PERSUASION OR LOYALTY: THE EFFECTS OF ELITE COMMUNICATION ON THE ELECTORAL SANCTIONING OF CORRUPTION

A Survey Experiment in Hungary

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Abstract This thesis offers a novel approach to the study of co-partisan biases in the electoral sanctioning. Prior research repeatedly showed the moderator role of partisanship on the sanctioning behavior of voters, however, no studies have looked at yet why voters use these biases in the first place. The thesis asks the question whether voters attenuate their sanctioning of co-partisan politicians to answer loyalty-based calls of their partisan identity, or because partisanship offers a channel for persuasion. Exploiting the heterogeneity of elite communication, the study contrasts the limiting effects of simple co-partisan cues to co-partisan elite persuasions on the sanctioning of corruption. The evidence for either alternative - loyalty-based sanctioning or a demand for persuasion - could have important implications for voter behavior and democratic accountability. A vignette-based survey experiment was designed to offer a first test of the hypothesized effects in Hungary in the context of political corruption at the local level. Although the multiple analytical strategies (OLS, 2SLS regression) do not offer clear results because of the unforeseen high rate of non-compliance in the experiment, the exposure of these challenges provide a potentially rewarding methodological and theoretical insight for the future research on the topic.

Keywords electoral sanctioning, party cues, elite persuasion, corruption, Hungary, survey experiments

I, the undersigned [Bence Hamrák], candidate for the degree of Master of Arts, the Central European University, Department of Political Science, declare herewith that the present thesis is exclusively my own work, based on my research and only such external information as properly credited in notes and bibliography. I declare that no unidentified and illegitimate use was made of works of others, and no part of the thesis infringes on any person's or institution's copyright. I also declare that no part of the thesis has been submitted in this form to any other institution of higher education for an academic degree.

Signature

Budapest, 22 June 2020

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List of Abbreviations

OLS Ordinary Least Squared

2SLS Two-Stages Least Squared

SATE Sample Average Treatment Effect

ITT Intent-to-Treat

CACE Compliers' Average Treatment Effect

IE Information Equivalency

SE Standard Error

IV Instrumental Variable

Introduction

Background

This experimental study is framed in a real-life political event that took place in Hungary, in the Fall of 2019. This story inspired the research question, and it suppose to show the relevance of the topic and the potential findings of the study.

In Fall 2019, Hungary was preparing for the municipal elections. The governing party's, Fidesz's chances to deliver a strong election result – as they did every time since the landmark election of 2010 - seemed to be very high. Even though, this election differed from the previous ones in some structural aspects (opposition-wide nomination of local candidates across the country), nothing extraordinary happened until late September, the finish of the campaign period, that could haven shaken the terrain of the competition. Then, on the 4th of October, the news broke: Pictures and then a video footage were published online by an anonymous blogger that showed Zsolt Borkai, the Fidesz-backed, ex-Olympic medalist and then incumbent mayor of Győr - an important economic stronghold in Hungary - on a yacht, in Croatia, with other well-known figures, and in the company of two women, while they were involved in sexual conduct. What started as a sex scandal with an enormous media and public attention soon turned into a full-fledged political scandal raising questions about the spending of public money, previous public procurements involving mayor Borkai and his close circle, and accusations of corruption. It was extraordinary, because the issues of public procurements and corruption was also

a sensitive topic for the government in general, posing a risk that the scandal can leave mark on the party and its election results.

In terms of the election outcome, it would be difficult to argue that Fidesz lost this election in the numbers[1], but they lost some key settlements – amongst them the capital, Budapest - to the opposition, and the communication advantage as well in interpreting the election results. It is difficult or even impossible to find the 'What would have happened if the scandal had not broke out?' counter-factual question, and it is definitely not this study's aim to attempt. It is rather the government's elite communication strategy around the scandal that led to the research question of this study. Fidesz and Borkai decided to stay silent on the issue. What would have happened if they had tried to explain or defend themselves against the accusations? Did Fidesz voters demand accountability for the events, and we have seen a potential backlash (active or passive) of the voter base because of the lack of answers? Do even partisan voters require some explanations? In other words, do partisans have to be persuaded to limit or to avoid sanctioning, or partisan loyalties cease the necessity of such action?

Partisan identities and electoral accountability has a conflicting, even opposing relationship. But does partisanship simply as a loyalty reduce voter punishment, or even partisanship has to be accompanied by persuasive efforts to attenuate electoral sanctioning? Therefore, the kind of counter-factual question that inspired the research was not the one 'what would have happened if the scandal had not broke'. Given the scandal and its management, the question is rather how the reaction of the Fidesz elite in their communication influenced the sanctioning behavior of the their co-partisan voters. Did we see Fidesz voters' moderated sanctioning on partisan bases at a full effect, or a more engaging elite communication strategy would have resulted in stronger partisanship-based sanctioning? In essence, is attenuated co-partisan sanctioning a simple question of identity reconciliation, or a result of elite persuasion?

[1]For a full analysis of the results, see the study from the Political Capital and the Friedrich Erbert Stiftung (2019)

Research Summary

This study joins two large research traditions in voter behavior: It is connected to the research of electoral sanctioning and elite opinion leadership. The contribution and novelty offered by this thesis is the combination of the two theoretical arms of voter and elite behavior by exploiting a gap in the past research on the phenomena: Even though partisan biases are a well-known and empirically proven moderators of electoral perception, accountability and sanctioning (e.g. Bartels, 2002; Rudolph, 2006; Malhotra & Kuo, 2008; Tverdova, 2011; Tilley & Hobolt, 2011; Anduiza, Gallego, & Muñoz, 2013; Kmetty, 2019), their source mechanisms are not well explored. Prior research mainly focused on the consequences of partisan biases in the electoral sanctioning, while the possible ways partisanship comes into effect was largely ignored in the studies. This leaves us with the question whether voters are expressing partisan loyalties or they are rather persuaded when they limit their sanctioning of co-partisan politicians. The main objective of the study is to conceptualize these different paths, and offer a first test of their relative power in eliciting partisan biases. The research on the elite's influence on public opinion comes helpful when exploring these dimensions. Elite messages are a mixture of cues and arguments or persuasion (Broockman & Butler, 2017). Decomposing these components, and comparing them in terms of their effects in the context of electoral sanctioning could reveal important lessons about the decision-making processes of voters when they attribute responsibility and blame for co-partisan corruption.

In a normative sense, the research question is whether any accountability exists beyond partisanship. This question is relevant because even if we are aware of the expansion of partisan behavior both at the level of the public and the elite, we do not have definitive scientific answers yet how far partisan behavior has spread in democracies. Whether partisanship-based position-taking is not only influencing the public's policy views, but also the core mechanisms of representative democracy, the citizens' ability or will to hold politician accountable for their conduct in office or for corruption for example. Therefore, in terms of the deliverable of the thesis, I seek to answer both empirical and normative

questions on how co-partisanship drive voter decision-making in sanctioning. An answer whether electoral accountability - in its persuasive, subjective rather than objective form - exist beyond partisanship, or partisan voters only follow cueing messages (Zaller 1992) where they make decisions on sanctioning only by the virtue of the actor involved.

The research strategy to offer a solution for these questions will be manifested by the comparison of two elite communication components, the effect of partisan cues and partisan elite rebuttals on the electoral sanctioning in a situation of co-partisan corruption. These will represent the theorized channels of how co-partisanship elicits its moderating effect on electoral sanctioning, through a call for partisan loyalty reconciliation or through partisan persuasion. Whilst partisan or source cues are passive elements in elite communication, offering only information about the identity of the actor, elite arguments involve active persuasion focusing on changing minds or creating opinions. Even though it can be argued that voters' listening to either is not essentially different, as the behavioral responses for both are sharing the same root, preexisting partisan attitudes, elite persuasion is certainly closer to voter accountability even in its partisan form: It suggests that partisan voters require explanations and justifications beyond the simple cognitive shortcuts provided by source cues to side with their partisan identities. It is equally possible that in the composite co-partisan elite messages cues are sufficient for attenuating co-partisan sanctioning, or that they are necessary but not sufficient. In other words, cues and persuasion in sanctioning situations may need to accompany each other to maximize co-partisan moderating effects. The processes evoked by each in the voters' decisions are not mutually exclusive, and this study is not seeking a 'one or the other' answer. It rather aims to explore the size of their role in the electoral sanctioning. Is their any benefit for elites to make persuasive efforts beyond offering cues?

As of my hypothesis for the effects, I assume that co-partisan voters attenuate their sanctioning by a *choice* of identity reconciliation rather than as a result of *persuasion*. Therefore, elite persuasions will not elicit an extra effect on attenuated co-partisan sanctioning beyond the effect of the simple partisan cues since cues already offer a motivation

for the preexisting directional goals of partisan identities (Rudolph 2003, 2006). I argue that if co-partisan sanctioning - assuming partisanship is a true moderator as previous research has shown - is truly about loyalties, persuasion offers no qualitative difference in partisan voters' decision-making.

The study deployed a survey experiment amongst Hungarian partisan voters to study these varieties of co-partisan effects. The stimuli shared information about a Hungarian, small-settlement mayor who was accused of corruption. In the vignettes, the design varied the elite communication environment of the corruption news. Then subjects were asked about their voting intentions for the mayor. This manipulation and measurement strategy will allow us to conduct an analysis on how voters sanction co-partisan corruption depending on the availability of the partisan cues and the elite persuasion. A nationally representative sample was recruited composing of partisan voters of both Fidesz and the Opposition.

Outline

The thesis will pursue the following structure: First, I introduce the relevant theories and the prior research on electoral sanctioning and elite communication in order to establish the conceptual background of the forthcoming experimental design (Chapter 1). Then, I pose the main assumptions and the arguments of the study (Chapter 2). Third, I introduce the experimental design (Chapter 3): I will detail the experimental conditions and the experimental procedure. Then, the manipulated variables and the stimuli will be described. Followed by this, the measurement strategy and the outcome variables will be formalized. Then, the sampling procedure and the analysis plan will be documented. Fourth, the data will be analyzed (Chapter 4): I will start with the description of the obtained sample, its characteristics and missing data. It will be followed the statistical analysis of the main effects, the comparison of the treatment groups in terms of the outcomes. Finally, a section will be devoted to the qualitative discussion on the results

and the challenges arising in the interpretation of those (Chapter 5). The conclusion will summarize the findings and the potential avenues for future research on the topic (Conclusion). The thesis is also accompanied by a detailed documentation of the pre-analysis stage of the research. In the Appendix[2], I share the documentation and the supplementary materials for the research: The pre-analysis plan, the survey questionnaire, the raw data file and a code book, and the R Studio script of the statistical analysis. In Appendix B-D, I provide the diagnostics for the experimental procedure and the sample for the upcoming analysis, whilst Appendix E-H offer supplementary analysis for the main results discussed in Chapter 4 and 5.

[2]Appendix A - Data Documentation and Supplementary Materials

Chapter 1

Theory

This study's goal is to understand why voters sanction co-partisan wrongdoings to a lesser extent, or in other words the role of partisanship and its mechanisms in electoral sanctioning. The theoretical chapter's goal is to introduce the core concept of voter accountability in the empirical reality, the electoral sanctioning and a detailed look at its limits and contingencies drawn by previous research. Then, the study takes a closer look at one moderator, partisanship, and explain its consequences for the electoral sanctioning. Finally, I re-introduce the problem tackled by this thesis, namely exposing the incomplete understanding in the literature how partisanship elicits its effects: Prior research mainly focused on the consequences of partisan biases in the electoral sanctioning, while the possible ways partisanship comes into effect was largely ignored in the studies. The thesis borrows the relevant theories of partisan biases from the elite communication literature to establish the ways partisan biases in sanctioning can come into effect. The breakdown of elite messages to party cues and persuasive efforts or elite arguments offer two viable – although non-exclusive – mechanisms how partisan communication can activate partisan biases in the electoral sanctioning. The main objective of the study is to conceptualize these different paths, and offer a first test of their relative power in eliciting partisan biases. As argued in the introduction, the implication could be consequential for our understanding how voters make decisions on sanctioning, and how they process partisan

information which also promises a normative lesson for democratic accountability.

1.1 Electoral Sanctioning

Electoral sanctioning is an empirical derivation of the normative theories about democratic accountability and voter accountability. The normative assumption in democratic theories is that voters hold their representatives accountable. Democratic accountability is one of the cornerstones in the justification for representative democracy. The corresponding model in voting behavior is electoral sanctioning or electoral punishment. The concept is very straightforward: When voters go to the ballots, they formulate their vote choices based on their evaluation of the past performance and outcomes of the incumbent government. The historical background of electoral sanctioning starts with the establishment of the theory of economic or retrospective voting, or the so-called 'punishment-reward' models (for an overview, see Anderson, 2007), one of the milestones in the research of voter behavior. The first influential theories of retrospective voting were built on the observed effect of the incumbents' past political and economic performance on their vote share (Key, 1966; Kramer, 1971; Lewis-Beck & Stegmaier, 2000). These theories assume that voters have an objective perception of the reality out there and an unbiased behavioral response to this perceived input/information, accumulating in their votes.

Theories of electoral sanctioning were extended beyond economic voting. Many studies argued and showed that the same model of voter behavior can be applied to fields and spheres outside of the economy, to the performance of politicians at several levels of politics, not just the macro-level of economic conditions. Perceptions, evaluations and behavioral responses to corruption were studied in the framework of sanctioning models (Anderson & Tverdova, 2003; Tverdova, 2011). Electoral punishment models were also tested on the electorate's reaction to individual cases rather than on systematic and complex phenomena like economic conditions and levels of corruption. One example is the study of the determinants of the public's response to the Hurricane Katrina

and their attribution of responsibility for the political actors' performances (Malhotra & Kuo, 2008). However, parallel to the expansion of theories of sanctioning to other territories in political science, this straightforward relationship pictured by the original economic/retrospective voting was also challenged by establishing the conditionalities and contingencies in the voters' ability and motivation to sanction in this objective manner. This pure and essential theory of electoral accountability was questioned by many accounts, causing electoral sanctioning to lose some ground in its status of being the justification for normative theories of democratic accountability. As Anderson (2007, p. 279.) puts it "this translation of objective economic conditions into voter motivation is anything but straightforward", and "from a normative perspective, contingent accountability is clearly problematic, and it calls for a reconsideration of the normative underpinnings of the economic voting paradigm" (Anderson, 2007, p.271.).

