separate analysis a necessary check on the results presented in Table 8.

Table 34 in the Appendix shows some interesting results for neutral and proportional reformers. Proportional reformers seem to have a decreased partisan bias compared to all other parties, and neutral reformers seem to perform slightly better than other parties. This means that electoral engineering does work for certain parties, specifically those that introduce small-scale, not overly salient changes to the electoral system (neutral reformers), while reforms that make the system more proportional hurt the large parties, including the cabinet members that introduce them. On the other hand, the partisan bias of majoritarian reformers is not significantly different from that of other parties. This result is interesting and will be discussed further in the discussion chapter.

Nonetheless, the results in Table 9 make the rejection of the hypothesis stronger. The table shows the results of the same analysis as Table 8 did, but within the population of incumbent government parties; all government parties in Row 2 and leading government parties in Row 3. In other words, in Table 9, reformers are compared to their governing peers.

OLS models of reform and partisan bias (government)	N	Constant (SE)	Reformer dummy (SE)	Vote share (SE)	Adjusted R squared
All reformers	1296	0.861*** (0.233)	0.216 (0.258)	0.004*** (0.001)	0.222
Main reformers	811	0.981*** (0.022)	0.019 (0.018)	0.003*** (0.001)	0.030

Table 9: OLS models of being a reformer predicting partisan bias, controlling for vote share and fixed effects (within the population of government parties and leading government parties, respectively)

There is no significant effect observed here for being a reformer. It appears that electoral engineers do not have a higher partisan bias than non-reforming incumbent government parties do, even if only the population of cabinet members is examined.

The results for different types of reformers and reformers from different regions largely support this null result as well. In the Appendix, Tables 36 through 39 show the results via OLS regression, while Tables 40 through 43 test the same hypothesis using unpaired t-tests. T-tests are relevant here and not in the analysis that compared reformers to all other parties because they do not allow for control variables, such as vote share, and large parties, such as the ones in government, are generally expected to have a better partisan bias than other parties do. Therefore, any effect that t-tests would show in a general comparison would be meaningless due to the fact that vote share distorts the results. Conversely, comparing government parties largely mitigates this issue, and t-tests are meaningful in this case.

Null results are overwhelming in the analysis of the subgroups, regardless of the method. The advantage neutral reformers had, and the disadvantage proportional reformers did in the previous analysis is all gone, and it seems that may have been somehow related to their incumbency advantage.

This prompts me to claim that electoral reformers do not appear to have a significantly higher partisan bias than others, and especially non-reforming government parties do. H2 can consequently be rejected.

2.6.3 Analyzing H3: reelection rates for reformers and non-reformers

Even if partisan bias seems to not improve in a significant way as a result of electoral engineering, and electoral reformers appear not to experience a significantly higher partisan bias than their peers do, if designing a new electoral system can lead to higher likelihood of reelection, it might be proven to be a useful tool of negative power consolidation.

Tables 10 and 11 indicate that electoral engineers do not have an advantage when it comes to getting reelected. Table 10 shows the descriptive statistics of reelection among reformers and non-reformers, as well as the Phi coefficient[23] for comparing the means of the two. Table 11 shows the estimates from the logistic regression model where the dependent variable is reelection, and the main independent variable is being an electoral reformer.

[23] The Phi coefficient is a measure of association between two binary variables, i.e. it measures how closely related the values of two variables (that have two possible values) are in the sample.

Comparison of reelection rates	**N**	**Re-elected**	**Not re-elected**	**Re-election%**	**φ**	**p**
All reformers	512	314	198	61.3%	-0.013	0.659
Non-reforming government parties	844	529	315	62.7%		
Main reformers	324	187	137	57.7%	0.003	0.938
Non-reforming leading government parties	524	301	223	57.4%		

Table 10: Comparing the reelection rates of reformers and non-reformers

Logit models of reform and reelection	**N**	**Constant (SE)**	**Reformer (SE)**	**Reformer odds ratio**
All reformers	1356	0.518*** (0.071)	-0.057 (0.115)	0.944
Main reformers	848	0.589*** (0.091)	-0.005 (0.147)	0.995

Table 11: Binary logistic estimates of the effects of being a reformer on the probability of reelection (within the population of government parties and leading government parties, respectively)

Both of the tables show results that imply no significant difference between the reelection probabilities of reformers and non-reformers; both types of parties are getting reelected at an incredibly similar rate. Furthermore, the findings of the same analysis conducted within the various subgroups (presented in Tables 44 through 51 in the Appendix) are also quite consistent with the conclusion drawn from Tables 10 and 11.

The only result worth noting from the analysis of the subgroups is the one of large-scale reformers: the difference between the means is significant, and even if it is not a significant variable in the logit model, it is visible that reformers who change the electoral system family have been reelected at a considerably lower rate than other government parties. This would suggest that while smaller reforms do not have a meaningful impact on the behavior of voters, large-scale changes to the electoral system could be more salient issues in campaigns and the consequent electoral backlash could be hurting reformers in those cases.

Another possible explanation is that large-scale electoral reform is generally enacted by unpopular governments as an attempt of last resort to preserve as much power as they can, and since they do not see any realistic chance at getting reelected, they introduce a new system aimed at preventing the total eradication of their parliamentary group. Alternatively, in the case of the introduction of a publicly demanded new system, it could often be a largely failed endeavor to regain some of their lost popularity. However, due to the relatively small number of large-scale reforms in the dataset, this finding is not something I would be overly confident about without confirmation from further, more in-depth case studies.

Nevertheless, the results demonstrate that there is no clear advantage secured by electoral reformers when it comes to getting reelected. The general takeaway is that electoral reform offers neither benefit, nor disadvantage to the parties that introduce it; they are just as likely to get reelected as their non-reforming peers are. This is generally true with the possible exception of large-scale reformers, who seem to be harmed by passing an electoral reform. Overall, H3 can be confidently rejected based on the findings of the analysis.

2.6.4 Analyzing H4a and H4b

The first three hypotheses, which are strongly related to my main research question, have all been rejected. That still leaves the question of whether reforms are initiated as a response to uncertainty, and if as Andrews and Jackman (2005) claim, proportional reforms are a result of parties with a small partisan bias hedging against the uncertainty of more majoritarian systems. Whereas the confirmation of this claim could help us understand why H1-H3 were rejected in general, it would not provide an explanation for observing no significant difference between the partisan bias of proportional and majoritarian reformers. It would thus be somewhat surprising, but certainly not improbable, if H4a or H4b were accepted.

Table 12 provides the estimates of the model testing H4a, i.e. whether a lower level of partisan bias at the election preceding the enactment of the reform led to a higher likelihood of proportional reforms. The findings do not confirm the hypothesis; there is no significant effect of previous partisan bias on the direction of the reform. In the Appendix, Table 52 indicates that this is true in every subgroup, and the results presented in Tables 54 and 55 also corroborate this through a comparison of means via t-tests.

Logit model of previous partisan bias and the direction of the reform	**N**	**Constant (SE)**	**Previous partisan bias (SE)**	**Previous partisan bias odds ratio**
Main reformers (majoritarian or proportional reforms)	198	0.823 (0.861)	-0.405 (0.732)	0.667

Table 12: Logit model of partisan bias at the previous election predicting the direction of the reform (population of all leading majoritarian and proportional reformers)

This proves that H4a can be rejected, and the pattern identified by Andrews and Jackman (2005) cannot be observed in the dataset. That means that the uncertainty of having a relatively low partisan bias is not the primary factor compelling parties to introduce proportional electoral reforms, and that majoritarian reformers are not necessarily emboldened by their comparatively high partisan bias in their decision to enact changes to the electoral system. That disproves one possible rational explanation for differences in the direction of electoral reforms.

The confirmation of H4b could offer a similar, alternative explanation for these questions. The results of the logistic regression model that predicts the direction of the direction of the reform based on the change in vote share of the reformer party are presented in Table 13.

Logit model of change in vote share and the direction of the reform	**N**	**Constant (SE)**	**Change in vote share (SE)**	**Vote share change odds ratio**
Main reformers (majoritarian or proportional reforms)	198	0.346 (0.385)	0.023 (0.398)	1.024

Table 13: Logit model of the change in vote share predicting the direction of the reform (population of all leading majoritarian and proportional reformers)

Once again, the results do not indicate a significant relationship between the variables. This is generally true across the various subgroups as well, and comparisons of means further confirm these results (for these estimates, see Tables 53, 56 and 57 in the Appendix). The only significant result is the one observed among the reformers in the Middle East and North Africa, but that shows the opposite of the relationship that would be expected, i.e. the vote share of majoritarian reformers decreased significantly more than that of proportional reformers. Due to the extremely low number of cases in that region (15), it is safe to ignore that specific result.

As no significant relationship could be demonstrated based on the analysis, H4b can be rejected as well. The rational motivation of the direction of electoral reforms, namely that parties with declining popularity introduce proportional reforms to limit their losses, cannot be confirmed based on the data, which means that alternative explanations need to be pursued.

2.7 Discussion and possible areas of further research

2.7.1 Implications of the analysis

All five hypotheses have been consistently rejected through the analysis. Electoral engineering does not seem to provide any electoral payoffs for the reformers: they do not disproportionately increase their partisan bias compared to the previous election, their partisan bias in the new system is not significantly higher than that of their peers, and their reelection rates are strikingly similar to those of non-reformers. In other words, electoral systems do not offer any advantage to those who designed them.

The only exception for the null findings was the case of neutral and proportional reformers with regards to H2. Neutral reformers had a small, but statistically significant advantage in their partisan bias when compared to non-reformers, while proportional reformers experienced the opposite; they had a small but still significant disadvantage compared to other parties. This result was only true when reformers were compared to all other parties though, and disappeared once the analysis was conducted only on the population of cabinet members, indicating that the incumbency effect could distort the results somewhat.

Nevertheless, this is an interesting finding and lines up well with theory. The fact that neutral reformers performed better than other parties aligns well with the thesis of McElwain (2007) and Tan and Grofman (2016) that miniscule alterations to the electoral system that are not heavily politicized could 'fly under the radar' of voters and avoid any backlash for partisan engineering, raising their effectiveness. Conversely, larger-scale reforms generate heavier electoral backlash that negate any advantage the new system was supposed to bring about for the reformers. This backlash could be more sever in the case of majoritarian reforms that are designed to increase the difference between the partisan bias of winners and losers. Often, these reforms might be subject to heavier public scrutiny and reformers who expected to be the winners might turn out to be losers as a result of this public reaction, making majoritarian reforms ineffective.

The other statistically significant result, the disadvantage experiences by reformers who make the system more proportional, is also in accordance with the theoretical expectations. Proportional reforms are more likely to be introduced as a result of public demand or for ideological considerations to create a 'more fair' system, where seat shares and vote shares are more closely aligned. Thus, the rationale for these reforms is generally less partisan, and the lack of partisan gain is consequently unsurprising.

The largely non-partisan motivations for proportional reforms could also partly account for the null results for H4. Based on the analysis, the direction of electoral reforms cannot be explained by the rational motivation of reformers, i.e. proportional changes to the electoral system cannot be attributed to the leading government party mitigating uncertainty it expects either due to the low partisan bias it experienced at the previous election or as a result of diminishing popularity.

