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DISSERTATION SUMMARY

Gergely Rajnai

The Consolidation of Power through Electoral Engineering

Ph.D. dissertation

Supervisor:

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Professor

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Department of Political Science

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Table of Contents

1. Justification of topic selection

The topic of this dissertation has recently received significant attention from political journalists and social scientists alike. The news often cover 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 efficacy of these steps was seldom questioned in the public discourse, 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 the entire political system. Concepts such as state capture (Hellman, 1996), abusive constitutionalism (Landau, 2013), electoral authoritarianism (Schedler, 2006), competitive authoritarianism (Levitsky-Way, 2010), democratic deconsolidation (Foa and Mounk, 2017) and democratic backsliding (Bermeo, 2016) all describe how the system is affected by the acts of power-seeking politicians, and neither puts an emphasis on how individual power relations are affected by this phenomenon.

Others have described the consolidation of power as phenomenon that is very distinct to my definition and focus on the power of institutions rather than individual actors. These include Turner (2004), Mann (1984) and Tilly (1975). Furthermore, other concepts have been developed that are close to grasping the phenomenon that I am interested in, but fall short of fully describing it, such as the investment of power (Korpi, 1985). Consequently, there is 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

lack of conceptualization in political science when it comes to power grabs from the perspective of individual actors.

Part 1 is a theoretical argumentation that intends to remedy this by creating the framework that I will use for my research not just in this dissertation, but also in the future. It defines, describes and classifies 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.

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. 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.

Part 2 is useful in determining these factors and the general effectiveness of one particular tool, offering a blueprint for the evaluation of other tools in the future. Electoral engineering is suitable to be the subject of this blueprint, because it meets the criteria of power consolidation (as laid out in Part 1), and is an easily observable phenomenon as it is an institutional change, making its empirical analysis relatively straightforward.

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 in those systems affect the outcomes of elections. The political gains and losses changes in the electoral system could result in has thus often been a point of political debate. This is especially the case in countries where large-scale electoral reform has taken place, such as the one in Italy in 2017. 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.

The rational-choice institutionalist school of analyzing electoral systems has ascertained how the various electoral systems should affect results in general (Shugart and Taagepera, 2018; Colomer, 2018) and also in specific cases (e.g. 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. The main assumption derived from the literature is that actors initiating electoral reform will create a system that will be beneficial to them in the future. 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".

This theoretical assumption has been tested empirically by numerous papers. Some have used case studies (e.g. Brady and Mo, 1992; McElwain, 2007; Pilet, 2008; Tan and Grofman, 2016; Walter and Emmenegger, 2019). Others conducted cross-national data analysis (e.g. Rokkan, 1970; Boix, 1999; Blais et al, 2005; Andrews and Jackman, 2005; Calvo, 2009; Riera, 2013; Bol, 2016). However, even the cross-national surveys are limited in scope. They all focus on one region (in almost all cases, Western Europe), on one time period (often, the interwar period, when the earliest proportional systems were introduced), and only on reforms of the largest scale, i.e. reforms that changed the electoral family (majoritarian, mixed or proportional) of the system.

Consequently, none are suitable for a general investigation of the efficacy of electoral engineering. Part 2 intends to fill this gap by testing the assumption that electoral reformers design systems that benefit them. Thus, the objective is to create a dataset that covers as many cases as possible, that are as diverse as possible with regards to the scale of the reform, the region or the time period it took place, then test the efficacy of electoral engineering on that dataset. It could prove if partisan electoral reforms are an effective way of consolidating power or not. This is the first step towards the general evaluation of the consolidation of power in practice.