These limits of objective sanctioning can have various sources. One type of commonly articulated source of bias in sanctioning is the institutional context where voters make decisions, or vote. Some types of governments[1], and party systems[2] can affect the clarity of responsibilities for the performance that is supposed to serve as the base of voter evaluation (Tavits, 2007; Anderson, 2007). More importantly for our purposes and attracting an enormous attention from the students of voter behavior, biases in electoral sanctioning also have several causes on the level of the individual rather than in the context, questioning not only the ability of voters to sanction objectively, but also their *motivation*. According to De Vries & Solaz (2017), these biases can modify responsibility judgements and the corresponding behavioral responses like vote choices even in the possession of clear, factual information. In other words, beyond acquiring the information about the performance or conduct of a politician or a party, the information also needs to be causally attributed to the source or the *correct* source (Tilley & Hobolt, 2011). Attribution of responsibility and blame is a key mediator in the chain of electoral sanctioning. According to Rudolph

[1]Coalition governments common in multi-party systems, divided governments in the U.S., or cohabitation in France.

[2]Multi-party, fragmented systems like in the Benelux states.

(2003, p.700.), "The concept of responsibility lies at the heart of theories of democratic accountability (...) responsibility is the chief mechanism through which individuals hold actors accountable for their conduct.". As Malhotra and Kuo (2008, p.121.) argues its importance in electoral sanctioning, attribution of responsibility "allows individuals to connect events to actors".

While institutional context affects the ability of the perception of performance or conduct, individual-level biases target the attribution process. A significant stream of studies in the literature explain the biases in the attribution of responsibility by social-psychological processes influencing voters' attitude. Identity-driven biases - relying on Tajfel's (1981) social identity theory - can manipulate correct (objective) assignment of responsibility. According to these explanations, in-group loyalties (Solaz, De Vries, & de Geus, 2019) or group-serving attribution biases (Rudolph, 2003; Fiske & Taylor, 1991) modify the voters' sanctioning decisions in accordance with the identity of the actor or the group who is the target of the sanctioning. Partisan allegiances were proven to have a limiting effect on people's corruption perception and vote choice (Anderson & Tverdova, 2003; Tverdova, 2011; Anduiza et al., 2013). Ethnic identity was shown to have similar effects: In their experiment, Solaz and colleagues (2019) showed how ethnic identity can attenuate electoral consequences – a withdrawal of support – for politicians involved in corruption. A similar study conducted by Rudolph (2003) shows that factors like economic ideology, institutional context and partisanship moderate the effects of economic perceptions on presidential and congressional approval.

Therefore, the objective perception and evaluation of political performance and conduct is a questionable assumption. Voters respond differently to information that has political consequences, and this response is not only shaped by circumstantial factors affecting voters' ability to evaluate performance and conduct, but also by the individual factors - attitudes and identities - that affects voters' motivation to sanction or reward. In short, sanctioning is a heterogeneous model of voting behavior (Shabad & Slomczynski, 2011), where the effect of the perception of the same facts or events on vote intentions

shows a systematic variation in case of the individuals, driven by mainly individual-level characteristics like personal traits, ideology or partisanship.

1.2 Partisanship and Electoral Sanctioning

As pointed out earlier, partisanship is one of the main identity-based biases in the process of electoral sanctioning. It is arguably the most consequential heuristic when it comes to the opinion formation of the voters about political matters. Therefore, it should come by no surprise that it is one of the most commonly researched and evidenced moderator of electoral sanctioning. Thanks to its prestigious position among the 'most wanted' culprits, we have a vast amount of causal evidence on partisanship's role in sanctioning. These evidence cross-cut geographical and thematic contexts which make the findings fairly generalizable.

Tilley and Hobolt (2011) investigated the role of partisanship in the blame or reward attribution in the economic performance evaluation of the government in the United Kingdom. They found that while partisanship has smaller effect on performance evaluations, it strongly influences the attribution of that evaluation. In contrast, Bartels (2002) using a U.S. panel data found that partisanship already influences the perception of objective facts. In the Spanish political context, Anduiza and his colleagues (2013) showed that besides performance evaluations, partisanship is also an important moderator in voters' willingness to sanction corruption. When exposed to the co-partisan link, voters limit their sanctioning of corruption. Similarly, Munoz and colleagues (2016) tests the effects of co-partisan arguments on the electoral sanctioning of corruption in Spain. They test and prove a specific argument that partisanship reduces the credibility of a corruption accusation, and therefore attenuates corruption sanctioning. However, they are not looking for which part of the partisan messaging drives home the effects in specific, the partisan cues or the credibility arguments, an important point of difference that will be the focus of this research. Malhotra and Kuo (2008) studied the effects of partisanship on

the voters' blame attribution in an episodic event, Hurricane Katrina. They found that partisanship indeed influences who the public blame's for the failed crisis management, but it is not an absolute effect: When the job title/position of the person to be judged also shared with the respondents, they seem to make reasoned judgements about responsibility. In a Hungarian context, Kmetty (2019) showed in a survey experiment that in contrast with the information hypothesis, assuming that certain partisan voters simply do not receive reliable information about corruption and partisanship as a moderator on sanctioning is heavily confounded therefore, information exposure do not eliminate the differences between respondents' vote choice conditioned by their partisanship. Moreover, those who consumed dominantly pro-government media were even more likely to vote for the government after the exposure with the accusations of governmental corruption.

What else can moderate electoral sanctioning of corruption? Even though partisanship is a very influential moderator of sanctioning, it is not an absolute condition to observe attenuated sanctioning. In the context of corruption sanctioning, the pioneer experimental study by Rundquist and his colleagues (1977) showed that voters may reduce their sanctioning behavior when they expect material inducements or returns from the corrupt action, the so-called 'implicit exchange theory' of corruption sanctioning. Similary, Munoz and colleagues (2016) replicated the findings of Rundquist (1977) about the role of implicit exchange when voters judge corruption. Fernández-Vásquez and his colleagues (2016) also shows the importance of material compensation in attenuating the electoral consequences for corruption, using a Spanish data of local elections in 2011 which was surrounded by corruption allegations of projects that also offered side-benefits for a significant part of the electorate. Accounting for the multi-dimensional environment of sanctioning, several conjoint choice experiments were conducted where researchers measured the different factors' simultaneous effect on the sanctioning of corruption, instead testing these in isolation (Klašnja, Lupu, & Tucker, 2019; Breitenstein, 2019; Franchino & Zucchini, 2015; Mares & Visconti, 2020). These studies observe and benchmark the known influential effects of vote/candidate choice (performance, traits, partisanship, ideology, policy view) and corruption against each other.

In sum, studies on the moderator role of partisanship in electoral sanctioning are generalized over geographical contexts and themes (performance, crisis management, conduct in office). Partisan bias proven to be an influential moderator of sanctioning, however its effect is not absolute, and it is not the exclusive aspect of voter decision-making. However, there is a lack of discussion whether partisanship in itself as a label matters, or it works by re-enforcing persuasive effects. This is the point, where I can re-introduce the problem tackled by this study. By selecting partisanship as the studied moderator, I expose a well-researched, but still incompletely understood phenomenon of electoral sanctioning. The problem discussed here comes from the observation that despite the central role of partisan biases in the selective sanctioning behavior of voters, we lack the possible explanations and evidence on why exactly co-partisans react in the observed ways to partisan biases. We have a rich explanations and evidence on its consequences for the attribution of blame and further behavioral responses, but it is still to be discovered how partisanship sets off its chain of consequences in the sanctioning process. Do voters answer loyalties or partisanship elicits its effect through persuasion?

1.3 Elite Communication

As we cannot read voters' mind, we need to find the external source of these effects to develop explanations. As these biases are evoked by and ultimately dependent on the communication environment in which sanctioning takes place, elites are a group of actors who are responsible for activating and pulling these effects. Therefore, elite communication theories could represent the ways partisanship interacts with sanctioning and can be used as a vehicle to develop the explanations on why and how voters listen to co-partisan calls in sanctioning. We use the heterogeneity of elite messages to conceptualize the ways how partisanship comes into effect in sanctioning. One component is the partisan/source cue theories (Zaller, 1992; Druckman, 2001), whilst the other one is theories of elite persuasion (Chong & Druckman, 2007; Cohen, 2003).

In terms of the empirical evidence, there is a vast amount of research on the effect of elite's position on public opinion (Zaller, 1992; Abramowitz, 1978; Gabel & Scheve, 2007; Arceneaux, 2008; Lenz, 2009), however, as Butler and Broockman (2017, p.3.) highlighted it correctly, these elite messages usually contain 'a mix of cues and persuasive messages', it is generally hard to subtract the components' individual effect on voters' position from the total effect – especially in non-experimental studies where the manipulations are not engineered. The components could support different sub-theories of elite influence on voter sanctioning (as summarized by Broockman & Butler, 2017): The effect of source cues individually would imply the so-called position adoption theory (Lenz, 2009; Cohen, 2003) where voters simply adopt their co-partisan position on an issue by the virtue of the actor's identity, whilst the necessity of persuasive messages in form of elite arguments could provide a support for the more restricted elite influence theory, elite persuasion (Chong & Druckman, 2007; Grose, Malhotra, & Van Houweling, 2015), where voters demand justifications too in order to follow the elites' lead and attenuate their sanctioning when it comes to co-partisan corruption. In the original comparison of the elite communciation components, Butler and Broockman (2017) showed that beyond cues, extensive arguments do not elicit substantial effect on voters' position, supporting an issue adoption view of voters' reaction to elite messages. However, in my study's case, because of the more serious or bad valence implying decision situation, voters may demand some kind of accountability even when the sanctioning scenario is conditioned by partisanship.

In preview, this study mimics in nature the comparison of the elite communication elements' effects in voter behavior in the Butler and Broockman (2017) study, but introduces an important divergence: It places these compared effects in the circumstance of electoral sanctioning, therefore proposes potentially different findings. Namely, in the heightened situation of electoral accountability that surrounds electoral sanctioning, cues may not be enough for partisans, but they may need to be persuaded. In other words, in the case of the electoral sanctioning, position adoption may be a weaker explanation than elite persuasion for the limiting/moderating effect of partisanship in the sanctioning behavior of partisans. The elite influence theories usually test the effects in the context of

policy issues. This is an important domain restriction for our purpose. The context, especially the severity of the accountability situation differs in case of elites' policy positions and their conduct or performance in office. As Peterson and Simonovits (2018) showed, the issue salience influences the effect of elite frames on voters' behavior. The high valence issue that usually surrounds situations of electoral sanctioning may prove to be moved with more difficulty by elite messages. Consequently, the situations of heightened electoral accountability could change the relative effects of simple cues and persuasive messages. In normative terms, sanctioning could be closer to a partisan form of electoral accountability where co-partisan actors are required to justify their performance or conduct, in contrast with the position adoption by voters shown in the case of policy issues.

As Carmines and Stimson (1980) phrased it, there are 'harder', technical, mean-oriented issues, and 'easier', end-oriented, symbolic issues. Policy issues can be a 'harder' and more 'obscure' themes in voters' opinion, while the instances of wrongdoings like corruption can carry normative bearings where voters form 'easier', 'clearer' and stronger positions. According to Nicholson (2011), source/partisan cues may fit the harder issues, while symbolic, high-salience issues can be resistant to source cues. This could raise the bar for the effectiveness of elite influence over voters' choices, for example in terms of requiring some forms of accountability by demanding justifications – or any other form of persuasion. In terms of this study's compared effects, in the case of corruption sanctioning, for partisanship in order to attenuate sanctioning, simple cues may be insufficient alone, and persuasive efforts – arguments against the accusations – could be demanded by co-partisans to reduce their punishment.

In sum, regarding our main query - the reasons why partisan voters listen to co-partisan calls in sanctioning - these components may imply different mechanisms for the decision-making process of the electorate. Instrumenting elite messages and decomposing it to its components - to cues and the arguments - can offer us a new angle to approach the relationship of partisanship and electoral sanctioning. Using simple heuristics like partisan/source cues would imply that partisanship can be important in itself as a loyalty

call. Otherwise, partisanship can make elite persuasion effective. These channels could be neither exclusive nor distinct processes at all, but the portion they own from the general effects of partisanship on sanctioning promise an important lesson for the research question of the study, whether voters rather sanction based on loyalties or by partisan persuasion. These have different normative and practical implications, as it has been articulated in the introduction.

Therefore, the decomposed elite communication components offer a viable strategy to operationalize and and test the varieties of ways voters can listen to co-partisanship, thereby accounting for its effect. These will represent the assumptions and formulate the key variables in the experimental manipulation. In short, elite communication offers us a bridge to study how voters' sanction based on their partisanship.

Chapter 2

Assumptions

Hypothesis 1: Exposure to political wrongdoings does elicit electoral sanctioning, even in the case of co-partisan voters.

Hypothesis 2: The extent of sanctioning is conditional on the accessibility of co-partisan cues. Co-partisan cues attenuate sanctioning effects.

Hypothesis 3: Elites' persuasive messages alone, without a reference to the partisanship of the actor, elicit a small, non-substantial limiting effect on electoral sanctioning.

Hypothesis 4: When partisan cues are already available, the addition of further argument by elites against accusations elicit no extra effect in reducing the sanctioning behavior of co-partisan voters.