While there were some interesting positive results for neutral and proportional reformers, these effects were largely negated in other aspects of the analysis: there was no real effect of the reform for the reelection chances of these reformers and their partisan bias did not significantly change compared to their previous bias before the reform, so these are not sufficient to make a general claim that neutral reforms are affective, while proportional reforms are detrimental for reformers. The otherwise consistent null results suggest that electoral engineering for partisan gain is either actually not widely practiced and alternative reasons lead to electoral reform, or that those who engage in it are not particularly successful, or a combination of the two. There are several different ways to explain either reason, which I will lay out in the following sections.

2.7.2 Possible reasons for the lack of partisan electoral engineering

First, the fact that electoral reforms generally come about for a different reason than electoral engineering for partisan gain is backed up by several claims in the literature, sampled in Section 2.2.5. It is possible that cultural modernization theory, as described by Norris (2004: 16-17) is a better way to approach electoral system change than rational choice models, and it is mostly underlying developments in political culture that dictate reforms, not the partisan motivations of the parties in government.

Plenty of studies found that cultural pressures, both domestic and international, have a significant impact on whether the electoral system is reformed and if so, how it is designed (Bol et al. [2015]; Norris [2011]; Blais et al. [2005]). These studies and those examining the same phenomena have often pointed out that partisan self-interest is still a significant driver of reforms, but maybe in general, it is only a secondary consideration that does not allow reformers to truly tilt the system in their favor, and the system is mainly designed in accordance with the demands of the pressuring public.

This could be especially true in the case of proportional reforms, which indicated slightly negative results for reformers when compared to non-reforming parties, making the case for this explanation stronger. On the other hand, neutral reforms, which are small-scale by definition, are never a result of public demand, and this explanation should fail in their case – as it does with regards to H2, where it seemed that neutral reformers have a higher partisan bias than other parties did. Further research should consequently pay attention to whether there was public demand for the reform or not; in the former case, electoral engineering probably does not work, in the latter, it may.

Another possible explanation is that while parties are pursuing their own self-interest when pushing for electoral change, they are primarily focused not on future electoral payoffs, but on the adoption of a system that is in accordance with their ideological beliefs, as Bol (2016) observed. That would mean that in the case of electoral systems, political actors are not office-seeking, but either vote-seeking or policy-seeking (for more on this distinction in general, see Müller and Strøm, 1999), which is somewhat surprising, considering electoral systems are not major policy areas. If this is indeed the case, and parties are not primarily office-seeking even with regards to electoral systems, that would lend plenty of support to the idea of understanding political actors as generally vote-seeking or policy-seeking.

Vote-seeking or policy-seeking behavior is probably more visible in the case of proportional reforms. Proportional reforms seem to have harmed reformers in at least one aspect (their partisan bias is lower than that of other parties), which indicates that these kinds of reforms are indeed introduced to placate voters who demand more proportional systems, as it was in the case of New Zealand (Norris, 2011). Otherwise, parties could have a platform that calls for a proportional reform, and they thus pursue it once in government not for a gain in partisan bias, but because that is a policy goal for them.

An additional possibility to explain the lack of electoral engineering practiced could be the fact that governments are not in full control of the electoral reform process. Public pressure groups, independent experts or opposition parties could all be majorly involved in the shaping of the new electoral system due to either legal obligations or as a result of a perceived strain from the public. This causes governments to not be the sole or the main architects of electoral systems, and while their interests are represented in the reform to a degree, as the reform could not pass in the legislature otherwise, the demands of other actors are included just as much, and that prevents the possibility of electoral gains for the government. This would be consistent with findings of Walter and Emmenegger (2019) and Brady and Mo (1992).

A variation of this explanation that could be linked to cartel party theory (Katz and Mair, 1995) is that incumbent political parties are designing systems that are aimed at the consolidation of their collective power, not the power of a singular actor. That means that the lack of partisan electoral engineering can be attributed to the fact that the true opponent that governments are attempting to suppress through electoral engineering are not opposition parties, but extraparliamentary actors that could prove to be a threat to the status quo. In that sense, the opposition is not an adversary, but an ally of governments, and they design electoral systems in order to maintain the alternation of their rule.

Self-interested, office-seeking motivations could still lead politicians when they design electoral reforms, but, instead of the electoral success of their respective parties, it could be individual influence that they are attempting to maximize. Thus, it is not the partisan bias or the reelection of the government that should be examined when evaluating the success of electoral engineering, but the reelection rates and career prospects of the specific architects of the new electoral system.

As Renwick (2018: 120) points out, individual interests may diverge from that of the collective interests of the party, and certain politicians could benefit from a decrease in partisan popularity or a decline in partisan bias. For example, they could potentially design electoral rules that are aimed at eliminating internal rivals from parliament, consolidating intra-party power. Pilet (2008) corroborates this hypothesis by pointing out that in certain cases, electoral rules were designed by party leadership to weaken backbenchers. Treating parties instead of individual politicians as the architects of electoral systems could therefore have been an issue that led to the negative results.

2.7.3 Possible reasons for the failure of partisan electoral engineering

The other route of making sense of the findings in the analysis is not to discard the axiom that electoral systems are designed by political actors who are seat-maximizers, but to maintain that assumption and inferring from the results that they are quite ineffective at enacting electoral reforms that are beneficial for them.

This ineffectiveness could be ascribed to the vast uncertainty surrounding electoral reforms, as described in Section 2.3.2. In order to create a perfect electoral design for one particular political actor, the following parameters need to be known or at least approximated: the number and profile of rivals (i.e. the parties realistically competing for seats), the approximate relative popularity of the parties, the geographical distribution of votes, the psychological effects of enacting the reform, and the exact mechanical effects of the potential electoral systems. As Shvetsova (2003) illustrates with examples from Eastern European countries, often times, even the most easily perceptible of these factors, the mechanical effects of various electoral systems is not precisely known by the reformers. Since the other factors are even more difficult to predict, it is easy to imagine that the uncertainty reformers face is so tremendous that it renders most attempts of electoral engineering ineffectual.

It is also possible that the act contingencies of electoral reforms are generally underestimated by reformers, and voters are punishing governments that seem to be using their power to increase their future electoral payoffs. The fact that the analysis of H3 in Section 2.5.3 showed that large-scale reformers seem to have a below-average rate of reelection indicates that this is at least partly the case, and the fact that neutral reformers experience a higher partisan bias than other parties do point in the same direction. Wholesale alterations of the electoral system are almost always salient issues in campaigns, and if the reform is enacted without the approval of external groups or the opposition, allegations of power abuse can be expected from political opponents. These accusations might seem credible for a significant portion of the electorate and electoral backlash could ensue, which not only diminishes the popularity of the government, but also upends the political circumstances that reformers were basing electoral design upon, further adding to the uncertainty described in the previous paragraph.

Nevertheless, smaller-scale reforms are not expected to always be salient issues in the political arena, and therefore simply expecting punishment by the voters for each and every reform is not realistic. However, electoral reforms could also be a part of a larger consolidation attempt used in conjunction with other tools. It is also possible that voters are reacting negatively to electoral reforms not necessarily because the adoption of these reforms is a salient issue itself, but because other, more politically sensitive tools are used at the same time by the consolidating actors and voters are reacting to those as opposed to having a direct reaction to the reforms themselves. In other words, smaller-scale reforms are good indicators of larger power consolidation attempts in general and that is why we observe electoral backlash when they are introduced.

Another possible aspect of act contingencies that could be unexpected by reformers is adaptation. If voters and rival parties adapt better to the new system than the reformers expected, that could lead to the ineffectiveness of partisan reforms. For example, a party could design a specific electoral system that disadvantages a fragmented political side, but that could lead to the fragmented side coalescing and show a united front, negating the expected electoral payoffs for the reformer.

Alternatively, in some cases, legal or constitutional obstacles could hinder the effectiveness of partisan electoral engineering. These could include limitations to the scope of the reform, i.e. statutes that prohibit certain changes. Another such legal constraint could be a requirement of a supermajority to alter electoral rules, i.e. more than 50% of the legislature needs to approve changes to the electoral system. That could mean that compromises need to be made by the reformers throughout the reform process, causing the effectiveness of electoral engineering to dwindle with each step. Another legal requirement could be the involvement of independent bodies that the reformers do not have control of. These independent groups could decide to counteract any electorally advantageous aspects of the changes for the reformers, dissipating the benefits of electoral engineering.

Even if there are no strict legal constraints that limit their room for maneuver, the judiciary could still be an influential actor in shaping the electoral reforms, possibly negating the advantages created by electoral engineering. Cases where judges significantly altered the design of electoral systems, and markedly influenced the reform process are reviewed by Williams (2005), Katz (2011) and Baldini and Renwick (2015) among others, proving that this is a phenomenon that is quite common in various different political settings.

Moreover, beyond the cases of explicit intervention by the judicial branch, there certainly are instances where reformers put forward a milder version of their proposal expecting that the 'perfect system' they would design would not pass judicial review. Thus, the mere existence of possible legal hurdles might induce ineffectual reforms without any active interference in electoral engineering.

Finally, the issue of collective action could also lead to partisan electoral engineering to be ineffectual. As Renwick (2018: 123) argues, translating the various different interests of politicians into a single outcome that is generally beneficial for the party is not an easy and straightforward task, and the discordant choir of party members could bring about a reform that is not advantageous for the reformer party as a whole in its final form.

2.7.4 Possible inadequacies of the analysis

The possible inadequacies of the research design should also be considered as reasons for the negative results. First, the identification of electoral reforms could have been flawed: it is possible that the mere fact that there was a change in the electoral system does not necessarily mean that electoral engineering was attempted. While this method of case selection allowed for a comprehensive, large-n comparative analysis, it caused the dataset to have a large variety of electoral changes all coded as a case for electoral engineering, which does not allow for distinguishing between actual attempts of power consolidation and electoral changes that cannot be treated as such.

Nevertheless, even if this is true, the analysis demonstrated that government parties are generally not beneficiaries of electoral system change, which is an interesting if limited finding on its own, and in addition to the potential explanations listed in the previous two sections, it could mean that partisan electoral engineering is not practiced in every electoral reform process. That conclusion may lead to the softening of the rational-choice seat-maximizer model widely used in the literature of electoral reforms.

The database itself had certain shortcomings that could have affected the results. For instance, the minor, peripheral modifications of electoral rules that McElwain (2007) and Tan and Grofman (2016) highlighted as extremely effective tools of partisan electoral engineering, as well as a lot of cases of redistricting, are not included in the analysis as cases of electoral reform. It is possible that these types of changes are more effective than the larger, more visible ones I analyzed and electoral engineering is indeed an effective tool, but it has to be applied on the smallest of scales to prevent the negative act contingencies of electoral reform.

Furthermore, the main actors could have been misidentified in plenty of cases by the acceptance of the widely-used assumption that it is mainly the government that design the reforms. As pointed out in Section 2.7.2, there are other actors that could wield even more influence than the government throughout the process, and therefore it is not only the interests of the parties in government that are represented in the final form of the electoral system. Additionally, in countries with a multi-level system of government, local authorities, such as states or provinces might be in charge of determining electoral rules, not the national-level government, and electoral rules might be in accordance with the preferences of these local actors instead of the national ones examined in this dissertation.

2.7.5 Interpreting the null findings

It is also important to discuss the nature of the null results themselves. According to Rainey (2014), even if the research design is sound, there are at least three ways to interpret null empirical findings. Firstly, and Rainey (2014: 1085-1086) points out that this is very often the case when political scientists conduct cross-country analyses of elections, the sample size might be too small to demonstrate statistically significant effects. These effects might exist in a 'superpopulation', i.e. if there were enough cases, a statistically significant relationship would be observed, but the dataset simply did not have enough cases. Since democratic elections have only been conducted in the last 100-150 years, and only occurred in a handful of countries for a long time, there simply are not enough national elections to have enough cases to build a database with enough cases for any significant effect.