2. Hypotheses, data and methods

2.1 The methods of Part 1

The theoretical argumentation in Part 1 mainly builds on the literature of democratic consolidation, most importantly on the typology created by Schedler (1998) and Merkel (2004). The concept of power is developed in the pluralist tradition, originally established by Robert Dahl (1956; 1961). In his understanding, power is a purely political concept that can be described analyzing formal procedures This is in contrast with broader definitions of power that encompass phenomena outside of the formal political arena. Power is defined as the ability to influence policy-making in case of disagreement or clear, open, verifiable conflict based on revealed preferences (Dahl, 1961: 330). This is the concept I use when defining and categorizing the consolidation of power.

2.2 Hypotheses of Part 2

In Part 2, I operationalized the impact of electoral systems on the electoral success of parties by looking at the partisan bias of parties. This simple metric 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 that investigate electoral engineering (e.g. Calvo [2009]), and these studies posit that parties are looking to maximize b in this equation.

I had four hypotheses to test the assumption described in the previous section. Firstly, I compared observed results 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.

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.

Secondly, I posited that electoral engineers also aim to design a system that benefits them more than it does other parties in general. Thus, their standard is not their previously attained bias, but simply a higher bias than that of their rivals.

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.

Thirdly, I posited that electoral engineers only aim at power preservation. Consequently, all they care about is reelection. 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.

In addition to finding an answer to my research question through the testing of the three hypotheses, I also attempted 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 a relatively low partisan bias at preceding elections will favor proportional electoral reforms, while those with a larger bias will introduce 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).

The reason for this is that parties with a low bias are generally more uncertain whether they will be winners again, and consequently adopt systems that mitigate the risk of losing in a more majoritarian system by introducing proportional reforms. Conversely, parties with a relatively high partisan bias can be confident that they will secure reelection and will aim to maximize their electoral payoffs by creating more majoritarian systems that reward the winners.

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 tested this hypothesis in an alternative way. If the direction of the reform is mostly based on whether the reformers feel confident in winning again, then it is logical to assume that they are looking closely at polling results and deduce whether their popularity is rising or falling. If they are becoming more popular, they can be certain in victory and will strive to introduce a system that will increase the differences between the winners and losers. If they are losing popularity, then they will be inclined to mitigate their upcoming losses by introducing proportional reforms.

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.

2.3 Data used in Part 2

To test these hypotheses, I created a novel dataset by fusing the Democratic Electoral Systems around the World database (DES)[3] and the Database of Political Institutions (DPI)[4], and modifying the resulting database where it had missing or incorrect information. The DES had detailed data on electoral institutions, allowing me to identify electoral reforms: if the institutions between two elections changed in any way, an electoral reform took place. The DPI has data on electoral results on the party level, including seat shares and vote shares. The DPI thus allows me to compute partisan bias for each party at each election that it covers.

The resulting dataset covers every democratic election in the world between 1974 and 2017. 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

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

[4] 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

were 512 reformer parties out of 1,356 parties that were a member of government prior to an election. This is the most comprehensive dataset used for the analysis of electoral engineering.

Electoral reformers were identified as the parties in government when the reform was introduced. This was based on the widely accepted assumption that no electoral reform has been passed without approval of the government, and government parties always substantially shape the features of the new system (Renwick, 2010: 16). Other studies have also used the same logic in their empirical analysis of electoral engineering (e.g. Andrews and Jackman, 2005; Calvo, 2009). I ran all the models with two different assumptions: one posited that all cabinet members are electoral engineers and should benefit from the reform, while the other assumed that only the largest party in power should benefit and junior cabinet members may be 'left behind', i.e. the senior cabinet members design the system so not only the opposition, but also their own coalition partners are harmed by the changes.

2.4 Methods used in Part 2

In order to test H1, i.e. whether reformers have a better partisan bias after the reform than they did beforehand, I first conducted several straightforward analyses of descriptive statistics. 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 assessed H1 comparatively as well, that is, I compare the change in the partisan bias of reformers to non-reformers. This was done in several ways. First, I ran OLS regressions, where the dependent variable is the partisan bias of the party after the reform, the main 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.

Another method of testing the change in the partisan bias of reformers was 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 was attributed to the 'treatment effect'. For more details on this method, see Wing et al. (2018).