Hypotheses 1 and 2 are essentially made in an effort to confirm previous research. On the baseline, exposure to corruption evokes sanctioning effects, however its magnitude is conditional on partisanship. Hypothesis 3 intends to test the assumption that persuasion without the strong heuristics of partisan information offer a small, non-substantial remedy for sanctioning effects. The possible explanation is that in the absence of strong source cues, arguments against corruption lack a strong mediator that co-partisanship could offer. Arguments alone are weak instruments against the strong normative judge-

ment motivation evoked by the exposure to corruption. Finally, Hypothesis 4 assumes that in the availability of these partisan cues, partisan voters do not need to be persuaded, the cues turn the question of sanctioning into a situation of expressing partisan loyalties. Despite of the previous considerations about the differing situations in which elite messages elicit their effects in the theoretical chapter, I argue here that cues will maximize the moderating effect of the co-partisanship we will examine in the sanctioning situation constructed by the study, and the addition of persuasive messages by the co-partisan elite actor will not elicit further decrease in the respondents' sanctioning behavior.

I take the position that voters are less persuaded or manipulated into believing simple denials or justifications for wrongdoings rather than consciously choosing to endorse co-partisan actors to express their political identities. This is similar to the arguments made by the proponents of motivated reasoning theories (Kunda, 1990; Rudolph, 2006; Leeper & Slothuus, 2014; Flynn, Nyhan, & Reifler, 2017). Motivated reasoning suggests that voters - as a consequence of biases induced by partisan cues for example - digest information with setting clear directional goals that align with their identities or attitudes beforehand. These goals influence how the voters eventually attribute responsibility for given actions. In a world where voters believe political facts and express political opinions based on goals that are made prior to and independent of the information they learnt, I argue that persuasion has a lower or no importance compared to cues in decision-making as the outcome decision under this view is already set before acquiring the arguments exclusively based on the voters' relationship to the actor. Under this view, I argue that the effect of partisan cues and partisan elite persuasion on sanctioning is qualitatively equivalent: The outcome of voter decision is not made based on the content of the message (the persuasive effort), but only based on the identity of the actor who is target of the sanctioning.

On the other hand, this argument could be refuted if elite persuasion would not serve the role of changing voters mind by manipulating co-partisans to believe 'cheap talks', but by only improving the motivated reasoning based decision-making by offering a confirmatory or justification source for the co-partisan voters directional goals in sanctioning.

Therefore, persuasion could actually matter, but not for its primary virtue. Some motivated reasoning theories like Leeper and Sloothus' (2014) high and low effort directional goal categories actually imply the possibility of differential effects of simple cues and partisan persuasion. While low effort directional goals lead co-partisans to adopt positions and practice endorsements just by the virtue of the identity of the actor, high effort directional goals require the confirmatory link between the goals and the action (lower or no sanctioning). In this view, Hypothesis 4 could be refuted as elite persuasion could elicit extra effects on co-partisan biases in sanctioning even if these could be restricted to improve the loyalty calls of the co-partisan identity. This would create a hybrid version of the two theories about why voter respond to co-partisanship in sanctioning which would state that persuasion is required (or beneficial) in sanctioning situations, but not for their primary function - changing minds, but only to strengthen calls for loyalty reconciliation in sanctioning.

Therefore, if Hypothesis 4 is confirmed the implications are quite clear: Co-partisan sanctioning is driven by motivated reasoning where the question is not the sanctioning per se, but the voters' relationship to the identity of the actor who is target of sanctioning, and therefore, cues condense all the necessary information for co-partisans. Simplifying the matter, the question is not *what* to sanction, but *who* to sanction. Persuasion deals with the what dimension, cues deal with rather the who aspect. In contrast, if Hypothesis 4 is refuted, the potential implications are a bit more ambivalent: Partisan persuasion is either effective beyond cues because it changes partisan minds, or just because it complements loyalty reconciliation motivations with a confirmatory or self-justifying manner. Even though the line seems thin between the two implications, it would depict two different worlds both for the practical question - in terms of why voters use partisan biases in sanctioning - and for the normative issue at stake - whether any practice of accountability exists beyond partisanship: It would matter whether persuasion has a pro-active or reactive, supporting role in partisan decision-making.

These assumptions will be tested in an original survey experiment. The experimental

design is introduced in the following with an aim to turn the concepts and the assumptions into measurable quantities offering testable implications for the research's queries.

Chapter 3

Experimental Design

3.1 Overview of the Experiment

The study conducted a survey experiment in Hungary to study the effects of the different elite communication components on sanctioning behaviour when it comes to co-partisan corruption. Subjects read vignettes about a mayor who manipulated a public procurement to favour his own economic circle. The experiment deployed a between-subjects fractional factorial experimental design with 2x3 conditions: Two blocs and 3-3 groups within the blocs. One bloc displayed partisanship information, the other does not. In each bloc, there were 3 congruent treatment groups. A control group, a corruption news exposure group, and a news exposure and elite rebuttal group.

3.2 Procedure

The study followed a standard experimental procedure. First, participants read the description of the survey and signed a consent form (a standard process by the polling firm). As part of a pre-treatment measurement, at the beginning of the survey, I asked several partisanship questions to select subjects who can be assigned to the treatment conditions

as the experiment's units of analysis were co-partisan voters. Pre-filtered participants were then randomly assigned into the treatment or control groups in a double-blinded procedure by the survey software. The study used full randomization for the vignette assignment. Random assignment to treatment groups was performed by the survey platform used for the data collection (Lime Survey). Treatment groups were set to have equal sizes. The vignettes within the treatment groups were not randomized, they had a fixed order (description of the politician, corruption news, elite rebuttal), however, treatment groups differed on how many vignettes subjects assigned to the specific group read (see experimental conditions below). Participants subsequently proceeded to the experimental stimuli (vignettes) described below where they read about a case of local political corruption about a small-town mayor in Hungary, the political affiliation of whom is adjusted to their partisanship (based on pre-treatment measurement) in the corresponding conditions. After showing the vignettes, subjects answered two questions which served as the outcome measures for corruption sanctioning. Later, subjects were tested in two manipulation check questions to ensure compliance and the receipt of the treatment. The experiment was concluded by debriefing the subjects about the hypothetical nature of the study.

3.3 Experimental Structure

3.3.1 Experimental Logic

The experiment works on two levels: First, it observes the general effect that co-partisanship plays in the electoral sanctioning of corruption. This inquiry is reflected in the two-bloc structure of the design. The study observes the effect of corruption exposure in a partisan and non-partisan setting. Second, beyond the general comparison between the partisan and non-partisan blocs – a replication attempt of previous findings on the effects of co-partisanship on sanctioning -, the design also offers a comparison of the different communication environments in form of which the partisanship can elicit its effect on the

sanctioning behaviour of voters. These within-bloc differences between the studied elite communication situations will be used to answer the main query of the study - what is behind the effect of co-partisanship on sanctioning. These are sub-effects attributed to the cues and elite persuasion behind the composite elite messages. The design that follows this logic was constructed by the experiment: Two baseline, control groups against which the effects of corruption exposure in the two news exposure groups can be measured in a co-partisan and non-partisan world, and two elite rebuttal groups, a co-partisan and non-partisan where the persuasive effects can be measured against the non-partisan or co-partisan news exposure.

3.3.2 No Cue Bloc

The control group respondents read a description of a politician, but did not receive the corruption news featuring the politician. This group intended to serve as the baseline measurement of the featured politician's approval, where voters were allowed to acquire knowledge about the politician – except for his partisan affiliation. The second group in addition received the corruption news but still without co-partisan affiliation. This intended to serve as a benchmark for corruption sanctioning attitudes by measuring the effects of news exposure on approval without any source cues or elite arguments. In the elite argument treatment group subjects read in addition to the previous texts a short response/argument against the allegation from the involved actor, however, the politician's partisanship was still unknown.

3.3.3 Cue Bloc

The partisan control group where subjects read the same description, but with a co-partisan affiliation. This group intended to serve as the baseline measurement of the partisan approval by allowing for source cues. The second group read the description and received the same corruption news as in the no cue bloc but with partisan affiliations. This

measured the effects of news exposure conditional on co-partisanship. The final treatment group read the description and received a vignette featuring both the partisanship and the elite argument against the corruption allegation.

This design will help us to observe the relative effect of the news exposure and elite rebuttals on the extent of sanctioning in the cue and the no cue experimental blocs, therefore enabling us to study the central effect of partisanship and its hypothesized source mechanisms: Partisan cues and persuasive messages.

Mean Vote Likelihood	**Experimental Bloc I. No Cue Party Cue = 0**	**Experimental Bloc II. Cue Party Cue = 1**
Corruption News Exposure = 0	Control Group - Only Bio	Partisan Control Group - Only Bio
Corruption News Exposure = 1	News Exposure Group	Partisan News Exposure Group
Elite Rebuttal = 1	News and Rebuttal Group	News and Partisan Rebuttal Group

Table 3.1: Experimental Structure

3.4 Experimental Setting

The experiment was set up in Hungary where the issue of corruption is in the front of the media and political agenda, and corruption perception is high by voters, and this perception is divided along partisan lines (Kmetty, 2018). Therefore, a hyper-partisan context was provided for studying of the corruption sanctioning which was suitable to observe the moderating effect of partisanship and its elite communication-related causes. However, the generalizability of the potential results has to be adjusted to the specificity of the setting, namely, the effect of partisanship and the way voters react to different elite communication components may differ in less polarized political and media environments.

3.5 Manipulated Variables

The manipulated variables were operationalized based on the elite communication concepts introduced in the conceptual chapter. They correspond to the assumed ways partisanship can elicit its effect on voters' sanctioning. The experiment manipulated three variables, each coded on a binary variable: Whether the co-partisanship of the mayor described in the vignette was displayed for the respondents, or not, whether respondents were exposed to the corruption news about the mayor, or not, and finally, whether the mayor was allowed to make a rebuttal against the accusations, or not. These binary variables were embedded in the experimental groups in the combinations shown in Table 3.1.

3.6 Vignettes

The experiment employed a two-step vignette structure: Before receiving the treatment, all subjects read a short 'biography' of the mayor who is later featured in the corruption news manipulation, in the second part of the vignette. This biography described the political and personal background of the mayor who was later depicted in the corruption news – in the corresponding treatment groups.

BIOGRAPHY:

Németh László is the (*partisanship*) mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the (*partisanship*) politician.

The aim of the first part of the vignette was to allow subjects to learn more about the politician. This description intentionally shares positive traits about the mayor (evidenced

by previous experimental studies in candidate choice: (Mares & Visconti, 2020; Klašnja & Tucker, 2013; Breitenstein, 2019; Franchino & Zucchini, 2015). I thought that this positive description of the politician can reduce the obtrusive nature of the corruption news which could artificially accelerate the willingness to sanction, when voters must evaluate an actor they have no prior information about. As I did not vary these traits in any conditions, I excluded the possibility that they confound our explanation about the effects of the main manipulation. This is not an artificial intervention – as in most of the cases, voters do actually possess background information on the traits and performance of the politician, therefore corruption is not sanctioned in an information void, and this design may have enhanced the ecological validity of the situation.

The second part displayed the corruption news in which the different elite communication components were be varied. In the event, a fictional newspapers' news piece displayed a hypothetical corruption case in a fictional settlement in Hungary where the previously introduced mayor has been accused to manipulate a public procurement. It was followed by a rebuttal from the mayor. Hypothetical situation was chosen to avoid any potential pre-treatment exposure or media source bias. Some treatment groups have only seen the partisan cues while other groups have also read a rebuttal from the mayor against the corruption accusations – as described in the experimental conditions above.

Trans-Tisza Herald
Nyírbánság's (*partisanship*) mayor, Németh László has manipulated several tenders to favour his economic circle

A judicial case has been initiated against Németh László the (*partisanship*) mayor of Nyírbánság, after he has been accused with the manipulation of several public procurement. According to the alleged evidence, the mayor influenced the results of the procurement for the new sport facility in Nyírbánság. He assumedly received bribe money in return. The bribe could be connected to a businessman close to the (*partisanship*) mayor. The (*partisanship*) mayor firmly rejected the accusations, because those – according to his statement - „are a result of an organized political witch-hunt without any substantial evidence". According to the mayor, his political opponents are „envy of his successes as a leader of the town", and "his accomplishments that helped the city and its citizens" (...)

The operationalization of partisan cues was straightforward: party labels – if made available - were adjusted in the vignette based on the declared partisanship of the respondent from the pre-treatment stage. The elite argument was operationalized based on the so-called 'credibility hypothesis'. The effectiveness of the elite blame management strategy which is based on discrediting the information was evidenced by Munoz and his colleagues (2016). I decided to choose 'credibility arguments' as the elite argument type against corruption allegation in this experiment because of the recent prevalence of these types of blame managing arguments in the elite discourse today (see for an example Van Duyn and Collier (2019) on the fake news arguments in contemporary elite discourse).

I also have to declare some limitations of this operationalization of elite communication effects on partisan sanctioning which can be again consequential for the generalizability of the potential results. This study did not observe a variation of elite argument types that manage blame for corruption: It can be several, deploying different mechanisms through which persuasion could work. Here, I opted for the assumedly most frequent one, the denial of the accusation based on discrediting the information. Further limitation with the operationalization is that I also did not vary the accusation source that could again vary the relative strength of elite communication components. Here the judicial process may have evoked stronger voter sanctioning based on the perceived seriousness of the situation indicated by the investigative actor, the judiciary. In other cases, if the actor is more politicized – like another party or a media outlet – the respondents' trust in the investigative actor may be lower, causing less sanctioning, and potentially more effective elite communication.

3.7 Measurement Strategy

3.7.1 Dependent Measures

The main outcome was measured by the vote intention of the subjects for the mayor where I expected to observe sanctioning effects accumulated in vote choices[1]. The question I asked respondents was to indicate their likelihood of voting for the mayor on a 1 to 5 rate - ranging from 'Not likely at all' to 'Very likely' - if the subjects would live in the featured settlement and local election would be held the next day. For the analysis, all outcome variables were re-coded to 0-100 scale for the ease of the analysis, where 0 meant the minimum score, while 100 the maximum. I decided to choose vote intention as the outcome measure because this is the ultimate stage, the manifestation of electoral sanctioning. Although a potential conflict between the condemnation of corruption, perception of corruption or other dimensions of sanctioning and the eventual voting decision could be an interesting angle to the study of co-partisan sanctioning, and would add knowledge to the main question asked in this study too. Another limitation of the outcome measurement chosen in the experiment was the fully hypothetical nature of the question which could increase the gap between survey responding and real-life behavior.