In the case of electoral engineering, it could be that the hypotheses should all be accepted, but this will not be clear until a lot more elections take place and are entered into the database. I had less than 5,000 cases in my dataset, even though I included every democratic election since 1974. This number is still quite low for a large-n dataset, and this is especially true for the subgroups. In certain regions (e.g. Oceania, Middle East and North Africa), there were only a handful of reforms to analyze, so meaningful results cannot be expected due to the lack of data. It is certainly possible that in some regions, electoral engineering works indeed, but more elections need to take place before the effect will be statistically significant. The same could be true about the direction and the scale of the reforms; maybe small-, mid- or large-scale reforms increase partisan bias, but more cases are necessary to prove that statistically. Some of the pure descriptive statistics point to this direction as some indicate that electoral engineers do have a higher mean partisan bias than non-reformers do, but other descriptives indicate the opposite. The true effect might not be known for centuries, by which time a lot more elections will have taken place, but for now, I will have to settle for the data that already exists.

If we ignore the fact the population of elections is necessarily not as large as desired, there are two ways to interpret null results. The first possibility is that while no statistically significant effect is observed, the precision of this finding is very low, and the actual effect might be a strong positive or a strong negative. The other option is that the precision of the results is high, and there is no effect in the analyzed sample. I treated my results as if this last possibility was indeed what happened, i.e. my analysis uncovered highly precise results, and the null findings are reliable. There is a chance that the my findings are not precise and the effect of electoral engineering could be volatile in the dataset, and while there is no statistically significant effect, the actual effect could be anything ranging from a strong negative to a strong positive. This should be noted and could be further clarified through further research.

2.7.6 Potential directions for further research

There are many issues that could explain the results of the analysis, as discussed in this chapter. These could be cleared up with further research. More in-depth analysis of specific cases could shed some light on which of these reasons are the most prominent ones.

Investigating the reasons for the failure of partisan electoral engineering targeting one or more of the specific explanations described in this chapter could be fruitful both in enhancing the understanding of the effects of electoral reforms and in improving the design of research aimed at answering the same questions this dissertation asked. Any of the explanations offered in Sections 2.7.2 and 2.7.3 could be confirmed or rejected by case studies or in-depth comparative analysis on a smaller population of electoral reforms, and based on the results of those investigations, the limitations discussed in

Section 2.7.4 could be corrected in another large-n comparative study to further explore the partisan consequences of electoral reforms.

Selecting cases could be based on the results of this dissertation. Looking at the most and least successful reformers in terms of increasing partisan bias compared to the one experienced at the previous election could be a good starting point. The most effective examples of electoral engineering from my dataset are listed below:

- Liberia, 2011 (Unity Party)
- Colombia, 2002 (Colombian Conservative Party)
- Panama, 2014 (Democratic Change)
- Georgia, 2008 (United National Movement)
- Turkey, 1987 (Motherland Party)

The five least successful reformers are the ones on the following list:

- Turkey, 2002 (Democratic Left Party)
- Mauritius, 1982 (Labour Party)
- Madagascar, 2002 (Malagasy Revolutionary Party)
- Peru, 2006 (Possible Peru)
- France, 1993 (Socialist Party)

Taking a closer look at these examples could be a starting point to understanding what makes electoral engineering work and what makes it fail. These extreme cases could shed light on these, but another strategy would be to choose random cases with different backgrounds and conduct an analysis on those.

Up until now, case studies focused on influential cases, i.e. countries with highly politicized electoral reforms, such as New Zealand or Italy recently. Those reforms are generally large-scale, and thus smaller changes have largely been ignored, when the results in this dissertation suggest they deserve more attention. No matter what the strategy for choosing the cases, this dissertation could serve as a resource for the choice of cases instead of choosing to most publicly discussed reforms for further analysis.

Other than the case studies, another route would be to expand the research conducted in this dissertation. New cases and variables could be added to make the analysis more accurate. For example, public demand should be introduced as an important variable, and it could also be clarified which parties voted for the reform and which parties opposed it. Another addition could be to identify electoral changes that are so miniscule that they were not included in my dataset, such as the ones McElwain (2007) and Tan and Grofman (2016) discuss in their studies, e.g. campaign finance reforms. Furthermore, gerrymandering, which is not fully included in my dataset (if the number of districts do not change, there was no electoral reform identified), could be another area that should be analyzed from the perspective of power consolidation. Even though redistricting has been examined extensively by the literature, looking at it from the perspective of power consolidation could add a new wrinkle to both that vast strand of research and to the related field of engineering, as well as to the project of exploring the consolidation of power.

CONCLUSION

This dissertation focused on the topic of the consolidation of power, both in theory and in practice. Part 1 is mostly established a theoretical foundation of this concept, mostly connecting it to the democratization/democratic backsliding literature. What was lacking from that literature, and what I attempted to contribute to it, was strictly political perspective that put the power of individual actors at the center of the theory. Most of the literature was concerned with the evolution of democracies as a whole; how those systems are changing and what alters them. Individual actors are only of ancillary interest in that research; they are treated as either passive elements of a larger system or active players who shape the system; but are never the focus of attention. The same is true of contemporary power theory: the focus is mostly on the power of systems, not on the influence of individuals or groups.

I created the conceptualization of the consolidation of power to fill that gap. I defined it as a phenomenon where a political actor possessing power uses said power to preserve or increase its influence in the long run. After discussing the definition and similar concepts in the literature, a typology of the consolidation of power was created based on a parallel with the typology of democratic consolidation. Power is negatively consolidated if it is preserved in the long run, positive consolidation is the increase of power, while negative consolidation is about organizing or embedding power.

The definition and the typology can be used to describe and classify phenomena that are at the forefront of interest for political science and political journalism alike. Most observers of politics have been noticing a trend that the de-democratization literature attempted to capture from a systematic viewpoint, and this trend deserved a new perspective that focuses on individual power. Part 1 offered that, and that could advance the public and scientific discussion of this trend, and somewhat clarify the muddy picture we have of these phenomena.

I argues that all three types of power consolidation could exist within the setting of liberal democracy. While the consolidation of power can certainly lower the quality of democracy or could even be a way to upend the democratic system, just because an actor preserves, increases or stabilizes its power, democracy does not have to end. Moreover, in some cases, the consolidation os power is necessary to establish or improve democracy, making the relationship between democracy and power consolidation even more complex. These complexities need to be taken into account and not every attempt at power consolidation should be described as anti-democratic.

This is the main contribution of Part 1 in and of itself. However, my main objective was to create a framework suitable for empirical analysis because I felt the literature on democratic backsliding lacked standards that were testable on an individual actor-level. The power consolidation framework is thus not only a theoretical contribution, but is also the foundation of a larger empirical project.

The first step in this project is the second part of the dissertation. It defines electoral engineering, i.e. changing electoral rules for partisan gain as a tool of consolidation, and tests whether it is effective as such, i.e. whether reformers reap benefits from the new system they design. First, I argued for electoral engineering as a fitting tool to analyze from the consolidation perspective. It is an easily observable act, because it requires a change in formal institutions, and is therefore straightforward to detect whether it occurred or not. Partisan electoral reforms are also by definition aimed at the preservation or increase of the power of reformers, and electoral engineering is consequently a clear attempt at negative or positive consolidation. Furthermore, the success of partisan reforms can be measured in a relatively straightforward way, as they are only concerned with electoral results, which have accessible and quantifiable data.

Electoral reforms have a vast literature that I summarized in Part 2, and while there have been similar analyses conducted previously, none has this theoretical perspective, and more importantly, none of them used a comprehensive dataset that I did. My database had every democratic election result between 1974 and 2017, making a general assessment of electoral engineering possible. In the dataset, the seat share and vote share for each party was included as well as the characteristics of the electoral systems for each election. Electoral reforms were defined as a change of any electoral rule between two elections, and electoral reformers were identified as the governing parties at the time of the electoral reforms.

In order to answer whether reformers benefited from their own reforms, I turned to the widely accepted metric of the impact of electoral systems, partisan bias, i.e. seat share divided by vote share. Electoral systems benefit those who receive more seats for a given vote share, and this is probably the metric electoral engineers have in mind when designing reforms.

For the evaluation of the efficacy of electoral engineering, three hypotheses were posited. Firstly, that electoral reformers were looking to improve their partisan bias compared to the old system, and consequently should perform better in the new system compared to the old one. This hypothesis was rejected due to statistically insignificant results in every analysis that I conducted; there was no indication that electoral reformers improved their performance after the reform.

Secondly, I hypothesized that reformers were performing better compared to their non-reforming peers. If their partisan bias was higher than that of other parties, they created a system that benefits them, and were thus successful electoral engineers. This hypothesis was also largely refuted; reformers did not have a significant advantage compared to non-reformers. Nonetheless, neutral reformers, i.e. reformers who initiated changes that did not make the electoral system more majoritarian or more proportional were slightly successful compared to non-reformers. This might indicate that non-politicized, small reforms can be an effective way to consolidate power. Furthermore, proportional reformers, i.e. reformers that pushed the system in a proportional direction had a worse performance compared to non-reformer parties, suggesting that – just as the literature would suggest – proportional reforms are often introduced for non-partisan rather than partisan reasons.

Thirdly, I hypothesized that reformers intended to consolidate power negatively, and designed reforms to ensure they continue in government. That is measured via reelection rates; i.e. reformers should be reelected more often than non-reforming government parties. This hypothesis, just like the first two, was rejected as well; there is no significant difference between the reelection chances of reformers and non-reformers.

Additionally, I tested a related hypothesis, namely that proportional reforms are introduced by parties whose popularity is diminishing to mitigate electoral losses, while majoritarian reforms are introduced by parties with increasing popularity to maximize electoral gains. Another version of this hypothesis was that proportional reforms are introduced by parties with lower partisan bias, as they are inclined to 'level the playing field', since they were not real winners of the previous electoral system in the first place, so they design systems that reduce the differences of partisan bias between parties, which proportional systems tend to do. This hypothesis was also rejected, there was no significant difference observed with regards to the direction of reforms and the change in popularity among reformers, and their partisan bias also did not explain the direction of the reform they introduced.

All four hypotheses have been thus consistently rejected. This can be attributed to potential inadequacies in the research design, or to the failure of electoral engineering. The fact that electoral engineering is ineffective is an important conclusion and goes against the general assumption by both the public and the literature. Either electoral reforms are so complex that it is impossible to precisely design electoral systems that benefit reformers, or reformers have other motivations when introducing these reforms then maximizing electoral returns. Both of these refuted claims are widely accepted by political journalists and political scientists alike, and it is therefore surprising that either one or both of them appear to be incorrect based on my results.

Part 2 offers an interesting contribution to the literature of electoral reforms, both in terms of my dataset and my results, and just like the theoretical arguments made in Part 1, can be evaluated on its own. Nonetheless, in my opinion, the most crucial contribution of this dissertation lies in the combination of the two parts, bridging the gap between the theoretical and the empirical literature that exist completely separately, barely recognizing the existence of one another. The main objective of my project is to connect these related strands of political science in a single endeavor. That was the main aim of this dissertation and it a first step in that direction.