In my case, the dependent variable was partisan bias, the 'treatment' is electoral reform, the 'treated' group was electoral reformers, and the 'untreated group' was 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 was considered a case in the 'untreated after' group, while their partisan bias at the preceding election was 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 was considered the treatment effect, and based on H1, we can expect that the 'treated' group (reformers) has a larger increase or a smaller decrease in their partisan bias as a result of the reform.

Furthermore, I also ran independent unpaired t-tests, 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.

H2, i.e. whether electoral reformers have a higher partisan bias than non-reformers do, was examined by way of OLS regressions. The dependent variable was the partisan bias of parties, the independent variable was 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 were also 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, 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 was used among parties that were members of the government when the election took place. The dependent variable was whether the party in question was reelected or not, and the independent variable was whether the party was a

reformer or not. The results show if reformers are reelected at a significantly higher rate than non-reformers are. This model also allows for the inclusion of control variables. Additionally, I also reported 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 was also tested via logistic regression on the population of proportional and majoritarian reformers. The dependent variable was whether the party in question introduced a proportional reform or not. This has the downside of not having a continuous variable as the dependent one in the model, 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 was 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.

2.5 Control variables used in Part 2

In the case of OLS regressions and DID analyses, I had to control for fixed effects to mitigate the issues caused by the fact that results from the same election are not independent from each other. For example, if one party receives 40% of the vote, then the rest of the parties only have 60% to compete for. In my case, fixed effects were introduced for each election, i.e. in each model each separate election was used as a dummy variable, so that for parties in the same election the same variable had a value of 1, and for all the other parties that value was 0.

Furthermore, since larger parties tend to have a larger partisan bias (Cox, 1997), I also controlled for the size of the party in the models that had partisan bias as the dependent variable (H1, H2). I also conducted 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. Additionally, I tested the different effects of electoral reforms based on the direction of the reform (whether it made the system more majoritarian, more proportional or if it was neutral from that perspective). Finally, to control for potential cultural effects, I ran all the tests in different regions as well. There were seven regions distinguished in the dataset, these

were: Latin America, Middle East and North Africa, Oceania, Eastern Europe and post-Soviet states, the West, Asia and Sub-Saharan Africa.

3. Main findings

3.1 The theory of the consolidation of power

The consolidation of power is defined as an action by an actor (individual or group) that meets four criteria:

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. 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. That is due to the fact that short-term increases in power often affect long-term power negatively, meaning that short-term power increase can be counterproductive to power 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.

Based on the parallel with the classification of democratic consolidation, there are three main types of power consolidation. 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. This could be done within the confines of democracy through means such as good governance, responsiveness to the demands of voters,

effective campaigning, etc. This subtype of negative consolidation is the *competence-based preservation of power*. On the other hand, if power is negatively consolidated as a result of non-democratic acts, such as legally excluding rivals, using violence against opponents, etc., then it belongs to another subtype, *limiting competition*.

Positive consolidation occurs when an actor increases its long-term power. Fundamentally, this can be done in two main ways. 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 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.

The third type is *neutral consolidation* or *'embedding power'*. In this case, the consolidating actor neither makes 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. There are four subtypes here based on the parallel to the framework created by Merkel (2004). *Embedding power internally* happens when the various sources of power are made interdependent and consequently more stable. *Embedding power in the socio-economic environment* means creating an environment where social and economic conditions are favorable for the consolidation of power. *Embedding power in the international system* means that the power structure is supported by foreign actors, either tacitly or explicitly. Finally, *embedding power in civil society* occurs when the consolidating actor integrates certain elements of civil society in its power structure, or when it appeases or weakens the antagonistic civil groups.