3.7.2 Manipulation Check

A factual manipulation check was asked from the subjects to ensure their compliance with the treatment: The receipt of both components of the elite communication, partisan cues and elite denials were tested. First, I asked subjects what the partisan affiliation of the

[1]Originally, the study included a two-step measurement of outcomes where before the main outcome - vote likelihood - the potential mediators of the effects were also measured such as corruption perception and the rating of the mayor on different traits such as accomplishment, experience, competence (See the Survey Questionnaire in Appendix I - Survey Questionnaire). These mediators were supposed to represent different theories about why voters could limit their sanctioning of corruption such as the so-called Implicit Exchange Theory (Rundquist et al., 1977; Muñoz et al., 2016). A future research could deploy a mediation analysis of the main effects based on the measured mediators.

featured mayor was (if any), then I asked who was cited in the news piece (if anyone). This manipulation check carried lot of weight in the upcoming analysis when I am going to talk about the compliance issues in the experiment.

3.8 Sampling

The data collection for the survey experiment was conducted as a part of the third-party, the Hungarian Social Research Institute's (TÁRKI) quarterly omnibus survey, in a multi-party research project that has been running from late January, to early February 2020. TÁRKI used a TAPI/CAPI technique for the interviewing of the participants. The survey vendor randomly recruited the subjects based on a representative sampling frame of the Hungarian voter-aged population. 1.018 respondents filled the survey. The attrition as a consequence of the selection of only partisan voters will be addressed in the description of the data preceding the analysis of the results.

3.9 Hypotheses Testing

Now that the experimental design has been introduced with the attempt to turn the central concepts of the study into a measurable quantities of interest, and with a measurement strategy in hand which assumedly detects the potential treatment effects, I can address the exact way the hypotheses will be tested by the deployed design.

Hypothesis 1 assumed a general sanctioning effect by the subjects as a result of the corruption news introduced in the vignettes. At this stage, the magnitude of the sanctioning is not differentiated, neither between the blocs (Cue/No Cue), nor within them (Partisan Cue groups / Rebuttal groups). The two blocs are observed separately with using their corresponding baseline – no sanctioning situation – control groups, therefore possible moderator effects of partisanship are not causing accidental confusion in detecting

the presence of general sanctioning effects. In both blocs, both treatment groups are expected to report lower mean vote likelihoods compared to their corresponding control groups. For Hypothesis 2, a between the blocs difference will be tested, namely the moderator effect of partisanship on the extent of sanctioning resulted from the corruption news exposure. This is effectively a difference-in-differences testing method comparing the change of the means in vote likelihood caused by moving from the control groups to the news exposure group in each blocs.

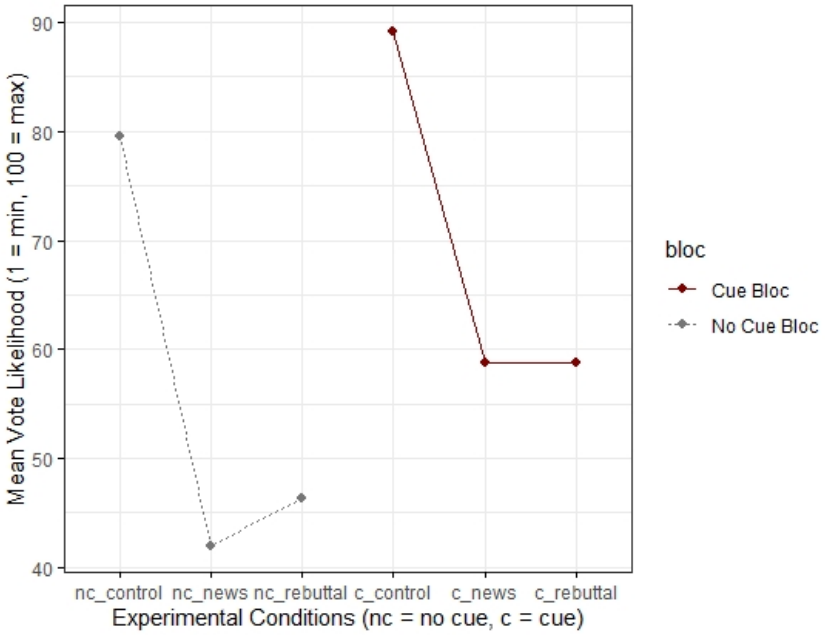

Figure 3.1: Hypothesized Effects - Expected Experimental Group Means

Hypothesis 3 observes a within bloc difference, namely, whether in the no cue bloc the persuasive efforts in the rebuttal group decrease the sheer effect of the damaging corruption news. Mean differences in terms of the main outcome will be compared between the corruption news exposure group and the rebuttal group. Hypothesis 4, the main hypothesis for the research question tackled by the study, will be again a within bloc test of mean differences between the vote likelihoods in the partisan news exposure group and the partisan elite rebuttal group in the cue bloc. This statistical comparison will allow us to make judgements on the theoretical claims raised by the thesis earlier, on how voters respond to partisanship in their sanctioning behaviour, through adjusting loyalties or by persuasion.

3.9.1 Inference Criteria

Hypotheses 1-3 are directional hypotheses; the test of significance is based on the one-tailed test with a critical p-value of .05. If the p-value is exceeded, we have no sufficient evidence against the null hypotheses. Hypothesis 4 is a null-effect hypothesis, the test of significance is based on a one-tailed test with a critical p-value of .05. If the $p < .05$, we have no sufficient evidence that the conditions fail to differ, therefore the confirmation of the null effect lacks evidence. The multiple comparisons of experimental groups will be accounted for by the Bonferroni-correction to adjust for the chance of statistically rare events accelerated by the testing of multiple hypotheses on the same data. The alpha level (.05) will be divided by the number of hypotheses (4) to gain a new alpha level for statistical significance of the findings ($.05/4 = .0125$).

3.10 Analysis Plan

The main effects in the experiment were planned to be tested through an Ordinary-Least Squared regression (OLS) model. The main outcome, the mean vote likelihood is on the left-hand side of the equation, while on the right-hand side I placed the regressor, the experimental group indicator. The effects were analysed in separate regression models corresponding to the experimental blocs, allowing us to use the different baselines of the studied effects. The results will be the Sample Average Treatment Effect, which is an Intent-to-Treat estimand of the effects which bases the comparison of the outcomes on the treatment assignment.

$$P(vote) = \beta^0 + \beta^1 experimentalgroupindicator + \varepsilon$$

Changes has been made in the analysis plan compared to the pre-analysis stage (see Appendix A - Data Documentation and Supplementary Materials) as a result of the exploratory stage of the research after the data have been examined. The experiment was heavily affected by non-compliance. A separate section will be devoted to the discussion of its sources and its consequences for the inferences of the experiment. In the analysis plan, I document the changes in the analytical strategy by which the issues caused by the non-compliance are handled. As non-compliance makes the ITT estimate of the effects unreliable, a Two-Staged Least Squares Regression (2SLS) model will be used to adjust for the potential biases in the estimated effects. This utilizes the compliance information provided by the manipulation check of the experiment. Compliance was examined through the successful transmission of the co-partisan identity of the mayor in the vignettes. It enables us to study the Compliers' Average Causal Effect (CACE) where treatment effects are adjusted to the actual receipt of the treatment compared to the assignment of the treatment.

$$P(received) = \beta^0 + \beta^1 assigned + \varepsilon$$

$$P(vote) = \beta^0 + \beta^1 received + \varepsilon$$

Chapter 4

Analysis

In the analysis, I discuss the results of the introduced survey experiment. I begin with the description of the obtained sample and the patterns of missing data. Later, I turn to the main results from the Ordinary Least Squared (OLS) regression. Subsequently, I address the internal validity issues in the main effect estimation arising from the non-compliance with the experimental treatments. Finally, I adjust the analysis to the observed non-compliance by conducting a Two-Stages Least Squared (2SLS) regression. I conclude the analysis part by discussing the strength and general solidity of the presented evidence.

4.1 Data Description

During the data collection stage, 1.018 subjects took part in the experiment. To arrive to the final sample with the study's units of analysis, co-partisan voters, 18,5% of the subjects were excluded from the experiment based on the pre-treatment question on the respondents' partisanship: They were not assigned to the treatment group and did not answer the outcome questions[1]. Therefore, the experimental data consisted of the responses

[1]Non-partisan voters were not assigned to non-partisan treatment groups for the considerations of the independence of the randomization procedure (assignment is not conditional on any other covariates) and

of 830 partisan voters. Treatment assignment was executed based on the pre-treatment measurement: Both Fidesz and Opposition voters received co-partisan or non-partisan vignettes corresponding to their treatment group assignment. No participants received out-partisan manipulation. Opposition voters were not divided into partisan groups because of considerations on the ecological validity of the manipulation[2]. Experimental group sizes are reported in the Appendix[3]. To assess the effectiveness of the randomization procedure the balance statistics are described in the Appendix[4]. In summary, the randomization is considered adequate based on the chosen criterion. Experimental groups are comparable in terms of the studied effects as the potential outcome assumption seem to be fulfilled based on the equal composition of the observed covariates across the experimental groups.

With regards to the missing outcomes, the survey did not record the the main outcome in the case of 48 respondents. Therefore, the statistical analysis was performed on 792 subjects. In terms of the attrition patterns, the process through which missing data is generated can have important consequences for the inferences. It is crucial to find out which respondent groups' outcomes we cannot observe because it could affect our ability to generalize our findings to the whole population of interest (because some segments – based on a covariate – of the sample experienced systematic attrition in the survey), or even could influence the validity of the causal estimates of the manipulated effects if the attrition is systematically related to one or few of the experimental groups.

the integrity of the exchangeability assumption of the experimental procedure (i.e. the treatment groups on baseline have identical potential outcomes as a consequence of the equal group composition in terms of respondent characteristics). These criteria or assumptions are crucial for the causal interpretability of the experimental results.

[2]In the current party politics context in Hungary, opposition parties nominated mayoral candidates in an all-opposition alliance in the last municipal elections in 2019. Even though, it improved the external validity of the study, it also led to some losses on the treatment compliance side – as it will be addressed later.

[3]Appendix B - Experimental Group Sizes and Sample Characteristics

[4]Appendix C - Balance Statistics

I assess the missing outcome patterns in a binomial logistic regression model[5]. Following Rubin's (1976; 2001) categorization of the missing data patterns, I conclude from the test that there is no evidence for a MAR pattern[6], but there is some evidence for a MNAR[7] pattern in the post-attrition data. There is a treatment group (Partisan Control Group) in the case of which the missing data on the main outcome seems to be systematic based on the logistic regression based estimates. However, the analysis proceeds with the assumption that this attrition is too minor to influence the study's ability to derive causal inferences.

4.2 Results

The analysis proceeds with the reporting of the main effects from the OLS regression that compared the results in the experimental groups in terms of the main outcome. The acquired estimate is the Sample Average Treatment Effects (SATE) which is produced based on the so-called Intent-to-Treat (ITT) estimation of the effects. It means that subjects were compared based on their treatment assignment status. Ideally, this gives us a valid estimation of the treatment effects for the sample, but it has to meet the criterion that treatment assignment equals or closely corresponds to the actual receipt of the treatment, in other words, a compliance with the treatment status.

Figure 4.2 below is the graphical illustration of the main effects represented by the mean outcome values in each treatment group. It is compared with Figure 3.1 from Section 3.9. where I illustrated the hypothesized effects. Table 4.1 provides the output from the performed OLS regression: Model 1. reports the regression coefficients from the partisan cue bloc where the reference level was the partisan control group. Model 2. reports the regression coefficients from the no partisan cue bloc where the reference

[5]Appendix D - Missing Outcome Test
[6]Missing at Random: systematic attrition based on a covariate
[7]Missing Not at Random: systematic attrition based on the group assignment

level was the non-partisan control group. In the following, I describe these results in a statistical and substantive terms, and later I assess the study's hypotheses in the light of these results.