There are numerous implications of my findings for various groups. These findings, especially the empirical ones, could be interesting for politicians, who might conclude that electoral reform is such a complex process that electoral engineering is almost impossible to execute effectively. Therefore, if their goal was to consolidate power, they could be looking at other avenues. Alternatively, they could be designing less politicized reforms to avoid potential backlash at the ballot box, as those subtle reforms were the only ones that proved to be at least partly successful. Additionally, when electoral reforms are designed, partisan debates might not be as intense and adversarial as there is no effective way to create a system that undoubtedly favors one party over others. Reformers could thus be more focused on creating a fair system since engineering one to maximize partisan gain seems to be futile. This is obviously only a faint hope, but the findings would indicate that this behavior would be reasonable from politicians.

For observers of politics, such as journalists or analysts or just casual consumers of news, both parts offer some interesting takeaways. The theoretical framework established in Part 1 could provide context for political discussion and certain acts could be understood as a form of power consolidation. Even more importantly, the relationship between the power of the government and the quality of democracy could come closer to the forefront in these discussions, and more questions could be asked about them rather than turning to the general narrative of any act motivated by power is a detriment to democracies.

Part 2 could foster more questions by the public as well. Firstly, the effectiveness of electoral engineering has to be questioned and not accepted universally. Therefore, electoral changes should not be feared by those who worry about democracy and a fair playing field; even if they are motivated by the self-interest of politicians, they might not benefit reformers at the end of the day. Those who are worried about the entrenchment of politicians and their ever-increasing levels of power could look at other tools more closely as electoral engineering is simply not an overly effective way of consolidation.

Moreover, the general idea behind the project of the dissertation could spur new questions in public debates. Whenever politicians do something that is presumably motivated by power, which seems to be everything any politician ever does nowadays, the question ‘Does it actually increase the power of the actor?’ could be asked and investigated. This question is skipped so often, and every act is presumed to be perfectly designed when the contingencies of the world make everything in the future so uncertain that maximizing long-term power is a daunting task for any actor. Attention should not only be paid to the attempts at power consolidation, but also to the effectiveness of those attempts. This is the main idea behind my project that brought this dissertation about and would be a welcome addition to public discourse as well, especially in Hungary, where for the last twelve years, it has been assumed that the government cunningly calculates every possible scenario and perfectly designs each and every act to maximize power and does so successfully. This may even be true, but the demand to test these assumptions empirically would considerably improve the quality of public discussion in my opinion.

Last but certainly not least, this dissertation offers plenty of implications for political science that delineate a number of possible ways to continue this project. Part 1 offers a theoretical framework that could be used in the future by scholars who are interested in the dynamics of power relations of individual actors rather than the state of a political system or regime. It can be utilized for either theoretical or empirical investigations, or for research that connects the two.

While the theoretical framework can already be used, it is naturally incomplete in its current form. A lot of the elements can be elaborated further, including the details of the definition, the types and subtypes, the relationship between the various types, and the relationship between the consolidation of power and democracy. Case studies could be used to further illustrate the theory and for a deeper understanding of the general claims made in this dissertation that can have different manifestations in different environments. These case studies and their comparison could improve the theory considerably.

The efficacy of electoral engineering could also be investigated further. As suggested toward the end of Part 2, case studies could inform why my results were all null, what makes electoral engineering work or what makes it completely ineffective. The circumstances probably play an enormous role and exploring them would be crucial for further research. Alternatively, an expansion of the dataset to include the smallest reforms and who initiated or opposed reforms could be another route to take into this inquiry. That would lead to an even better general understanding of electoral engineering, while case studies could shed light on the circumstances that contributed to the success or failure in individual cases as well as inform future large-n research (e.g., which control variables to include or discard, etc.).

Other than continuing the research done in Part 1 and Part 2, another possible avenue to take is to continue the larger project, namely the empirical evaluation of the effectiveness of power consolidation. Other tools could be analyzed and compared to each other based on the blueprint offered in this dissertation on the example of electoral engineering. This was my main objective from the beginning, and this dissertation is the first step towards achieving it. The next steps include identifying more tools of power consolidation that are suitable for empirical analysis, then conducting the general analysis of them, and finally, in-depth exploration via case studies.

This is certainly a time-consuming and resource-demanding path, but one I intend to take and have already begun analysis for an additional consolidation tool (changing voting rules in the legislature). There are countless possible tools to analyze, and this project has consequently no clear finish line at this point. I believe it is worthwhile to continue on this route to improve our understanding of how power is consolidated. This is a central question of politics and political science, and even though there are several strands of literature examining it from very different angles, there is no inquiry that puts it at the focus. Filling this gap is a difficult, but rewarding task, and this dissertation is the first step towards that objective.

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APPENDIX

Notes on modifications made to the data of electoral results in the DPI and DES

1) If there were more than five years between two democratic elections, they were considered to be non-consecutive. The reason for this is that countries that regularly hold democratic elections generally do so every 2-5 years. If there was no election for such a long period, that could either mean that there was a non-democratic election between the two democratic ones, or that the ruling party held onto power by delaying the elections. Either way, in these cases, it is very likely that democracy was disrupted in the period between the elections, and therefore the former and the newer electoral results cannot be compared as if they were conducted under the same constitutional circumstances.

2) Independent groups and groups that had were not named in the DPI (party name indicated as 'not applicable') were not considered as parties.

3) Parties that changed their names between elections have been treated as the same party.

4) If a party split between elections, the party with the larger seat share after the split was considered to be the successor of the original party.

5) If two or more parties merged or contested an election together (using a joint list or not running candidates against each other in districts), they were considered to be a successor of the party with the largest seat share before the merger.

6) The following elections were discarded from the database, due to the fact after reviewing them, they either did not take place in that year, were not legislative elections or were clearly not democratic: Czech Republic 2012, Estonia 1997 and 2016, Germany 1974, Japan 2010, Netherlands 1991, New Zealand 1974, Paraguay 1978 and 1983, Philippines 1978, Poland 1989, Solomon Islands 1976, Suriname 1965 and 1976, Taiwan 1996 and 2005, Trinidad and Tobago 1974, Ukraine 1991, Uruguay 2015, Uzbekistan 2005, Vanuatu 1980. In addition, all of the elections in Turkish Cyprus (it is not a widely recognized country) and in Jordan (all members of parliament are independent) were discarded as well.

7) The following electoral results have either been amended or supplemented based on data from Nohlen's books, the Parline database or the websites of local election authorities: Albania 1992, 1996 and 2005; Australia 1974, 2010, 2013 and 2016; Argentina 1989, 1991, 1995, 2001 and 2003; Azerbaijan 1995 and 2010; Bangladesh 1979, 1986, 1991, 1996, 2001, 2008 and 2014; Barbados 2013; Belize 1981; Benin 2011; Brazil 1994 and 2014; Bulgaria 2014; Bosnia and Herzegovina 2000, 2010 and 2014; Burkina Faso 1978; Cambodia 1993; Cabo Verde 2011; Chile 1993; Comoros 1990, 1991, 1994, 2010, 2014; Costa Rica 1978, 1986, 1990 and 2006; Cote d'Ivoire 2011; Croatia 2000, 2015 and 2016; Czech Republic 2013; Dominican Republic 1978, 1982, 1986, 1990, 1994, 2006, 2010 and 2016; Ecuador 1979, 1984, 1986, 1988, 1990, 1992, 1994, 2002, 2009 and 2013; Egypt 1987; El Salvador 1988, 2000, 2012 and 2015; Fiji 1976, 1992, 1994 and 1999; Gambia 1992; Georgia 1992, 1995, 1999 and 2003; Ghana 1996, 2012 and 2016; Greece 1989 and 2012; Grenada 1999; Guatemala 1994, 1995 and 1999; Guyana 1992 and 1997; Haiti 2006; Honduras 1981, 1985, 2009 and 2013; Hungary 2010; India 1977, 1980, 1991, 1996, 1998 and 2009; Indonesia 2009; Iraq 2010 and 2014; Israel 2015; Italy 2008 and 2013; Jamaica 2011 and 2016; Japan 2009, 2012 and 2014; Latvia 2010; Lesotho 2012 and 2015; Liberia 2011; Lithuania 1992, 1996, 2008, 2012 and 2016; Macedonia 1994; Madagascar 1996 and 2013; Malawi 2004, 2009 and 2014; Malaysia 1978, 1982, 1986, 1990, 1995 and 1999; Maldives 2014; Mali 1992, 1997 and 2013; Mauritania 2013; Mauritius 1976, 1982, 1987, 1991, 1995, 2000, 2005, 2010 and 2014; Mexico 1982, 1985, 1988, 1991, 2000,

2006, 2009, 2012 and 2015; Mongolia 1990, 1992, 1996 and 2000; Morocco 1993, 2011 and 2016; Nepal 2008 and 2013; Niger 1993, 1995, 1999, 2004 and 2009; Nigeria 1979; Norway 2013; Papua New Guinea 1977, 1982 and 1987; Pakistan 1993, 1997, 2002, 2008 and 2013; Panama 2009 and 2014; Paraguay 2008 and 2013; Peru 1978; Philippines 2004, 2007, 2010, 2013 and 2016; Romania 1996, 2012 and 2016; Russia 1993; Samoa 1982, 1985, 1988, 1991, 1996, 2006, 2011 and 2016; Senegal 1988; Sierra Leone 2007 and 2012; Singapore 2011 and 2015; Solomon Islands 1980, 1984, 1989, 1993, 2006 and 2014; South Korea 2008, 2012 and 2016; Sri Lanka 1977 and 1994; St Lucia 2016, Suriname 1977, 1987, 1991, 1996 and 2015; Switzerland 2007 and 2015; Taiwan 1998, 2008, 2012, and 2016; Tanzania 1995; Thailand 1975, 1979, 1983, 1986, 1988, 1992, 1995, 1996, 2001, 2005, 2007 and 2011; Tunisia 2011; Ukraine 1994, 1998, 2002, 2006, 2012, 2014; Uruguay 1989, 2009, 2014 and 2016; United States 1974, 2006, 2008, 2010,, 2012, 2014 and 2016; Vanuatu 1991, 1995, 2004, 2008, 2012 and 2016; Venezuela 1978, 1983, 1993, 2005, 2010 and 2015; Yemen 1993 and 1997; Zambia 2006, 2011 and 2016; Zimbabwe 1990.