Negative, positive or neutral consolidation can all happen both within the confines of democracy or could contribute to de-democratization as well. While in some cases, especially

in young democracies, consolidating power might even be crucial for the survival of the democratic system, the consolidation of power generally contributes to the erosion of the quality of democracy. Moreover, the consolidation of power and democratic consolidation are often inversely related, that is, the more consolidated a democracy is, the fewer opportunities there are for successful power consolidation for any actor.

3.2 The effectiveness of electoral engineering as a tool of power consolidation

The first hypothesis, i.e whether electoral reformers improved their partisan bias in the new system compared to the previous one, was consistently rejected in the analysis. This was due to statistically insignificant results in every model that war run; there was no indication that electoral reformers improved their performance after the reform. This is true regardless of the geographic region of the reformer, or the type and scale of the reform that took place.

The second hypothesis, namely that electoral reformers have a better partisan bias than non-reformers do, was also generally rejected due to statistically insignificant results. Reformers in general did not have a significant advantage compared to non-reformers. However, 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.

The third hypothesis posited that electoral reformers are reelected at a higher rate than non-reformer 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.

H4a retested the claim made by Andrews and Jackman (2005) that proportional reforms are introduced by parties with lower partisan bias, as they are inclined to 'level the playing field'. H4b also investigates the potential motivations for the direction of electoral reform, but is based on a slightly different premise, 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. These hypotheses was also rejected due to statistically insignificant results; 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 were rejected in general, indicating that electoral engineering is not an effective way to consolidate power. There are numerous potential reasons that could account for that. The first one is that electoral engineering is actually not as widely practiced as presumed by rational-choice institutionalism. That would indicate that the explanations of cultural modernization theory (Norris, 2004: 7) are more plausible: electoral reforms are mainly shaped by changes in society and public demand rather than the self-interest of reformers. Therefore, electoral engineering is often not possible, a new electoral system does not come about because those in power would like to change the previous one, but because the change in public attitudes. Therefore, electoral engineering is not even a common phenomenon.

Another possible reason for the null results is that there is so much uncertainty surrounding future electoral payoffs, that electoral engineering is often ineffective due to miscalculations on behalf of the reformers (Shvetsova, 2003). They are incapable of perfectly forecasting future electoral results and how the changes to the system will affect the results. They might particularly underestimate the potential electoral backlash of electoral engineering: voters tend not to support parties that change institutions for their own benefit. In other words, electoral engineering might be too complex to be an effective tool for power consolidation.

Furthermore, it is also possible that reformers are seeking non-electoral benefits when designing electoral systems. The new system itself could be an ideological goal for the party or a pledge made to voters. Consequently, parties could adopt electoral systems not because they expect electoral payoffs from the changes, but because the introduced modifications are important to them in and of themselves. In this case, parties are pursuing ideological goals instead of electoral benefits. Moreover, the two goals could also be linked: if voters expect certain electoral changes based on campaign pledges, then not delivering those changes could result in dissatisfaction and a consequent decline in popularity. Hence, even if a certain reform does not favor a reformer, it could feel compelled to adopt it to prevent the loss of support.

4. Conclusion

The dissertation establishes the definition and the classification of a novel theoretical concept, the consolidation of power. This conceptualization is useful to contextualize and understand current trends in politics that have previously been described from the perspective of the political system instead of individual actors. Furthermore, it is a concept that is a suitable basis for empirical inquiries.

Part 2 applied that conceptualization to one particular tool, electoral engineering. The analysis concluded that electoral reforms generally do not favor those who design them. This result has wide-ranging implications as it contradicts the consensus both in the literature and in the broader political discussion. The most concise way to sum up this part is as follows: contrary to the general consensus, electoral engineering is either not widely practiced or reformers are ineffective in at creating favorable systems for themselves.

In addition to the contributions of Part 1 and Part 2 to their respective strands of literature, the combined theoretical-empirical nature of the dissertation and the larger project it begins is also important. Describing how the consolidation of power works in practice is an important and interesting endeavor that is worthwhile to pursue, and the foundations for it are laid down in this dissertation.

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