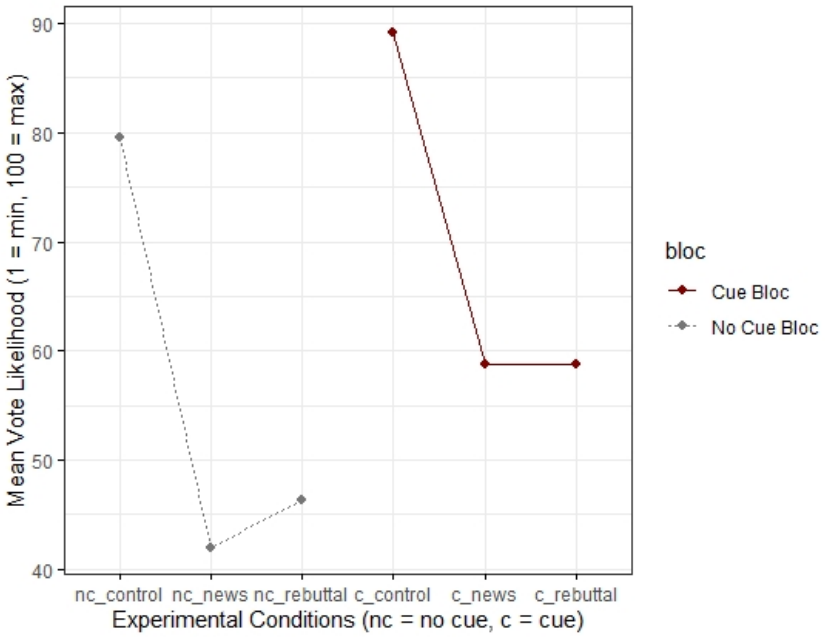

Figure 4.1: Hypothesized Effects - Expected Experimental Group Means (1)

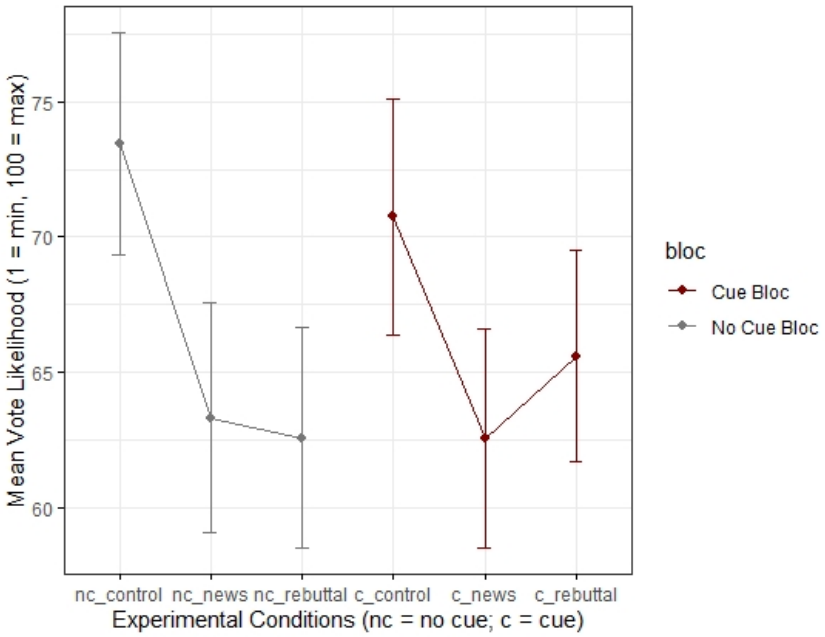

Figure 4.2: Main Effects - Experimental Group Means

Table 4.1: Main Effects OLS Regression

	DV: Vote Likelihood for the Mayor	
	(1) Cue Bloc	(2) No Cue Bloc
	β/SE	β/SE
Corruption News Exposure	-8.172*** (2.912)	-10.112*** (3.021)
Elite Rebuttal	-5.144* (2.992)	-10.855*** (3.016)
Constant	70.744*** (2.133)	73.445*** (2.214)
Observations	386	396
R^2	0.02	0.04

Note: DV Scale 1 = Min, 100 = Max *p<0.1; **p<0.05; ***p<0.01

In the cue bloc (Model 1), compared to the reference level – the partisan control group – corruption news exposure significantly decreased the respondents' reported likelihood to vote for the mayor. This equals to a minus 8.17 percentage points decrease which is statistically significant on the selected p-level of .05 ($p < 0.01$). The effect stays significant even after adjusting for the potential bias of multiple comparisons where the selected p-level is reduced to .0125. Still in the same bloc, as a result of the additional elite rebuttal against the corruption accusation, we still encounter a sanctioning effect (minus 5.14 percentage points), however, this decrease is not significant neither on the original nor on the adjusted significance level ($p < 0.1$). In the no cue bloc (Model 2), the effect of the corruption news exposure is minus 10.11 percentage points decrease of the subjects' reported vote likelihood for the mayor compared to the reference level – the control group without partisan cues -, and this decrease is significant on the adjusted p-level ($p < 0.001$). After the addition of the elite rebuttal to the corruption news exposure the figures stay effectively unchanged: Elite rebuttal in the no partisan cue bloc still leads to a decrease of minus 10.86 percentage points in the respondents' reported vote likelihood for the mayor, which is again a significant change compared to the reference level ($p < 0.001$).

What do these point estimates mean in terms of the hypotheses of the study? With regards to the general sanctioning effect (Hypothesis 1.) - disregarding the separate effects of the elite communication components for the moment - we can conclude that there is a substantial and significant decrease as a result of the damaging corruption news exposure in the treatment groups compared to the control groups. Respondents in general sanctioned the politician whose involvement in a corruption case they learnt about, even in the cases where the co-partisan link has been established. However, this conclusion is not completely consistent with the co-partisan elite rebuttal group's result (minus 5.14, $p < 0.1$). Even though, the effects are relatively large in terms of their magnitude, in our data we do not have enough evidence against the null hypothesis of no sanctioning effect. Nevertheless, this is assumed to be strongly related to the statistical power of the study - the relatively modest sample size. As a result, Hypothesis 1. about the general sanctioning effects of corruption is both maintained in the cue and no cue blocs' treatment groups.

Still on the level of the general effects of co-partisanship on sanctioning, Hypothesis 2 expected a strong, positive moderating effect of co-partisanship in subjects' sanctioning behavior provoked by the corruption news exposure. We can evaluate the size of the sanctioning effects by comparing the negative effects' magnitude as a consequence of the corruption exposure in cue and the no cue blocs. In order to obtain this difference-in-differences estimate (DID), I ran an additional regression[8] where I created two binary variables: One is for the treatment level (either control or news exposure), and the other for the availability of the co-partisan cue (either cue or no cue bloc). The interaction term retrieved the difference of the estimates from the original OLS regressions' two models corresponding to the two blocs[9]. However, this difference in the change effects (from exposing the respondents to the corruption news) is not statistically significant ($p = 0.64$). Even though, the results confirm the general direction of the moderating effect of

[8] Appendix E - Difference-in-Differences Estimations for Hypothesis 2

[9] 10.11 - 8.17 = 1.94 percentage points

co-partisanship on the sanctioning, the substantive effect is much smaller than expected. However, as at this level we are not comparing the effect of different elite communication components yet, in order to obtain a full picture - by testing the composite effects of elite messages - I also performed the same DID estimation[10] for the elite rebuttal groups compared to the control groups. The interaction term here also retrieved the differences between the estimates of the two blocs from the main OLS regression[11]. When we look at the composite effect of elite messages (both cues and rebuttals), we have a stronger substantive evidence for the positive moderating effects of co-partisanship on sanctioning, however, our data does not provide a strong enough evidence to reject the no moderating effect null hypothesis of co-partisanship on sanctioning in statistical terms. This, again, can be contributed to the low statistical power of the experiment[12].

Turning to the within-bloc differences - the effects of the different elite communication components - as expected, in the case of Hypothesis 3, elite persuasion in the no cue bloc did not decrease sanctioning effects. Compared to the negative effect of sheer news exposure group on the vote likelihood, the negative effect of elite rebuttals without partisan cues were effectively equal (-10.11 and -10.86 percentage points). On the other hand, we got a contrasting evidence from the OLS regression when it comes to main assumption, Hypothesis 4: In the comparison of the elite communication components in the co-partisan environment of sanctioning (cue bloc), the observed effects provide some substantive evidence against the study's assumption: It seems persuasion attenuated sanctioning effects by almost 3 percentage points compared to the partisan cues alone. According to the results, in the context of co-partisan corruption sanctioning, elite persuasion seems to elicit higher moderating effects on the extent of sanctioning as a result of the corruption than co-partisan cues alone.

[10] Appendix E - Difference-in-Differences Estimations for Hypothesis 2

[11] 10.86 - 5.14 = 5.72 percentage points

[12] See a supplementary analysis on the moderating effect in Appendix H - Non-Experimental Test of Co-Partisan Moderation of Sanctioning

In sum, in terms of the baseline hypotheses of the study (Hypothesis 1 and 2) about the sanctioning effects in general and the moderating effect of co-partisanship, the results from the experiment seems to maintain these assumptions, however, because of the low statistical power of the study we may lack a strong corroborating evidence. In terms of the studied effects, the differences between the elite communication contexts where the effect of the simple party cues and the addition of the elite persuasion is compared, the results seem to refute the main assumption of the study. In the context of corruption sanctioning, co-partisans may sanction less when they are provided with a persuasive account than when they are only shown co-partisan cues. However, before turning back to the theories of elite communication and co-partisan sanctioning, and before developing explanations for the observed, differential effects, I have to raise the question whether this evidence from the OLS regression is sufficient for the claim that co-partisan voters listen to persuasion beyond the loyalty calls when they sanction. As we will see, due to the considerable size of the subjects' non-compliance with the receipt of the co-partisan link, we cannot be sure whether these effects are the results of the assigned treatments or just a consequence of the varying levels of compliance between treatment groups. As such, these results are difficult to be considered a strong evidence and to be translated into theoretical claims.

4.3 Treatment Compliance

The experiment was heavily affected by a non-compliance with the treatment assignment. For the experiment, the crucial compliance check was the receipt of the co-partisan or non-partisan link to the mayor featured in the vignettes. It was measured by the manipulation check revealing subjects' knowledge about the partisanship of the mayor with options of a co-partisan mayor, no affiliation mayor and an out-party mayor. Table 4.2 summarizes the compliance rates in the study in terms of the percentage of subjects who answered the manipulation check question correctly in each experimental group. It also provides a breakdown based on the partisanship of the respondents as the compliance rates showed

a great deal of heterogeneity based on this covariate, and they arguably expose different patterns of non-compliance.

Compliance / Groups %	Total	Fidesz	Opposition
Cue Bloc			
Control Group	66.2	87.7	44.7
News Exposure Group	66.5	70.7	62.3
Elite Rebuttal Group	76.6	87.6	65.6
No Cue Bloc			
Control Group	41.85	46.2	37.5
News Exposure Group	38.5	44	33
Elite Rebuttal Group	45.2	43.8	46.6

Table 4.2: Compliance Rates

Generally speaking, there are two major issues with the non-compliance patterns: First, there is general non-compliance at the experimental blocs' level: Many cue-receivers did not correctly report on the co-partisan identity of the mayor, whilst a large portion of no cue receivers reported receiving partisan cues. Second, there is an other layer of the non-compliance issue: Compliance rates vary by treatment groups within the experimental blocs[13].

These non-compliance rates are worrying because first, our ITT-based estimates may not reveal the true effect of cues, because the reported receipt of the cue was not perfectly in match with the cue assignment status in the treatment blocs. In other words, we may compare blocs where there is mix of subjects who reported receiving or not receiving partisan cues[14]. This could underestimate the moderating effect elicited by the co-partisanship: First, a potentially lower co-partisan effect in the cue bloc than the expectation woul be under full compliance, because of the outcome responses of those who reported no co-partisan link to the mayor. Second, a potentially lower sanctioning effect in the no cue bloc where a considerable amount of the subjects reported a co-partisan

[13]Most visibly, in the case of the cue-receiver Fidesz voters.
[14]Most notably, Opposition voters in the cue bloc failed to report the receipt of the co-partisan cue.

cue, and therefore could sanction less in the voting scenario than it would be expected if no one at all would have reported the receipt of the co-partisan cue. In sum, it is possible that these two trends brought closer the mean outcomes between the two experimental blocs.

Concerning the other layer of the potential biases in the estimates caused by the non-compliance, with regards to the main studied effects between the partisan cues and elite persuasion, the variance of compliance rates between the focal treatment groups (Hypothesis 4) may confound the biases arising from non-compliance with the actual treatment effects. The notably lower compliance of Fidesz voters in the case of the news exposure group in the cue bloc compared to the elite rebuttal group could lead us to fall for an appearance of assumed treatment effect which is in reality could be the matter of treatment validity only.

The discussion of the potential sources of this extraordinary non-compliance is left to next chapter of the thesis, where I address my theories for the patterns of non-compliance in the experiment. However, for the purpose of the analysis of the effects, in this section I will stick to the derivation of the results. The revealed non-compliance forces us to question the validity of the previous, ITT-based estimation: The mere knowledge of the treatment assignment is not a correct indicator of the actual treated status of the subjects in the corresponding experimental groups.

4.4 Alternative Analysis

I choose a strategy in the analysis with the help of which I can adjust the estimates to the observed non-compliance. This is an Instrumental Variable(IV) approach to experimental non-compliance (Greenland, 2000). The IV-based approach to non-compliance utilizes the opportunity offered by the procedurally independent assignment (randomized, independent of any unobserved or observed covariates) of the treatment or intervention to select an exogenous, instrumental variable. In the study's case it is the assignment of

the co-partisan cue. The core of the compliance problem is that the endogenous independent variable, the co-partisan/non-partisan identification of the mayor, is clearly not in a one-to-one correspondence with the treatment assignment, therefore we assume it is also influenced by other observed or unobserved variables that are potential confounders of the observed effects. To treat this problem, we can combine our knowledge on both the assignment and the manipulation check to isolate the 'true', causally interesting effects of the treatment.

With the help of a two-step regression (2SLS[15]), I can adjust the estimates to reveal the Compliers' Average Causal Effect (CACE) to improve the potentially biased ITT-based estimations. In technical terms, the procedure looks like the following: First, I regress the compliance variable (the receipt of the treatment from the manipulation check) on the treatment assignment variable. In the second stage, I use the fitted values from the first regression to regress on the main outcome, the vote likelihood. Therefore, I acquire an estimate where the predicted effects are adjusted to the treatment compliance.

As I work with an instrumental variable (cue or no cue assignment), I need to modify the logic of the comparison of the treatment groups in order to be able to compare both status of the instrument. Instead of comparing treatment groups within the same bloc, vertically (cue or no cue bloc), in the IV analysis I compare the same treatment groups across the experimental blocs, horizontally, to extract the binary IV. I slice up the sample to three parts to create the pair-wise comparisons of the identical treatment groups (except for the IV), and I will perform a 2SLS regression on each sub-sample. Substantively, I will compare the effects of the co-partisanship (instrument) in the different elite communication environments, instead of measuring the effect of the different elite communication environments in co-partisan/non-partisan contexts like in the original OLS models. This will not allow me to make a direct comparison within the blocs, therefore a direct test of the main hypothesis - the effects of simple partisan cues and partisan elite

[15]For an experimental application, see Gerber and Green (2012, p.145-48.)

persuasion on the sanctioning - however, I will be able to compare the effect differences between each IV regression on the different treatment levels: Between the control groups, the news exposure groups and the elite rebuttal groups with and without the co-partisan cues. To translate it into the hypotheses, for example, I will be able to see the moderating effect of the cues by comparing whether the initial differences between the baseline groups grow when I compare the news exposure groups (Hypothesis 2). Similarly, I can assess Hypothesis 4 by comparing the distances between the news exposure groups and the elite rebuttal groups. If distances stay constant in each case, we have a rough evidence to refute the assumptions. If distances change, we may find a corroborating evidence. The three IV regression are reported in Table 4.3.