Tables for testing H1

Change in partisan bias for reformers	Mean	Median	Standard deviation	Im-proved	Worse-ned	Im-proved%
All	0.976	0.978	0.427	197	298	39.8%
Majoritarian	0.954	0.972	0.357	52	86	37.7%
Neutral	0.98	0.972	0.476	66	103	39.1%
Proportional	0.99	0.983	0.429	79	109	42.0%
Large-scale	0.948	0.923	0.386	19	35	35.2%
Mid-scale	1.01	0.975	0.431	31	49	38.8%
Small-scale	0.974	0.982	0.433	147	214	40.7%
Latin America	1.06	1	0.608	48	50	49.0%
Middle East and North Africa	0.912	0.965	0.411	12	22	35.3%
Oceania	0.914	0.922	0.394	2	6	25.0%
Eastern Europe and post-Soviet states	0.94	0.929	0.428	23	49	31.9%
The West	0.951	0.978	0.184	51	98	34.2%
Asia	0.991	0.986	0.446	42	47	47.2%
Sub-Saharan Africa	0.96	0.991	0.499	19	26	42.2%

Table 14: The change in partisan bias for reformers of different types and reformers in different regions

Change in partisan bias for main reformers	Mean	Median	Standard deviation	Im-proved	Wor-sened	Im-proved%
All	0.947	0.977	0.263	115	197	36.9%
Majoritarian	0.988	0.98	0.288	29	50	36.7%
Neutral	0.912	0.966	0.293	41	78	34.5%
Proportional	0.954	0.983	0.202	45	69	39.5%
Large-scale	0.979	0.911	0.396	10	23	30.3%
Mid-scale	1.015	0.992	0.296	18	25	41.9%
Small-scale	0.929	0.978	0.231	87	149	36.9%
Latin America	0.997	0.998	0.331	29	37	43.9%
Middle East and North Africa	0.955	0.967	0.349	6	12	33.3%
Oceania	0.843	0.925	0.268	1	4	20.0%
Eastern Europe and post-Soviet states	0.91	0.911	0.229	8	31	20.5%
The West	0.95	0.981	0.135	29	57	33.7%
Asia	0.939	0.977	0.216	26	34	43.3%
Sub-Saharan Africa	0.91	0.992	0.383	16	22	42.1%

Table 15: The change in partisan bias for leading reformers of different types and reformers in different regions

Comparison of change in partisan bias	N	Mean	Standard deviation	t	Degrees of freedom	p
All reformers	493	0.971	0.999	-4.771	1807.3	0.000
Other parties	2395	1.104	0.419			
Majoritarian reformers	137	0.941	0.321	-1.823	2886	0.068
Other parties	2751	1.089	0.947			
Neutral reformers	169	0.98	0.476	-1.475	2886	0.140
Other parties	2719	1.088	0.948			
Proportional reformers	187	0.987	0.429	-2.786	332.2	0.006
Other parties	2701	1.088	0.952			
Large-scale reformers	53	0.913	0.292	-1.336	2886	0.182
Other parties	2835	1.085	0.935			
Mid-scale reformers	80	1.009	0.938	-0.714	2886	0.475
Other parties	2808	1.083	0.431			
Small-scale reformers	360	0.972	0.433	-4.182	995.8	0.000
Other parties	2528	1.1	0.977			

Table 16: Comparing the means of change in partisan bias for reformers and all other parties

Region	Comparison of change in partisan bias	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Reformers	97	1.046	0.585	-0.88	593	0.379
	Other parties	498	1.101	0.564			
Middle East and North Africa	Reformers	34	0.912	0.411	-0.535	134	0.594
	Other parties	102	0.974	0.637			
Oceania	Reformers	8	0.914	0.394	-0.694	68	0.49
	Other parties	62	1.078	0.651			
Eastern Europe and post-Soviet states	Reformers	72	0.94	0.428	-0.981	408	0.327
	Other parties	338	1.011	0.579			
The West	Reformers	149	0.951	0.493	-5.106	561.2	0.000
	Other parties	945	1.063	0.184			
Asia	Reformers	88	0.986	0.445	-2.832	319.1	0.005
	Other parties	269	1.409	2.323			
Sub-Saharan Africa	Reformers	45	0.96	0.499	-0.824	224	0.411
	Other parties	181	1.133	1.387			

Table 17: Comparing the means of change in partisan bias for reformers and all other parties in different regions

Comparison of change in partisan bias (main reformers)	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All reformers	311	0.941	0.242	-6.699	1867.9	0.000
Other parties	2577	1.099	0.977			
Majoritarian reformers	78	0.964	0.203	-1.133	2886	0.257
Other parties	2810	1.085	0.940			
Neutral reformers	119	0.912	0.293	-2.038	2886	0.042
Other parties	2769	1.089	0.945			
Proportional reformers	114	0.954	0.202	-5.104	395.2	0.000
Other parties	2774	1.087	0.945			
Large-scale reformers	32	0.923	0.227	-0.973	2886	0.330
Other parties	2856	1.084	0.933			
Mid-scale reformers	43	1.016	0.296	-0.470	2886	0.638
Other parties	2845	1.083	0.934			
Small-scale reformers	236	0.929	0.231	-6.918	1263.1	0.000
Other parties	2652	1.095	0.965			

Table 18: Comparing the means of change in partisan bias for leading reformers and all other parties

Region	Comparison of change in partisan bias (main reformers)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Main reformers	65	0.969	0.244	-3.461	179.2	0.001
	Other parties	530	1.107	0.594			
Middle East and North Africa	Main reformers	18	0.955	0.349	-0.023	134	0.982
	Other parties	118	0.958	0.617			
Oceania	Main reformers	5	0.843	0.268	-0.8	68	0.427
	Other parties	65	1.077	0.644			
Eastern Europe and post-Soviet states	Main reformers	39	0.91	0.229	-1.047	408	0.296
	Other parties	371	1.008	0.579			
The West	Main reformers	86	0.95	0.135	-2.045	1092	0.041
	Other parties	1008	1.06	0.481			
Asia	Main reformers	60	0.939	0.216	-3.326	320.9	0.001
	Other parties	297	1.378	2.223			
Sub-Saharan Africa	Main reformers	38	0.91	0.383	-1.011	224	0.313
	Other parties	188	1.136	1.370			

Table 19: Comparing the means of change in partisan bias for leading reformers and all other parties in different regions

Comparison of change in partisan bias (government)	N	Mean	Standard deviation	t	Degrees of freedom	p
All reformers	493	0.971	0.419	0.995	984.3	0.320
Other parties	786	0.949	0.388			
Majoritarian reformers	137	0.941	0.321	-0.513	1277	0.608
Other parties	1142	0.959	0.409			
Neutral reformers	169	0.980	0.476	0.667	203.3	0.505
Other parties	1110	0.954	0.388			
Proportional reformers	187	0.987	0.429	1.101	1277	0.271
Other parties	1092	0.952	0.395			
Large-scale reformers	53	0.913	0.292	-0.823	1277	0.411
Other parties	1226	0.959	0.404			
Mid-scale reformers	80	1.009	0.431	1.185	1277	0.236
Other parties	1199	0.954	0.398			
Small-scale reformers	360	0.972	0.433	0.823	1277	0.411
Other parties	919	0.952	0.387			

Table 20: Comparing the means of change in partisan bias for reformers and all other government parties

Region	Comparison of change in partisan bias (government)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Reformers	97	1.05	0.585	2.401	253	0.017
	Other parties	158	0.917	0.263			
Middle East and North Africa	Reformers	34	0.912	0.411	-0.551	39.5	0.584
	Other parties	35	0.952	0.131			
Oceania	Reformers	8	0.914	0.394	0.315	33	0.754
	Other parties	27	0.867	0.371			
Eastern Europe and post-Soviet states	Reformers	72	0.940	0.428	-0.048	189	0.962
	Other parties	119	0.943	0.258			
The West	Reformers	149	0.951	0.184	-1.195	459	0.233
	Other parties	312	0.971	0.160			
Asia	Reformers	88	0.985	0.445	0.133	156	0.895
	Other parties	70	0.968	1.104			
Sub-Saharan Africa	Reformers	45	0.959	0.499	0.287	54.2	0.775
	Other parties	65	0.937	0.203			

Table 21: Comparing the means of change in partisan bias for reformers and all other government parties in different regions

Comparison of change in partisan bias (leaders)	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
Main reformers	311	0.941	0.242	0.719	796	0.473
Other parties	487	0.928	0.928			
Majoritarian main reformers	78	0.964	0.203	1.244	796	0.214
Other parties	720	0.930	0.236			
Neutral main reformers	119	0.912	0.293	-0.874	142.4	0.383
Other parties	679	0.937	0.221			
Proportional main reformers	114	0.954	0.202	1.029	796	0.304
Other parties	684	0.930	0.238			
Large-scale main reformers	32	0.923	0.227	-0.251	796	0.802
Other parties	766	0.934	0.234			
Mid-scale main reformers	43	1.016	0.296	2.396	796	0.017
Other parties	755	0.928	0.228			
Small-scale main reformers	236	0.929	0.231	-0.306	796	0.760
Other parties	562	0.935	0.234			

Table 22: Comparing the means of change in partisan bias for leading reformers and all other leading government parties

Region	Comparison of change in partisan bias (leaders)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Main reformers	65	0.969	0.244	1.689	189	0.093
	Other parties	126	0.904	0.259			
Middle East and North Africa	Main reformers	18	0.955	0.349	0.176	32	0.862
	Other parties	16	0.939	0.130			
Oceania	Main reformers	5	0.843	0.268	-0.326	24	0.747
	Other parties	21	0.896	0.339			
Eastern Europe and post-Soviet states	Main reformers	39	0.910	0.229	-0.500	91	0.619
	Other parties	54	0.936	0.256			
The West	Main reformers	86	0.950	0.135	-0.384	256	0.701
	Other parties	172	0.957	0.148			
Asia	Main reformers	60	0.939	0.216	1.569	106	0.120
	Other parties	48	0.857	0.326			
Sub-Saharan Africa	Main reformers	38	0.910	0.383	-0.784	48.8	0.437
	Other parties	50	0.962	0.175			

Table 23: Comparing the means of change in partisan bias for leading reformers and all other leading government parties in different regions

OLS models of reform and change in partisan bias	N	Constant (SE)	Reformer dummy (SE)	Previous bias (SE)	Vote share (SE)	Adjusted R squared
All reformers	1296	0.639*** (0.231)	0.230 (0.252)	0.186*** (0.037)	0.004*** (0.001)	0.254
Majoritarian reformers	1296	0.734*** (0.093)	0.399 (0.240)	0.180*** (0.037)	0.004*** (0.001)	0.257
Neutral reformers	1296	0.758*** (0.123)	0.042 (0.160)	0.182*** (0.037)	0.004*** (0.001)	0.253
Proportional reformers	1296	0.863*** (0.107)	-0.231 (0.164)	0.182*** (0.037)	0.004*** (0.001)	0.256
Large-scale reformers	1296	0.799*** (0.100)	-0.074 (0.174)	0.181*** (0.037)	0.004*** (0.001)	0.253
Mid-scale reformers	1296	0.725*** (0.165)	0.074 (0.184)	0.181*** (0.037)	0.004*** (0.001)	0.253
Small-scale reformers	1296	0.780*** (0.089)	-0.045 (0.153)	0.181*** (0.037)	0.004*** (0.001)	0.255

Table 24: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for partisan bias at the previous election, vote share and fixed effects (reformers of different types compared to all other government parties)

OLS models of reform and change in partisan bias	N	Constant (SE)	Reformer dummy (SE)	Previous bias (SE)	Vote share (SE)	Adjusted R squared
Latin America	258	0.939*** (0.139)	0.055 (0.173)	0.059 (0.077)	0.005** (0.002)	0.413
Middle East and North Africa	70	0.412* (0.184)	-1.068*** (0.174)	0.519* (0.160)	0.007* (0.003)	0.631
Oceania	38	0.468 (0.602)	0.037 (0.445)	0.047 (0.490)	0.020 (0.018)	-0.283
Eastern Europe and post-Soviet states	193	1.051*** (0.184)	-0.149 (0.169)	-0.145 (0.128)	0.008*** (0.002)	0.091
The West	461	0.406*** (0.097)	-0.035 (0.104)	0.618*** (0.066)	0.000 (0.001)	0.432
Asia	158	0.798*** (0.214)	0.312 (0.272)	0.098 (0.080)	0.006 (0.003)	-0.076
Sub-Saharan Africa	118	0.498* (0.225)	0.041 (0.216)	0.452* (0.173)	0.003 (0.002)	0.378