Table 4.3: Instrumental Variable Regressions

	Vote Likelihood for the Mayor		
	(1) Control	(2) News	(3) Rebuttal
	β/SE	β/SE	β/SE
IV: Co-Partisan Mayor	-4.442(11.779)	-1.705(10.567)	8.754(8.040)
Constant	82.665*** (8.229)	68.532*** (6.421)	61.843*** (5.699)
R^2	0.06	0.053	0.098
Observations	222	251	226
Note:			*p<0.1; **p<0.05; ***p<0.01

Even though the previous description offered a viable strategy to adjust the estimation of the effects to non-compliance, the interpretation of the results from Table 4.3 would not be methodologically sound. Unfortunately, the low compliance rate and the further sub-setting of the sample produced huge standard errors, therefore the estimation is very noisy and there is a big uncertainty around the point estimates.

In conclusion, it seems that the extraordinary precedence of non-compliance in the experiment hinders us from making any conclusive statements about the studied effects. Non-compliance is not very common in survey experiments in general, nevertheless, in

this study's instance it played a huge role. Is it exclusively attributable to the flaws in the experimental design, or the manipulation of partisan biases in electoral sanctioning is a topic that is prone to respondent biases? As I am not able to offer a complete theoretical argumentation and theory development based on the findings, the discussion chapter will address the factors that could have proven to be the obstacles in deriving a clear evidence from the studied effects, hoping that it will provide a methodological and in some way a theoretical merit for the future research on the topic.

Chapter 5

Discussion

What can be learnt from the analysis of the effects? I argue that despite of the lack of clear evidence in terms of the studied effects, the experiment nonetheless offered important lessons and clues for future research on corruption sanctioning and partisan elite communication. In particular, the very factor that hindered the derivation of an evidence – non-compliance - is the aspect that stimulates the further discussion. It equally contributes to the methodological discussion on survey experiments that are manipulating political biases and to the theoretical topic of the research, corruption sanctioning and elite communication.

5.1 Sources of Non-Compliance

In retrospect, a number of theories can be brought for the reasons why the experiment could have elicited respondent biases. I discuss these theories to explain the assumed sources non-compliance in the study. In short, I believe the experiment suffered heavily from weak cues, expressive responding (Berinsky, 2018), a Bayesian update of voters in form of Information Equivalency (IE) (Dafoe, Zhang, & Caughey, 2018). These are partly attributable to the design choices made in the experiment, whilst in the other part are

the product of the specific experimental setting in which corruption and partisanship is examined. After all, I conclude that the caveats that I encountered in the study are a good illustration of the often-overlooked aspects on non-compliance in survey experiments which are generally treated as immune to such problems. I argue that the manipulation of partisan biases is an example of a heavily affected area by potential non-compliance embodied by the above-mentioned forms. Beyond the methodological challenges, I argue that the patterns of non-compliance extend our knowledge about how voters react to partisan and non-partisan communication in sanctioning situations. In the discussion of the previous aspects, I will make a repeated reference to the compliance rates table in Section 4.3. and the detailed, answer-level breakdown of the manipulation check question in the Appendix[1]. I will conclude the discussion of non-compliance by offering an outlook for the scientific value of investigating thoroughly treatment compliance in survey experiments.

5.1.1 Weak Cues

In general, we can observe a much lower compliance rate for Opposition voters than Fidesz voters when they are in a co-partisan treatment environment. I assume that a major source of this trend is the arguably weaker partisan cues the Opposition voters received. In contrast with Fidesz voters, Opposition voters did not get vignettes in which the partisanship of the mayor was fitted to their exact, party-level preference. Instead they read about an 'Opposition backed mayor'. This design choice was made for two considerations, both related to the ecological validity of the experiment. First, in the current party politics context, and in the 2019 local elections, Opposition parties nominated common-list candidates. Second, not every Opposition party had countryside, small-town mayors in the past. However, these considerations on the ecological validity could have proved to be a major trade-off for the treatment validity. It seems that the correct partisanship identification of the mayor was very low, especially in the control group where the

[1]Appendix F - Detailed Manipulation Check Results

party cues were not re-enforced several times. Compliance rates improve with the longer vignettes that showed the party cues repeatedly. The weak cues can be closely tied to other potential respondent biases discussed later, Information Equivalency and a 'filling effect', leading respondents to incorrectly update and identify the partisan affiliation of the mayor.

5.1.2 Expressive Responding

In turn, I suggest that experimental groups in the cue bloc did not suffer from weak cues in the case of the Fidesz voters. This is shown by the generally higher compliance rate, and that it was not modified – increased - by the re-enforced party cues in the extended vignette groups where subjects received the corruption news and the elite rebuttal. However, there is a visible and substantial drop in the compliance in the news exposure group. On the other hand, it recovers to the initial levels of compliance in the elite rebuttal group. As weak cues are not sufficient explanations here, what can account for the variance of compliance rates between treatment groups?

I argue instead that this variance demonstrates an instance of expressive responding. Expressive responding, in short, means that respondents in surveys report their knowledge of facts or opinion on something that is not based on their true perceptions or genuine beliefs, but that serves to support the integrity of their identities. For example, Berinsky (2018) investigated whether political rumours work by actually making people believe misinformation or it is just a channel to express their identities. In this experiment, when voters were exposed to co-partisan corruption, this identity-based motivator could bias their responses from there onward. The validity of this claim could be detected by the compliance rates reported by the manipulation check. There is no non-bias-based explanation why the compliance rates differed between the treatment groups that deployed the same number of cues.

As per my assumption, in the case of this experiment, Fidesz voters who were ex-

posed to co-partisan corruption could simply fail to acknowledge a fact (co-partisanship) that was in conflict with their identities. The fact that compliance rates drop when the damaging information about corruption is introduced in the news exposure group is a support for this assumption. Interestingly, when I also included the elite rebuttal against the corruption accusation, the compliance rates recovered to beyond the baseline level. It seems that arguably the co-partisan elite response lifted the burden of admitting co-partisan identities. In terms of the inferences, the problem is that even if we treat the recovery effect as a true treatment effect – respondents who received a rebuttal were more confident and comforted to acknowledge the co-partisan link - we have no reliable measure how in real-life people who have been only exposed to the partisan corruption would have behaved. For the same reasons as failing to acknowledge the co-partisan identity of the mayor, they could have acted like they are sanctioning the politician in the outcome question, however, in real-life, when they are not observed - as they have been in the survey – a simple indication of a partisan cue could work to the same extent as the elite argument on attenuating sanctioning.

This has very important consequences for the main effects studied in this research, as we seem to lack the tools for a real-life comparison of the effects of simple partisan cues and elite arguments on the co-partisan sanctioning of corruption. Partisan cues could elicit a potential side-effect – expressive responding - that makes it difficult to study its consequences for sanctioning in a survey environment. However, in terms of our main query, the denial of facts in the survey could still nonetheless result in a loyalty reconciliation in the voting booths. Thereby, the differences in the outcomes reported in the main effects OLS between the news exposure groups and the rebuttals in the partisan cue bloc (Table 4.1) could equally reveal a real-life effect – that co-partisan voters demand elite persuasion in the circumstances of sanctioning to follow the party line -, or simply a survey anomaly – that co-partisans who only receive cues hide their true behaviour when they are observed.

5.1.3 Information Equivalency (IE) and the Filling Effect

The third kind of bias that is brought as an explanation for the assumed varieties of non-compliance in the experiment can be both connected to weak cues and no cues. Therefore, I use this theory to explain the non-compliance both for the Opposition voters who received weak cues in the cue bloc, and all respondents in the no cue bloc who were left without partisan heuristics with regards to the mayor, a gap for which they arguably used IE.

IE, in short, suggest that a special kind of validity threat in survey experiments is that the assignment of the treatment could not only induce a variation in the manipulated variables, but also in some observed or unobserved co-variates which also influence the main outcome (Dafoe et al., 2018). Therefore, using the concept of Judea Pearl (2009), the treatment assignment could also affect the outcome through a non-causal, back-door path that makes the treatment effect estimates completely or partially confounded. Dafoe and his colleagues (2018) use the famous experiment by Tomz and Week (2013) on democratic peace to demonstrate such effects. A treatment that manipulates the variable if a country is democracy or non-democracy could also induce respondents to update other attributes of the country that is not fixed by the manipulation like geographic location or religion of the population.

In terms of this study, there are several types of 'Bayesian updates' that could have taken place and showed itself in the form of non-compliance. In the no cue bloc, the mayor's partisanship was omitted – it was not directly mentioned that he is independent. Therefore, it left respondents guessing the partisanship which is a crucial heuristic in voting decisions. A 'filling' effect could have taken place through which voters updated the partisanship of the mayor. In the no cue bloc, both Fidesz and Opposition voters identified the mayor as he is from Fidesz at a high rate[2]. This update could be connected

[2]See Appendix F - Detailed Manipulation Check Results for the option-level breakdown of the manipulation check response rates.

to the fact that in the Hungarian context, the countryside politics is dominated by the governing party. Moreover, in small settlements Fidesz is especially strong. Another potential type of Bayesian update could be connected to a possible pre-treatment bias in the case of Hungarian voters, especially Opposition voters who consume opposition media dominantly. Corruption is widely tied to Fidesz in the elite and media discourse. Exposing voters to corruption could have resulted in an update about the partisanship of the mayor. However, this assumption about the corruption-partisanship update is somewhat weakened by the fact that when we look into the option-level breakdown of the manipulation check question[3], opposition voters for example – in the cue and no cue bloc as well – seem not to report at higher percentage that the mayor is from Fidesz in treatment groups where corruption news is introduced compared to the baseline, control groups.

The patterns of potential IE are even more concerning in the cue bloc where I fixed for the partisanship by assigning the co-partisan cue, therefore theoretically prevented uncontrolled update of the partisanship information like in the no cue bloc where the mayor had no affiliation. While it seemed to work for Fidesz voters (except for the variance caused by the assumed expressive responding), Opposition voters seemed to update partisanship despite of the fixed partisanship, the introduced cues. This is argued to be the result of the weak cues I mentioned earlier.

The assumed filling effect in the case of the treatment groups with the weak cues and a lack of cues obscures the differences between sanctioning effects in the co-partisan and non-partisan blocs. Opposition voters who failed to receive the correct cues could sanction more than in a real co-partisan setting. Whilst Fidesz voters – who identified the mayor as having a Fidesz affiliation at a high rate even though no cues were available – could sanction less. Therefore, in terms of the general effects, the two experimental blocs may not be comparable along the presumption that these are different on the main

[3]Appendix F - Detailed Manipulation Check Results

variable, co-partisan or non-partisan context of sanctioning.

To summarize the discussion, the experiment dealt with a mixture of methodological challenges. A study specific flaw in the design setup was the usage of weak cues for Opposition respondents. Another design choice, contrasting co-partisan cues with no cues could have catalysed a specific voter behavioural dynamic – IE – which is a valuable finding for the theory, but proved to be an obstacle to interpret the studied effects. Lastly, the experiment could also expose a survey methodological challenge that is not specific to any design choices in this study, but a more general, however often overlooked problem in surveys that study biases or attitudes – expressive responding. These challenges proved to be detrimental for the studied effects in the experiment, as we cannot confidently assume that the manipulation validly simulated real-life circumstances of corruption sanctioning in the co-partisan contexts, and therefore the shown effects mimic real life voting behavior in sanctioning as a consequence of co-partisanship in general, and the elite communication varieties in particular. However, the exposed vulnerability of the studied topic to non-compliance provides an important moral both on the methodological and theoretical front.

5.2 Outlook for the Method

The outlook this thesis takes based on the investigation into survey experiment related non-compliance is that the proof of compliance should be a much more on the front when one wants to offer an internal validity check of an experimental design. Treatment validity in survey experiments is too often taken granted because of the sometimes false belief about the researchers' full control over the manipulation process. The inclusion and the reporting on the substantive manipulation checks should be a mandatory element in the studies applying these methods. Beyond the methodological importance that makes sure our inferences are rooted on stable causal grounds, the investigation into compliance could also reveal further mechanism of the studied effects. Manipulation checks that are crafted skillfully could not only offer a benchmark on treatment validity, but could also

reveal the potentially more complex nature of the effects that the experiment studies. If the study fails a compliance test, we may also want to receive clues for the reasons why. It could equally help us to improve or correct these with new design choices - if possible, and acquire new, until then hidden aspects of the effect mechanisms that could have stayed under the researchers' radar because of the early assumption about the straightforward nature how the manipulated effects interact with the subjects. The investigation into treatment compliance could be a source of new scientific knowledge: Even a study contaminated by non-compliance could improve the status of knowledge on a given subject to a great extent if it manages to reveal more than the mere fact of the insufficient manipulation.

I believe that the example of the experiment presented previously prove these points very well. It revealed the complexity of the interactions between partisan voters and elite communication or partisan cues in particular. What seemed a quite secure and straightforward manipulation - embedding partisan cues and thereby manipulating the co-partisan biases was revealed to be more complex than that. Nonetheless, the manipulation check explored some initial details of these complexities that will hopefully help future research in taking new directions and control for hidden challenges in studying these effects. For example the non-compliance with the cue assignment in the experiment suggested that voters contradict partisan information consciously (expressive responding) and sub-consciously (IE). The earlier is a technical challenge in studying real-life voter behavior, the latter is a theoretical challenge of how partisanship is working in the broader context of information and decision-making.