Table 25: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for partisan bias at the previous election, vote share and fixed effects (reformers compared to other government parties)

OLS models of reform and change in partisan bias (leaders)	**N**	**Constant (SE)**	**Reformer dummy (SE)**	**Previous bias (SE)**	**Vote share (SE)**	**Adjusted R squared**
Main reformers	811	0.738*** (0.050)	0.016 (0.017)	0.202*** (0.037)	0.003*** (0.001)	0.061
Majoritarian main reformers	811	0.733*** (0.051)	0.044 (0.029)	0.206*** (0.037)	0.003*** (0.001)	0.063
Neutral main reformers	811	0.744*** (0.050)	-0.007 (0.024)	0.202*** (0.038)	0.003*** (0.001)	0.060
Proportional main reformers	811	0.745*** (0.050)	-0.008 (0.024)	0.202*** (0.038)	0.003*** (0.001)	0.061
Large-scale main reformers	811	0.742*** (0.051)	0.014 (0.043)	0.203*** (0.038)	0.003*** (0.001)	0.060
Mid-scale main reformers	811	0.734*** (0.050)	0.066 (0.038)	0.206*** (0.037)	0.003*** (0.001)	0.064
Small-scale main reformers	811	0.743*** (0.050)	-0.001 (0.019)	0.203*** (0.038)	0.003*** (0.001)	0.060

Table 26: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for partisan bias at the previous election and vote share (leading reformers of different types compared to non-reformer leading government parties)

OLS models of reform and change in partisan bias (leaders)	**N**	**Constant (SE)**	**Reformer dummy (SE)**	**Previous bias (SE)**	**Vote share (SE)**	**Adjusted R squared**
Latin America	193	0.962*** (0.132)	0.068 (0.039)	0.014 (0.102)	0.002 (0.001)	0.006
Middle East and North Africa	35	0.756* (0.280)	-0.029 (0.098)	0.284 (0.208)	0.001 (0.002)	-0.028
Oceania	28	0.783* (0.337)	-0.120 (0.192)	0.253 (0.166)	0.003 (0.005)	-0.021
Eastern Europe and post-Soviet states	94	0.995*** (0.106)	-0.013 (0.042)	-0.012 (0.079)	0.004** (0.001)	0.051
The West	258	0.460*** (0.086)	0.001 (0.021)	0.447*** (0.072)	0.003** (0.001)	0.156
Asia	108	0.704*** (0.155)	0.066 (0.052)	0.115 (0.112)	0.006*** (0.001)	0.154
Sub-Saharan Africa	95	0.617* (0.248)	-0.074 (0.066)	0.288 (0.157)	0.003 (0.002)	0.034

Table 27: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for partisan bias at the previous election and vote share (leading reformers compared to non-reformer leading government parties)

DID model of change in partisan bias (government)	N (before)	N (after)	Diff. before reform (SE)	Diff. after reform (SE)	DID (SE)	R^2
All reformers	495	496	0.018 (0.064)	0.023 (0.064)	0.005 (0.025)	0.41
Other parties	796	796				
Majoritarian reformers	138	138	0.034 (0.097)	0.035 (0.097)	0.000 (0.040)	0.41
Other parties	1153	1154				
Neutral reformers	169	169	0.049 (0.091)	0.022 (0.091)	-0.027 (0.036)	0.41
Other parties	1122	1123				
Proportional reformers	188	189	-0.050 (0.105)	-0.016 (0.105)	0.034 (0.035)	0.41
Other parties	1103	1103				
Large-scale reformers	54	54	0.100 (0.225)	0.082 (0.225)	-0.019 (0.061)	0.41
Other parties	1237	1238				
Mid-scale reformers	80	81	-0.019 (0.134)	-0.002 (0.134)	0.017 (0.051)	0.41
Other parties	1211	1211				
Small-scale reformers	361	361	0.069 (0.069)	0.021 (0.069)	0.005 (0.027)	0.41
Other parties	930	931				

Table 28: Estimates of the difference-in-differences analysis of change in partisan bias among reforming and non-reforming government parties, controlling for vote share and fixed effects

Region	DID model of change in partisan bias (government)	N (before)	N (after)	Diff. before reform (SE)	Diff. after reform (SE)	DID (SE)	R^2
Latin America	Reformers	98	99	-0.015 (0.152)	0.084 (0.099)	0.099* (0.050)	0.30
	Other parties	159	159				
Middle East and North Africa	Reformers	35	35	0.088 (0.221)	0.045 (0.221)	-0.043 (0.084)	0.41
	Other parties	34	34				
Oceania	Reformers	8	8	0.351 (0.333)	0.239 (0.333)	-0.112 (0.370)	0.52
	Other parties	31	31				
Eastern Europe and post-Soviet states	Reformers	72	72	0.375 (0.246)	0.333 (0.246)	-0.042 (0.051)	0.41
	Other parties	120	120				
The West	Reformers	149	149	0.099 (0.112)	0.068 (0.112)	-0.031 (0.021)	0.46
	Other parties	313	313				
Asia	Reformers	73	73	0.653 (0.566)	0.741 (0.566)	0.089 (0.119)	0.32
	Other parties	89	89				
Sub-Saharan Africa	Reformers	65	65	-0.118 (0.222)	-0.155 (0.222)	-0.037 (0.077)	0.74
	Other parties	45	45				

Table 29: Estimates of the difference-in-differences analysis of partisan bias change among reforming and non-reforming government parties in different regions, controlling for vote share and fixed effects

DID model of change in partisan bias (leaders)	N (before)	N (after)	Diff. before reform (SE)	Diff. after reform (SE)	DID (SE)	R^2
Main reformers	312	316	0.057 (0.054)	0.072 (0.054)	0.014 (0.023)	0.58
Other parties	488	498				
Majoritarian reformers	79	79	-0.009 (0.084)	0.066 (0.084)	0.076* (0.037)	0.58
Other parties	721	735				
Neutral reformers	119	119	0.122 (0.083)	0.071 (0.083)	-0.051 (0.031)	0.58
Other parties	681	695				
Proportional reformers	114	118	0.018 (0.085)	0.044 (0.085)	0.026 (0.032)	0.58
Other parties	686	696				
Large-scale reformers	33	36	0.032 (0.164)	0.049 (0.164)	0.016 (0.056)	0.58
Other parties	767	778				
Mid-scale reformers	43	44	-0.060 (0.160)	0.022 (0.160)	0.081 (0.049)	0.58
Other parties	757	770				
Small-scale reformers	236	236	0.073 (0.057)	0.067 (0.067)	-0.007 (0.024)	0.58
Other parties	564	578				

Table 30: Estimates of the difference-in-differences analysis of partisan bias change among reforming and non-reforming leading government parties, controlling for vote share

Region	DID model of change in partisan bias (leaders)	N (before)	N (after)	Diff. before reform (SE)	Diff. after reform (SE)	DID (SE)	R^2
Latin America	Reformers	66	67	0.023 (0.133)	0.120 (0.133)	0.097 (0.049)	0.6
	Other parties	126	127				
Middle East and North Africa	Reformers	18	19	-1.213* (0.480)	-1.192* (0.480)	0.021 (0.116)	0.59
	Other parties	16	16				
Oceania	Reformers	5	5	0.111 (0.311)	-0.113 (0.308)	-0.224 (0.266)	0.62
	Other parties	21	23				
Eastern Europe and post-Soviet states	Reformers	39	39	0.349 (0.229)	0.296 (0.229)	-0.052 (0.074)	0.53
	Other parties	55	56				
The West	Reformers	86	86	0.708*** (0.193)	0.700*** (0.193)	-0.008 (0.023)	0.67
	Other parties	172	172				
Asia	Reformers	60	60	0.239 (0.266)	0.371 (0.266)	0.133* (0.067)	0.61
	Other parties	48	48				
Sub-Saharan Africa	Reformers	38	40	0.086 (0.220)	-0.002 (0.220)	-0.088 (0.078)	0.62
	Other parties	50	56				

Table 31: Estimates of the difference-in-differences analysis of partisan bias change among reforming and non-reforming leading government parties in different regions, controlling for vote share

Tables for testing H2

OLS models of reform and partisan bias	N	Constant (SE)	Reformer (SE)	Vote share (SE)	Adjusted R squared
All reformers	4224	0.637** (0.216)	0.022 (0.018)	0.008*** (0.000)	0.131
Majoritarian reformers	4224	0.633** (0.216)	0.031 (0.034)	0.008*** (0.000)	0.131
Neutral reformers	4224	0.639** (0.215)	0.089** (0.031)	0.008*** (0.000)	0.133
Proportional reformers	4224	0.628** (0.215)	-0.042 (0.028)	0.009*** (0.000)	0.131
Large-scale reformers	4224	0.632** (0.216)	0.004 (0.052)	0.008*** (0.000)	0.130
Mid-scale reformers	4224	0.634** (0.215)	0.081 (0.044)	0.008*** (0.000)	0.131
Small-scale reformers	4224	0.634** (0.216)	0.001** (0.021)	0.008*** (0.000)	0.130

Table 32: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for vote share and fixed effects (reformers of different types compared to all other parties)

OLS models of reform and partisan bias	N	Constant (SE)	Reformer (SE)	Vote share (SE)	Adjusted R squared
Latin America	848	0.939*** (0.106)	0.066 (0.040)	0.009*** (0.001)	0.080
Middle East and North Africa	229	0.986*** (0.122)	-0.168** (0.074)	0.007*** (0.002)	0.137
Oceania	123	0.998*** (0.180)	-0.035 (0.215)	0.008* (0.004)	-0.044
Eastern Europe and post-Soviet states	677	0.804*** (0.100)	0.025 (0.042)	0.010*** (0.001)	0.245
The West	1351	0.888*** (0.092)	0.030 (0.028)	0.006*** (0.001)	0.046
Asia	545	1.006*** (0.142)	0.073 (0.056)	0.009*** (0.001)	0.112
Sub-Saharan Africa	451	0.596* (0.243)	-0.063 (0.067)	0.009*** (0.001)	0.207

Table 33: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for vote share and fixed effects (all reformers compared to all other parties)

OLS models of reform and partisan bias	N	Constant (SE)	Reformer (SE)	Vote share (SE)	Adjusted R squared
Main reformers	4224	0.633** (0.216)	0.005 (0.021)	0.008*** (0.000)	0.130
Majoritarian main reformers	4224	0.635** (0.215)	0.055 (0.040)	0.008*** (0.000)	0.131
Neutral main reformers	4224	0.639** (0.215)	0.074* (0.034)	0.008*** (0.000)	0.132
Proportional main reformers	4224	0.622** (0.215)	-0.094** (0.033)	0.009*** (0.000)	0.132
Large-scale main reformers	4224	0.633** (0.216)	0.014 (0.060)	0.008*** (0.000)	0.130
Mid-scale main reformers	4224	0.634** (0.215)	0.069 (0.053)	0.008*** (0.000)	0.131
Small-scale main reformers	4224	0.630** (0.216)	-0.010 (0.024)	0.009*** (0.000)	0.130

Table 34: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for vote share (leading reformers of different types compared to all other parties)

OLS models of reform and partisan bias	N	Constant (SE)	Reformer (SE)	Vote share (SE)	Adjusted R squared
Latin America	848	0.960*** (0.106)	0.023 (0.045)	0.009*** (0.001)	0.076
Middle East and North Africa	229	0.989*** (0.124)	-0.087 (0.095)	0.007*** (0.002)	0.117
Oceania	123	0.961*** (0.117)	-0.015 (0.230)	0.008* (0.004)	-0.044
Eastern Europe and post-Soviet states	677	0.815*** (0.099)	0.005 (0.052)	0.010*** (0.001)	0.244
The West	1351	0.606*** (0.086)	-0.006 (0.034)	0.007*** (0.001)	0.045
Asia	545	1.007*** (0.142)	0.067 (0.063)	0.009*** (0.001)	0.111
Sub-Saharan Africa	451	0.598* (0.243)	-0.047 (0.070)	0.009*** (0.001)	0.206

Table 35: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for vote share and fixed effects (leading reformers compared to all other parties)

OLS models of reform and partisan bias (government)	**N**	**Constant (SE)**	**Reformer (SE)**	**Vote share (SE)**	**Adjusted R squared**
All reformers	1296	1.018*** (0.116)	-0.058 (0.141)	0.004*** (0.001)	0.222
Majoritarian reformers	1296	0.910*** (0.089)	0.423 (0.246)	0.004*** (0.001)	0.226
Neutral reformers	1296	0.952*** (0.120)	0.014 (0.165)	0.004*** (0.001)	0.221
Proportional reformers	1296	1.036*** (0.104)	-0.213 (0.169)	0.004*** (0.001)	0.223
Large-scale reformers	1296	0.987*** (0.095)	-0.117 (0.189)	0.004*** (0.001)	0.221
Mid-scale reformers	1296	0.870*** (0.167)	0.117 (0.189)	0.004*** (0.001)	0.221
Small-scale reformers	1296	0.960*** (0.084)	-0.011 (0.156)	0.004*** (0.001)	0.222

Table 36: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for vote share and fixed effects (reformers of different types compared to other government parties)

OLS models of reform and partisan bias (government)	**N**	**Constant (SE)**	**Reformer (SE)**	**Vote share (SE)**	**Adjusted R squared**
Latin America	258	1.047*** (0.118)	0.019 (0.153)	0.005** (0.002)	0.411
Middle East and North Africa	70	0.836*** (0.147)	-0.892*** (0.187)	0.011** (0.034)	0.522
Oceania	38	0.526 (0.243)	0.063 (0.694)	0.018 (0.015)	-0.086
Eastern Europe and post-Soviet states	193	0.884*** (0.143)	-0.013 (0.168)	0.007*** (0.002)	0.097
The West	461	1.082*** (0.083)	-0.089 (0.124)	0.001 (0.001)	0.185
Asia	158	0.907*** (0.196)	-0.267 (0.274)	0.006* (0.003)	-0.087
Sub-Saharan Africa	118	0.888*** (0.179)	0.085 (0.248)	0.003 (0.002)	0.214

Table 37: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for vote share and fixed effects (reformers compared to all non-reformer government parties)

OLS models of reform and partisan bias (leaders)	N	Constant (SE)	Reformer (SE)	Vote share (SE)	Adjusted R squared
Main reformers	811	0.981*** (0.022)	0.019 (0.018)	0.003*** (0.001)	0.030
Majoritarian main reformers	811	0.983*** (0.021)	0.036 (0.029)	0.003*** (0.001)	0.031
Neutral main reformers	811	0.986*** (0.021)	0.023 (0.024)	0.003*** (0.001)	0.030
Proportional main reformers	811	0.989*** (0.021)	-0.011 (0.024)	0.003*** (0.001)	0.029
Large-scale main reformers	811	0.986*** (0.021)	0.038 (0.042)	0.003*** (0.001)	0.030
Mid-scale main reformers	811	0.984*** (0.021)	0.056 (0.038)	0.003*** (0.001)	0.031
Small-scale main reformers	811	0.988*** (0.021)	0.001 (0.019)	0.003*** (0.001)	0.029

Table 38: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform, controlling for vote share (leading reformers of different types compared to non-reformer leading government parties)

OLS models of reform and partisan bias (leaders)	**N**	**Constant (SE)**	**Reformer (SE)**	**Vote share (SE)**	**Adjusted R squared**
Latin America	193	0.985*** (0.052)	0.065 (0.039)	0.001 (0.001)	0.009
Middle East and North Africa	35	1.108*** (0.094)	-0.043 (0.096)	0.001 (0.002)	-0.054
Oceania	28	1.181*** (0.143)	-0.058 (0.189)	0.001 (0.004)	-0.072
Eastern Europe and post-Soviet states	94	0.981*** (0.046)	-0.015 (0.042)	0.004** (0.001)	0.065
The West	258	0.947*** (0.037)	0.011 (0.023)	0.003** (0.001)	0.031
Asia	108	0.847*** (0.067)	0.059 (0.052)	0.006*** (0.001)	0.153
Sub-Saharan Africa	95	1.011*** (0.088)	-0.049 (0.067)	0.002 (0.002)	0.008

Table 39: OLS estimates of the model predicting partisan bias based on whether the party initiated a reform in different regions, controlling for vote share (leading reformers of compared to non-reformer leading government parties)

Comparison of partisan bias (government)	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All reformers	499	1.069	0.297	-0.533	1307	0.594
Other parties	810	1.08	0.356			
Majoritarian reformers	138	1.044	0.316	-1.174	1307	0.241
Other parties	1171	1.079	0.337			
Neutral reformers	169	1.089	0.34	0.535	218.959	0.593
Other parties	1140	1.074	0.334			
Proportional reformers	192	1.071	0.236	-0.297	353.655	0.766
Other parties	1117	1.077	0.349			
Large-scale reformers	57	1.079	0.325	0.084	1307	0.933
Other parties	1252	1.076	0.335			
Mid-scale reformers	81	1.076	0.298	0.002	1307	0.998
Other parties	1228	1.076	0.337			
Small-scale reformers	361	1.066	0.292	-0.619	1307	0.536
Other parties	948	1.079	0.35			

Table 40: Comparing the mean partisan bias of reformers of different types to all other government parties

Region	Comparison of partisan bias (government)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Reformers	98	1.092	0.281	0.903	0.258	0.367
	Other parties	159	1.058	0.3			
Middle East and North Africa	Reformers	35	0.983	0.422	-1.018	41.174	0.315
	Other parties	35	1.06	0.138			
Oceania	Reformers	8	1.084	0.214	-1.021	40	0.314
	Other parties	34	1.39	0.835			
Eastern Europe and post-Soviet states	Reformers	72	1.051	0.308	-0.525	192	0.6
	Other parties	122	1.072	0.244			
The West	Reformers	149	1.056	0.19	0.299	460	0.765
	Other parties	313	1.05	0.183			
Asia	Reformers	89	1.133	0.324	-0.162	96.818	0.872
	Other parties	73	1.148	0.703			
Sub-Saharan Africa	Reformers	47	1.035	0.407	-0.41	117	0.682
	Other parties	72	1.062	0.321			

Table 41: Comparing the mean partisan bias of reformers to all other government parties in different regions

Comparison of partisan bias (leaders)	N	Mean	Standard deviation	t	Degrees of freedom	p
Main reformers	316	1.104	0.26	1.231	812	0.22
Other parties	498	1.081	0.255			
Majoritarian main reformers	79	1.124	0.263	1.261	812	0.21
Other parties	735	1.086	0.256			
Neutral main reformers	119	1.115	0.323	0.965	141.991	0.34
Other parties	695	1.085	0.244			
Proportional main reformers	118	1.078	0.173	-0.722	226.018	0.47
Other parties	696	1.092	0.269			
Large-scale main reformers	36	1.145	0.289	1.316	812	0.19
Other parties	778	1.087	0.256			
Mid-scale main reformers	44	1.131	0.241	1.086	812	0.28
Other parties	770	1.087	0.258			
Small-scale main reformers	236	1.092	0.259	0.185	812	0.85
Other parties	578	1.089	0.257			

Table 42: Comparing the mean partisan bias of leading reformers of different types to all other leading government parties

Region	Comparison of partisan bias (leaders)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Main reformers	67	1.118	0.26	1.939	192	0.054
	Other parties	127	1.04	0.27			
Middle East and North Africa	Main reformers	19	1.086	0.356	-0.42	33	0.676
	Other parties	16	1.126	0.137			
Oceania	Main reformers	5	1.166	0.044	-0.61	24.054	0.547
	Other parties	23	1.22	0.408			
Eastern Europe and post-Soviet states	Main reformers	39	1.087	0.191	-0.48	93	0.636
	Other parties	56	1.11	0.256			
The West	Main reformers	86	1.071	0.181	0.667	0.256	0.505
	Other parties	172	1.056	0.17			
Asia	Main reformers	60	1.163	0.252	1.269	86.038	0.208
	Other parties	48	1.09	0.331			
Sub-Saharan Africa	Main reformers	40	1.077	0.401	-0.9	65.892	0.371
	Other parties	56	1.144	0.284			

Table 43: Comparing the mean partisan bias of leading reformers of different types to all other leading government parties in different regions

Tables for testing H3

Logit models of reform and reelection	N	Constant (SE)	Reformer (SE)	Reformer odds ratio
All reformers	1356	0.518*** (0.071)	-0.057 (0.115)	0.944
Majoritarian reformers	1356	0.504*** (0.059)	-0.068 (0.183)	0.943
Neutral reformers	1356	0.500*** (0.060)	-0.025 (0.165)	0.976
Proportional reformers	1356	0.502*** (0.060)	-0.035 (0.160)	0.966
Large-scale reformers	1356	0.529*** (0.057)	-0.698 (0.268)	0.497
Mid-scale reformers	1356	0.483*** (0.058)	0.228 (0.238)	1.256
Small-scale reformers	1356	0.491*** (0.066)	0.019 (0.126)	1.020

Table 44: Binary logistic estimates of reelection rates based on whether the party initiated a reform (reformers of different types compared to other government parties)

Logit models of reform and reelection	**N**	**Constant (SE)**	**Reformer (SE)**	**Reformer odds ratio**
Latin America	268	0.470** (0.158)	-0.081 (0.259)	0.922
Middle East and North Africa	48	0.661* (0.308)	-0.325 (0.457)	0.723
Oceania	50	0.318 (0.329)	0.018 (0.671)	1.018
Eastern Europe and post-Soviet states	194	0.164 (0.182)	-0.558 (0.301)	0.572
The West	462	0.613*** (0.118)	0.101 (0.211)	1.106
Asia	162	0.360 (0.238)	0.266 (0.326)	1.305
Sub-Saharan Africa	137	1.003*** (0.249)	-0.022 (0.392)	0.978

Table 45: Binary logistic estimates of reelection rates based on whether the party initiated a reform in different regions (reformers compared to other government parties)

Logit models of reform and reelection (leaders)	**N**	**Constant (SE)**	**Reformer (SE)**	**Reformer odds ratio**
Main reformers	848	0.589*** (0.091)	-0.005 (0.147)	0.995
Majoritarian main reformers	848	0.601*** (0.075)	-0.143 (0.242)	0.867
Neutral main reformers	848	0.581*** (0.078)	0.042 (0.202)	1.043
Proportional main reformers	848	0.580*** (0.077)	0.050 (0.208)	1.051
Large-scale main reformers	848	0.607*** (0.073)	-0.445 (0.338)	0.641
Mid-scale main reformers	848	0.574*** (0.074)	0.253 (0.329)	1.288
Small-scale main reformers	848	0.579*** (0.085)	0.028 (0.159)	1.029