Conclusion

Summary of the Findings

This thesis focused on the question whether voters use partisanship as loyalty call or as a channel for persuasion when they face the situation of co-partisan sanctioning. Using the heterogeneity of elite communication and messages, the study contrasted the limiting effect of simple partisan cues and partisan elite persuasion on the sanctioning of co-partisan corruption. The thesis assumed that persuasion will not elicit extra attenuating effects compared to simple partisan cues in a situation of corruption sanctioning. Thus, I implied voters are less persuaded or manipulated into limited or no sanctioning of co-partisan wrongdoings, and more consciously side with their loyalties. I argued it is because partisan voters make decisions in co-partisan sanctioning situations based on predefined, directional goals driven by their partisan identities, and not as a consequence of the information they learn, therefore, persuasion is unnecessary since it carries the same information value - the identity of the actor - as partisan cues does. I supposed that an evidence for the dominant effect of cues alone could endorse this assumption.

In normative terms, this was supposed to be used as an argument to challenge the electoral accountability theories' applicability or relevance in electoral sanctioning. On the other hand, a contrasting evidence for the necessity of elite arguments in the situation of sanctioning could have provided some ground for the existence of accountability by co-partisan voters, even though the quality of the elite persuasion can be another challenge

to democratic accountability. Nevertheless, treating it on the level of the action, the co-partisans' demand for arguments or justifications could have shown us that partisan biases in sanctioning are not completely dissolving electoral accountability and replace it with the sole question of in-group loyalties.

In a survey experimental study taking place in Hungary with the voting age partisan electorate, the thesis tested the above assumptions. Subjects' sanctioning behaviour was measured after exposing them to vignettes on a local case of political corruption varying partisan elite communications in the treatment groups. In an OLS regression analysis, I found that elite persuasion may matter. Compared to the simple cues, the elite rebuttal to corruption decreased sanctioning by 3 percentage points, even though this difference is not statistically significant, I believe that a statistically better powered study with a larger sample size could reveal significant differences. Therefore this evidence on the extra effect elicited by partisan elite persuasion on the co-partisan sanctioning refuted the main assumption of the thesis about the no additional effects of co-partisan elite persuasion. However, it still left the question open that in exactly what way persuasion matters: By actually changing partisan minds, or by only providing a justification for the predefined motivational goals of identity reconciliation.

Limitations

However, these results should be read by caution as the manipulation check revealed a serious non-compliance in the study. The cue assignment was insufficient in the experiment as reflected by the low compliance rates. Therefore, the simple ITT-based estimation may give us confounded effects rather than true causal effect about the studied treatments, especially because compliance varied between the treatment groups that manipulated the elite communication contexts (cues and arguments). The difference between the sanctioning behaviour of simple partisan cue-receivers and those who read partisan elite rebuttals could equally be a true treatment effect and a confounded effect of differ-

ent levels of compliance – the receipt of the co-partisan information. A new analytical strategy was deployed which could theoretically adjust for the potential biases caused by non-compliance. However, the Instrumental Variable approach implemented in the 2SLS regression was statistically not interpretable because of the noisy estimates with huge standard errors. The non-compliance that proved to be an obstacle in the estimation process is assumed to be a product of actual bad design choices (weak cues) and design choices that evoked various respondent biases, some with real theoretical implications for voter behaviour in sanctioning (filling effect for cues), whilst others seem more like general survey anomalies, an enduring problem of real-life and experimental circumstances in studying voters (expressive responding).

Improvements in the Design

What are the possible improvements that can help us to arrive at a design that allows us a valid investigation of the causal effects of co-partisanship on sanctioning in general, and the different theories of how this co-partisan bias is elicited in form of the comparison the partisan cues and elite persuasion? First of all, partisan cues must be much more carefully deployed: This experiment's difficulties with the transmission of the co-partisan or non-partisan cue treatment showed that the receipt of the cues are not as straightforward as it seemed first. For example, to avoid the filling effect or the automatic voter update on the partisan affiliation, co-partisan corruption treatments could be rather compared to out-partisan corruption treatments. In that case, the comparison groups will be less likely to experience false co-partisan updates than in the case of no affiliation groups that left respondents guessing the partisanship of the mayor.

Expressive responding is much trickier, and it is harder to adjust the design to tackle it, because it affects survey experimental technique in general, and it is not a problem specific to this design. One potential remedy for the problem is offered by Berinsky (2018) who uses list experiments to avoid expressive responding. In this study's case we could

use the list-design as a new, more subtle measurement strategy to detect the effects of co-partisanship and elite communication components on sanctioning than the currently used one where subjects have to rate their vote likelihood explicitly. Following the logic of list experiments, we could include a number of neutral statements and the statement for the outcome such as 'I would vote for the mayor'. We could detect treatment effects by comparing the mean accepted/rejected statements across different treatment groups. By this strategy, I believe we could reduce expressive responding, as this lifts the burden from co-partisan respondents to admit their potential endorsement of co-partisan corruption, and their true sanctioning behaviour would come in a more latent form[4].

Outlook for the Topic

In conclusion, even though the thesis could not offer a clear answer for the studied question about what is behind the co-partisan biases in the sanctioning, loyalty or persuasion, I believe the challenges encountered in the form of experimental non-compliance serve as an important methodological lesson for the future designs on the topic and the survey experimental technique in general. With regards to the original research question, I believe the novel conceptualization of the ways partisan biases come into effect in electoral sanctioning, the operationalization of those as components in elite communication, and the corresponding research design that was introduced laid a fertile ground for new research on the topic.

Without doubts, the potential practical and normative implications of how voters interact with partisan information and communication are consequential for both the field of voter behavior and democratic theories. Moreover, the increasing partisan attitudes at the level of the electorate and the elite as well make the question even more relevant. There is a growing chance that partisanship-based position-taking is not only influencing the

[4] A problem that this strategy would leave us with a raw, binary measure of sanctioning (either selecting the statement or not) in contrast with the 1-5 rates which allowed for gradual responses.

partisan public's policy views, but also their opinion and responses - such as sanctioning - to normative issues like the conduct in office. That is why the questions such as the one posed in this study matters. As I see the current matters in the democratic debates, there is a doubt whether any line exist between voters' partisan responses to different issues, with different salience for democratic accountability. In summary, it would be crucial to find out whether electoral sanctioning exist in its partisan, persuasive form, or it is diminished under the dominance of partisan position-taking. Furthermore, even if the necessity of partisan persuasion is shown, the question still remains if it serves as a reactive, justificatory tool for preexisting partisan motivations, or it has a pro-active role in forming the opinions of the partisans.

Future Research

Besides re-adressing the original research question, future research could look into how voters substitute for the lack of partisan cues or handle weak cues. Non-compliance in the no cue bloc suggested that voters look for partisan cues even in the absence of it. But how voters do it in special political contexts such as local elections or wide electoral alliances where the clarity of such cues is questionable. Furthermore, sub-group analyses based on respondents' partisanship would be also interesting for the future research of partisan biases and sanctioning, assuming a heterogeneity of the effects[5]: How the pre-treatment status of a party in terms of its previous, pre-treatment involvement in political corruption influences the co-partisan biases in corruption sanctioning? Is it a valid assumption that long-term exposure to corruption make the party's voters more resistant to sanctioning? Third, in the experiment I did not vary the underlying factors of the elite communication and the sanctioning situation. We have a strong reason to believe that the variation of

[5]The respondents' partisanship is a strong explanatory variable for sanctioning effects. Although these results are also potentially biased by non-compliance – especially because of the lower compliance rates in the case of the Opposition voters – See Appendix G - Treatment Effect Heterogeneity by Respondent Partisanship

the sources of corruption accusation or the variation of the elite persuasion types could lead to different results in the sanctioning behavior of the partisan voters.

Appendix

Appendix A - Data Documentation and Supplementary Materials

Data Documentation and Supplementary Materials

Pre-Analysis Plan - Made online before the analysis of the data through the *Centre of Open Science (OSF)* Study Registry public depository. URL: `https://osf.io/q4d9f`

Data File – Open depository URL for the .sav file containing the raw data from the data collection by TÁRKI:

`https://drive.google.com/drive/folders/1SQX5GhcbtEGMwipRHwJUGHgdnH69PdBY?usp=sharing`

Code book for the the .sav data file: URL:

`https://drive.google.com/drive/folders/1SQX5GhcbtEGMwipRHwJUGHgdnH69PdBY?usp=sharing`

Coding Script of the Statistical Analysis in *R Studio* - Open depository URL for the data file:

`https://drive.google.com/drive/folders/1SQX5GhcbtEGMwipRHwJUGHgdnH69PdBY?usp=sharing`

Appendix B - Experimental Group Sizes and Sample Characteristics

I report the experimental group sizes and the sample characteristics on a selected number of variables taken from the experimental sample. These covariates are the partisanship of the respondents, gender, settlement size, education and age. Partisanship was coded as a binary, in line with the manipulated variable co-partisanship (Fidesz/Opposition). Gender is again a binary, coded by the interviewer (Male/Female). Settlement size has been recoded from a continuous variable to a categorical one with the following categories: Settlements with less than 10.000 inhabitants, with less than 100.000 inhabitants, and with more than 100.000 inhabitants. Education is again a categorical variable with categories of lower than secondary education, secondary education or vocational training, and higher education. Finally, age was recoded to categories of between 18-25, 25-40, 40-65 and over 65.

	Cue Bloc	No Cue Bloc
	n=414 , 50%	n= 416, 50%
Control Group	n=134, 32%	n=125, 30%
News Exposure Group	n= 149, 36%	n=147, 35%
Elite Rebuttal Group	n= 131, 32%	n=144, 35%

Table B1: Experimental Groups Sizes

Variables	TÁRKI Sample (18+, Hungarian partisan voters)
Gender: Male	364 (43.9%)
Partisanship: Opposition	357 (43%)
Age: 18-25	35 (4.2%)
Age: 25-40	189 (22.8%)
Age: 40-65	423 (51%)
Age: 65+	183 (22%)
Education: Higher	114 (13.7%)
Education: Secondary/Vocational	308 (37.1%)
Education: Lower than Secondary	408 (49.2%)
Settlement Size: less than 10.000	342 (41.2%)
Settlement Size: less than 100.000	241 (29%)
Settlement Size: more than 100.000	247 (29.8%)
n	830 (100%)

Table B2: Sample Characteristics

Appendix C - Balance Statistics

In the assessment of the balance in the experimental groups in terms of the reported covariates, I offer two approaches: A formal test statistics with a Multinomial Logistic Regression model, and a descriptive approach with the calculation of the Mean Standardized differences of the covariate categories between the experimental groups.

The M-logit model reported in Table C1 below summarizes the results from the assessment of a potential covariate imbalance between the experimental groups after the randomization of the subjects into the treatment groups. This compares groups to each other. Therefore, the regression performs the calculation of the beta coefficients and the p-value statistics with a dependent variable of the group assignment and the exploratory variables are the observed covariates in the experiment. For the purpose of randomization test, I only report the p-value test statistics. On the selected 95% confidence level, I can only report a significant imbalance in the case of the settlement size lower than 100.000.

Variables	Non-Partisan News Exposure	Non-Partisan Elite Rebuttal	Partisan Control	Partisan News Exposure	Partisan Elite Rebuttal
Opposition	0.23	0.68	0.71	0.86	0.86
Male	0.55	0.29	0.12	0.09*	0.38
No Secondary	0.20	0.43	0.71	0.53	0.18
Secondary/Vocational	0.28	0.82	0.97	0.78	0.76
Age 25-40	0.63	0.64	0.92	0.33	0.67
Age 40-65	0.62	0.25	0.79	0.76	0.44
Age 65-	0.30	0.20	0.23	0.89	0.39
Lower than 100.000	0.09*	0.09*	0.04**	0.21	0.09*
More than 100.000	0.95	0.22	0.26	0.46	0.33

Table C1: Mlogit Regression Model P-Values. **p<.1; **p<.05; ***p<.001*

However, randomization test statistics are not always endorsed by methodologists in the discipline (Greifer, 2020). Therefore, I also calculated the absolute Mean Standardized Differences between the groups in terms of all the categories of the covariates with a threshold of difference of .01 as recommended by Stuart and colleagues (2013). Each treatment group in the cue and no cue blocs were summarized in terms of their

covariate balance compared to the whole sample (n=830). Figure C1. below shows the summary plot. In sum, no substantial imbalance was produced as a result of the treatment assignment based on the established threshold.