Table 46: Binary logistic estimates of reelection rates based on whether the party initiated a reform (leading reformers of different types compared to other leading government parties)

Logit models of reform and reelection (leaders)	**N**	**Constant (SE)**	**Reformer (SE)**	**Reformer odds ratio**
Latin America	201	0.331 (0.175)	-0.182 (0.301)	0.834
Middle East and North Africa	83	1.009* (0.436)	-0.251 (0.655)	0.778
Oceania	31	0.336 (0.414)	-0.049 (0.869)	0.952
Eastern Europe and post-Soviet states	95	0.288 (0.270)	-0.339 (0.419)	0.713
The West	258	0.624*** (0.160)	0.212 (0.284)	1.236
Asia	108	0.511 (0.298)	0.258 (0.407)	1.295
Sub-Saharan Africa	107	-0.299*** (0.473)	1.427 (0.321)	0.742

Table 47: Binary logistic estimates of reelection rates based on whether the party initiated a reform in different regions (leading reformers compared to other leading government parties)

Comparison of reelection rate	N	Re-elected	Not re-elected	Re-election%	φ	p
All reformers	512	314	198	61.3%	-0.013	0.659
Other parties	844	529	315	62.7%		
Majoritarian reformers	140	85	55	60.7%	-0.01	0.708
Other parties	1216	758	458	62.3%		
Neutral reformers	180	111	69	61.7%	-0.004	0.882
Other parties	1176	732	444	62.2%		
Proportional reformers	192	118	74	61.5%	-0.006	0.827
Other parties	1164	725	439	62.3%		
Large-scale reformers	59	27	32	45.8%	-0.072	0.008
Other parties	1297	816	481	62.9%		
Mid-scale reformers	85	57	28	67.1%	0.026	0.337
Other parties	1271	786	485	61.8%		
Small-scale reformers	368	230	138	62.5%	0.004	0.878
Other parties	988	613	375	62.0%		

Table 48: Comparing the mean reelection rates of reformers of different types to all other government parties

Region	Comparison of reelection rate	N	Re-elected	Not re-elected	Re-election%	φ	p
Latin America	Reformers	99	59	40	59.6%	-0.019	0.753
	Other parties	169	104	65	61.5%		
Middle East and North Africa	Reformers	36	21	15	58.3%	-0.078	0.477
	Other parties	47	31	16	66.0%		
Oceania	Reformers	12	7	5	58.3%	0.004	0.979
	Other parties	38	22	16	57.9%		
Eastern Europe and post-Soviet states	Reformers	72	29	43	40.3%	-0.134	0.063
	Other parties	122	66	56	54.1%		
The West	Reformers	149	100	49	67.1%	0.022	0.633
	Other parties	313	203	110	64.9%		
Asia	Reformers	89	58	31	65.2%	0.064	0.413
	Other parties	73	43	30	58.9%		
Sub-Saharan Africa	Reformers	55	40	15	72.7%	-0.005	0.954
	Other parties	82	60	22	73.2%		

Table 49: Comparing the mean reelection rates of reformers to all other government parties in different regions

Comparison of reelection rate (leaders)	N	Re-elected	Not re-elected	Reelection %	φ	p
Main reformers	324	187	137	57.7%	0.003	0.938
Other parties	524	301	223	57.4%		
Majoritarian main reformers	80	45	35	56.3%	-0.008	0.805
Other parties	768	443	325	57.7%		
Neutral main reformers	126	75	51	59.5%	0.017	0.627
Other parties	722	413	309	57.2%		
Proportional main reformers	118	67	51	56.8%	-0.006	0.856
Other parties	730	421	309	57.7%		
Large-scale main reformers	37	15	22	40.5%	-0.073	0.032
Other parties	811	473	338	58.3%		
Mid-scale main reformers	46	30	16	65.2%	0.037	0.279
Other parties	802	458	344	57.1%		
Small-scale main reformers	241	142	99	58.9%	0.018	0.61
Other parties	607	346	261	57.0%		

Table 50: Comparing the mean reelection rates of leading reformers of different types to all other leading government parties

Region	Comparison of reelection rate (leaders)	N	Re-elected	Not re-elected	Re-election%	φ	p
Latin America	Main reformers	67	33	34	49.3%	-0.049	0.40
	Other parties	134	73	61	54.5%		
Middle East and North Africa	Main reformers	20	11	9	55.0%	-0.169	0.24
	Other parties	28	20	8	71.4%		
Oceania	Main reformers	7	4	3	57.1%	0.06	0.74
	Other parties	24	12	12	50.0%		
Eastern Europe and post-Soviet states	Main reformers	39	17	22	43.6%	-0.01	0.92
	Other parties	56	25	31	44.6%		
The West	Main reformers	86	53	33	61.6%	0.039	0.53
	Other parties	172	99	73	57.6%		
Asia	Main reformers	60	36	24	60.0%	0.038	0.69
	Other parties	48	27	21	56.3%		
Sub-Saharan Africa	Main reformers	45	33	12	73.3%	0.008	0.93
	Other parties	62	45	17	72.6%		

Table 51: The reelection rates of leading reformer parties compared to all other leading government parties in different regions

Tables for testing H4a and H4b

Logit models of previous partisan bias and the direction of a reform (leaders)	N	Constant (SE)	Previous partisan bias (SE)	Previous partisan bias odds ratio
All reformers	198	0.823 (0.861)	-0.405 (0.732)	0.667
Reformers in Latin America	52	0.713 (1.818)	-0.242 (1.583)	0.785
Reformers in the Middle East and North Africa	16	-0.347 (3.548)	0.187 (3.080)	1.206
Reformers in Oceania	0	-	-	-
Reformers in Eastern Europe and post-Soviet states	33	-0.179 (1.616)	0.494 (1.282)	1.638
Reformers in The West	50	0.421 (3.676)	-0.089 (3.347)	0.915
Reformers in Asia	29	4.268 (3.216)	-3.177 (2.670)	0.042
Reformers in Sub-Saharan Africa	18	3.186 (2.768)	-2.357 (2.191)	0.095
Large-scale reformers	34	-1.114 (1.695)	1.310 (1.385)	3.707
Mid-scale reformers	28	1.179 (2.685)	-0.774 (2.303)	0.461
Small-scale reformers	132	2.656 (1.385)	-2.033 (1.205)	0.131

Table 52: Binary logistic estimates of previous partisan bias predicting the direction of an electoral reform in different regions (population of all leading majoritarian and proportional reformers)

Logit models of change in vote share and the direction of a reform (leaders)	**N**	**Constant (SE)**	**Vote share change (SE)**	**Vote share change odds ratio**
All reformers	198	0.346 (0.385)	0.023 (0.398)	1.024
Reformers in Latin America	52	-0.386 (0.775)	1.026 (0.911)	2.790
Reformers in the Middle East and North Africa	16	-7.238 (4.451)	8.356 (4.923)	4254.02
Reformers in Oceania	0	-	-	-
Reformers in Eastern Europe and post-Soviet states	33	0.429 (0.900)	0.002 (0.906)	1.002
Reformers in The West	50	1.090 (1.551)	-0.848 (1.679)	0.428
Reformers in Asia	29	1.600 (0.986)	-1.058 (0.884)	0.347
Reformers in Sub-Saharan Africa	18	1.288 (1.792)	-0.972 (1.863)	0.378
Large-scale reformers	34	-1.072 (1.184)	2.063 (1.478)	7.873
Mid-scale reformers	28	1.274 (1.237)	-1.048 (1.253)	0.351
Small-scale reformers	132	0.386 (0.450)	-0.055 (0.458)	0.947

Table 53: Binary logistic estimates of change in vote share predicting the direction of an electoral reform in different regions (population of all leading majoritarian and proportional reformers)

Comparison of previous partisan bias based on the direction of the reform (leaders)	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All proportional reformers	114	1.151	0.202	-0.553	192	0.581
All majoritarian reformers	80	1.167	0.195			
Large-scale proportional reformers	21	1.277	0.351	0.964	32	0.342
Large-scale majoritarian reformers	13	1.169	0.251			
Mid-scale proportional reformers	16	1.142	0.170	-0.325	26	0.748
Mid-scale majoritarian reformers	12	1.163	0.174			
Small-scale proportional reformers	77	1.119	0.130	-1.763	130	0.080
Small-scale majoritarian reformers	55	1.168	0.188			

Table 54: Comparing the mean partisan bias of leading proportional reformer parties at the elections preceding the reform to leading majoritarian reformers

Region	Comparison of previous partisan bias based on the direction of the reform (leaders)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Proportional	31	1.130	0.133	-0.15	49	0.88
	Majoritarian	20	1.138	0.242			
Middle East and North Africa	Proportional	7	1.142	0.259	0.05	6.5	0.96
	Majoritarian	8	1.137	0.054			
Oceania	Proportional	0	-	-	-	-	-
	Majoritarian	0	-	-			
Eastern Europe and post-Soviet states	Proportional	20	1.257	0.352	0.38	31	0.73
	Majoritarian	13	1.216	0.202			
The West	Proportional	29	1.095	0.099	-0.03	48	0.98
	Majoritarian	21	1.095	0.068			
Asia	Proportional	18	1.153	0.135	-1.21	27	0.24
	Majoritarian	11	1.225	0.184			
Sub-Saharan Africa	Proportional	9	1.180	0.220	-1.10	14	0.29
	Majoritarian	6	1.324	0.307			

Table 55: Comparing the means of the partisan bias of leading proportional reformers at the elections preceding the reform to leading majoritarian reformers in different regions

Logit models of change in vote share and the direction of a reform (leaders)	**N**	**Mean**	**Standard deviation**	**t**	**Degrees of freedom**	**p**
All proportional reformers	114	0.895	0.277	0.054	114.7	0.957
All majoritarian reformers	80	0.892	0.474			
Large-scale proportional reformers	21	0.886	0.363	1.469	31	0.152
Large-scale majoritarian reformers	13	0.715	0.228			
Mid-scale proportional reformers	16	0.892	0.283	-0.853	26	0.401
Mid-scale majoritarian reformers	12	1.003	0.404			
Small-scale proportional reformers	77	0.899	0.252	-0.106	72.1	0.916
Small-scale majoritarian reformers	55	0.907	0.521			

Table 56: Comparing the change in vote share of leading proportional reformer parties to leading majoritarian reformers

Region	Logit models of change in vote share and the direction of a reform (leaders)	N	Mean	Standard deviation	t	Degrees of freedom	p
Latin America	Proportional	31	0.862	0.262	1.14	49	0.26
	Majoritarian	20	0.748	0.460			
Middle East and North Africa	Proportional	7	0.953	0.121	2.53	9.1	0.03
	Majoritarian	8	0.639	0.327			
Oceania	Proportional	0	-	-	-	-	-
	Majoritarian	0	-	-			
Eastern Europe and post-Soviet states	Proportional	20	0.913	0.389	0.00	31	1.00
	Majoritarian	13	0.912	0.431			
The West	Proportional	29	0.892	0.205	-0.50	48	0.62
	Majoritarian	21	0.917	0.134			
Asia	Proportional	18	0.930	0.311	-1.39	27	0.18
	Majoritarian	11	1.237	0.858			
Sub-Saharan Africa	Proportional	9	0.867	0.300	-0.49	13	0.63
	Majoritarian	6	0.949	0.333			

Table 57: Comparing the change in vote share of leading proportional reformer parties to leading majoritarian reformers in different regions