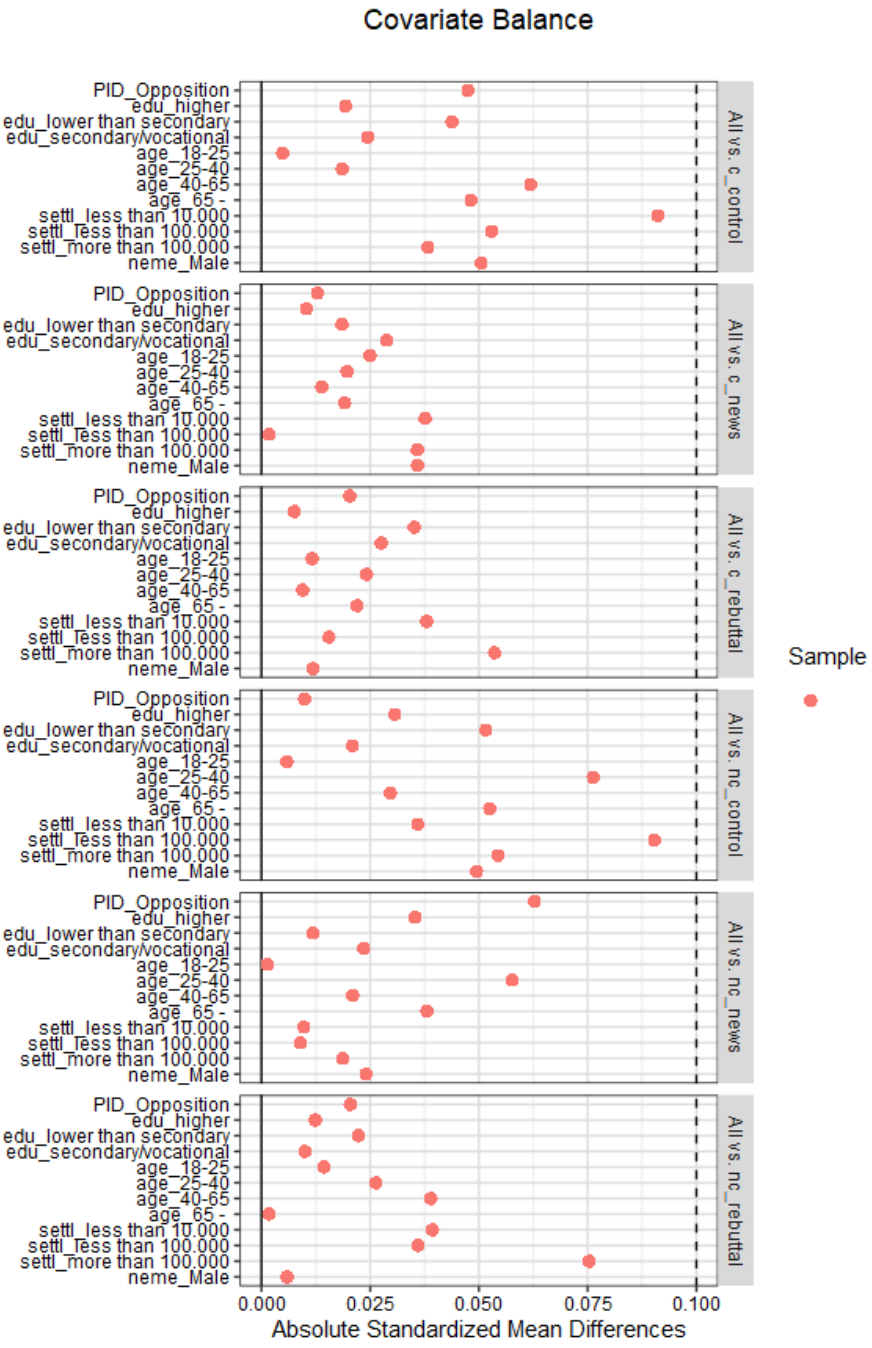

Figure C1: Covariate Balance Plot

Appendix D - Missing Outcome Test

I conducted a binomial logistic regression where I predicted the missing data on the vote likelihood (binary: missing or not missing) by the experimental groups and the observed covariates to assess potential MAR and MNAR patterns (Rubin, 1976; Little & Rubin, 2001). Table D1. reports the results. There is an evidence for a non-random attrition that is related to the group assignment: Respondents in the cue control group were 3-6% more likely to miss responses on the main outcome compared to the other groups, and this difference is significant when it is compared to the no cue elite rebuttal group.

Table D1.: Missing Outcome on Vote Likelihood

	Vote Likelihood NA
	β/SE
Experimental Conditions:	
Cue News	0.035(0.028)
Cue Rebuttal	0.048* (0.029)
No Cue Control	0.040(0.029)
No Cue News	0.036(0.028)
No Cue Rebuttal	0.064** (0.028)
Covariates:	
Male	0.022(0.017)
Age 25-40	-0.038(0.043)
Age 40-65	-0.039(0.041)
Age 65 -	-0.043(0.043)
Settlement less than 100.000	-0.035* (0.020)
Settlement more than 100.000	0.023(0.020)
Opposition	0.001(0.017)
Education less than secondary	-0.030(0.026)
Education: secondary/vocational	-0.035(0.026)
Constant	0.964*** (0.050)
Observations	830
Note:	*p<0.1; **p<0.05; ***p<0.01

In order to better assess the potential changes in the treatment groups compositions as a result of the attrition from the study, I offer a repeated description of group balances, but now in the post-attrition stage (In Appendix C, I showed the pre-attrition stage balance to assess the effects of randomization.). Figure D1. displays the covariate balance

in the post-attrition sample using the absolute Mean Standardized Differences with a difference threshold of .01 between each treatment groups and the whole post-attrition sample (n=792). I conclude that attrition did not significantly change group balances on the reported covariates. The sole exception is the cue bloc control group - consistent with the results in the logit regression (Table D1) - where respondents from the settlements with less than 10.000 inhabitants showed greater attrition than in other groups.

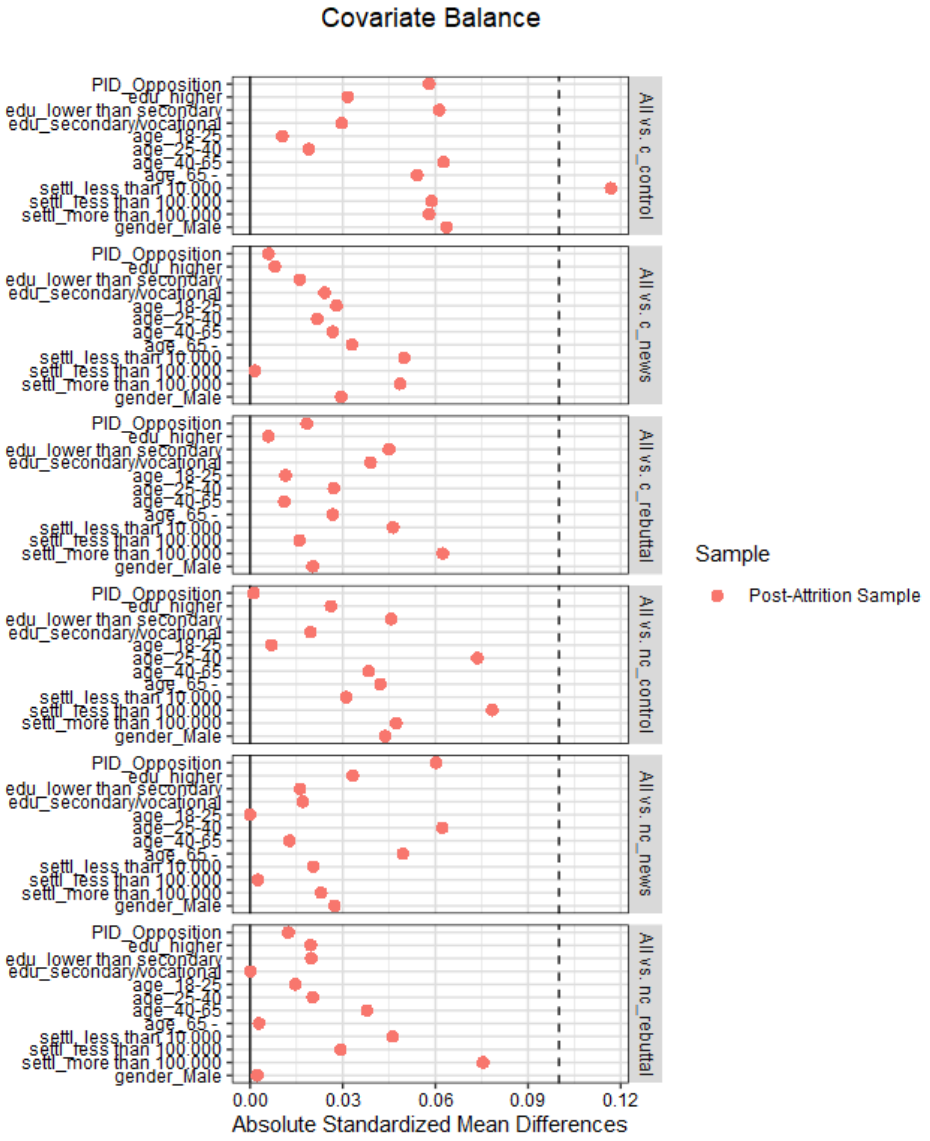

Figure D1.: Post-Attrition Covariate Balance Plot

Appendix E - Difference-in-Differences Estimations for Hypothesis 2

In order to obtain the difference-in-differences estimates for Hypothesis 2 about the moderating effects of co-partisanship on the sanctioning of corruption, I ran additional regression models where I created two binary variables: One is for the treatment level (either control and news exposure levels or control and rebuttal levels). The other binary variable for the availability of the partisan cue (either cue or no cue bloc). The interaction terms between the binary variables in the case of both the control group vs. news exposure group comparisons and the control group vs. elite rebuttal group comparisons retrieved the effect differences resulted from the corruption exposure in the respective non-partisan cue and co-partisan cue blocs from the main OLS Regression Model (Table 4.1). Neither Model 1 and Model 2 interactions are statistically significant (p=0.64 and p=0.17, respectively), however, in the case of the control vs. rebuttal group comparison the moderating effect of co-partisanship on the sanctioning effects resulting from the corruption exposure seem to be substantive compared to the non-partisan group: When exposed to corruption, subjects who received a co-partisan cue were 5.7 percentage point more likely to vote for the mayor than those who did not receive cues. The statistical insignificance of the effect may be attributed to the small sample size.

Table E1: OLS Change Effects

	Vote Likelihood for the Mayor	
	(1) Control vs. News	(2) Control vs. Rebuttal
	β/SE	β/SE
Corruption Exposure	-10.112*** (3.012)	-10.855*** (2.915)
Cue Assignment	-2.702(3.109)	-2.702(3.014)
Corruption Exposure:Cue Assignment	1.940(4.244)	5.712(4.167)
Constant	73.445*** (2.207)	73.445*** (2.140)
R^2	0.036	0.033
Observations	518	504
Note:		*p<0.1; **p<0.05; ***p<0.01

Appendix F - Detailed Manipulation Check Results

Beyond the low compliance rates reported in Table 4.2, the different patterns of non-compliance are further explained by the detailed manipulation check question results (Table F1 and F2) where all the answers options and their respective percentages by all responses are reported by the partisanship of the respondents. These results expose the IE and filling effect assumptions introduced in the Discussion (Chapter 5).

In the original compliance rate table, we have seen that in the cue bloc, the correct identification of the co-partisan mayor was heavily lop-sided: Opposition voters were far more likely to non-comply than Fidesz voters. As an explanation, in the Discussion, I mentioned that the weak cues could not neutralize IE effects for Opposition voters. The figures show that the relatively low co-partisan identification in the cue conditions is paralleled by a relatively high share of identification pointing at the opposing party (Fidesz). Many Opposition voters believed – even with access to co-partisan cues – that the mayor is from Fidesz. For Opposition voters the opposing party identification is around 21% in the cue conditions, whilst the same for the Fidesz voters is only around 8%. In terms of the assumed filling effects in the no cue bloc, we can see from the responses that Fidesz voters tend to project their own partisanship to the mayor with unknown partisan affiliation at a remarkably higher rate (on avg. 23% higher) than Opposition voters. In contrast, Opposition voters thought at a high rate that the unaffiliated mayor is a Fidesz mayor (on avg. 34%). In comparison, Fidesz voters identify the unaffiliated mayor as Opposition only on avg. 9% through the no cue conditions.

In sum, filling effects in the treatment groups heavily tilted to incorrectly identify the mayor as having a Fidesz affiliation. Fidesz voters in the no cue condition projected their own partisanship to the mayor, while Opposition voters both in the cue and no cue conditions projected Fidesz as an opposing partisanship to the mayor in the vignettes. These non-compliance patterns are very likely that influenced the sanctioning differences between the no cue and co-partisan cue bloc detected by the analysis.

Response Option %	Control	News Exposure	Elite Rebuttal
Cue Bloc			
Fidesz Mayor	79	65	85
Opposition Mayor	9	7	10
No Affiliation	3	19	3
NA	10	8	3
No Cue Bloc			
Fidesz Mayor	44	39	42
Opposition Mayor	6	13	8
No Affiliation	43	40	39
NA	7	9	11

Table F1.: Manipulation Check Results - Fidesz Voters

Response Option %	Control	News Exposure	Elite Rebuttal
Cue Bloc			
Fidesz Mayor	23	20	20
Opposition Mayor	42	56	61
No Affiliation	28	14	12
NA	6	11	7
No Cue Bloc			
Fidesz Mayor	33	39	29
Opposition Mayor	27	22	19
No Affiliation	36	30	41
NA	4	9	12

Table F2.: Manipulation Check Results - Opposition Voters

Appendix G - Treatment Effect Heterogeneity by Respondent Partisanship

I extended the the original OLS regression models performed in the cue and no cue blocs with a control variable for the Partisanship of the respondents. Being an Opposition voter meant a significant and negative difference in terms of the main outcome, vote likelihood for the mayor. However, these results seem to exist already on the baseline, and Opposition voters do not sanction more or less than Fidesz voters as a result of the news exposure or the elite rebuttal neither in the co-partisan cue bloc nor the no cue bloc as revealed by the insignificant interaction terms. Furthermore, the higher non-compliance in the co-partisan cue bloc and the opposing party filling effect in the no cue bloc (as discussed in the Discussion part) could raise a question whether the baseline differences are valid at all between Opposition and Fidesz voters, or the effect is only a result of non-compliance bias.

Table G1.: OLS Treatment x Respondent's PartyID

	Vote Likelihood for the Mayor	
	(1) Cue Bloc	(2) No Cue Bloc
	β/SE	β/SE
News Exposure	-10.837*** (3.900)	-11.853*** (3.745)
Elite Rebuttal	-8.724** (4.022)	-12.687*** (3.805)
Opposition ID	-13.691*** (4.180)	-16.667*** (4.286)
News:Opposition ID	4.485(5.729)	2.049(5.918)
Rebuttal:Opposition ID	6.781(5.879)	3.938(5.849)
Constant	77.419*** (2.919)	80.588*** (2.806)
R^2	0.067	0.124
Observations	386	396
Note:		*p<0.1; **p<0.05; ***p<0.01

Appendix H - Non-Experimental Test of Co-Partisan Moderation of Sanctioning

In the main analysis, the results somewhat under-performed the expected size of the moderating effect of co-partisanship on the sanctioning of the corrupt mayor in the vignettes. I concluded that this may be largely attributable to the non-compliance with the partisan cue assignments: The small differences between the sanctioning behavior of the no partisan cue receiver subjects and those who received co-partisan cues may be explained by the fact that while cue receivers often reported wrong cues (mainly the Opposition voters), no cue receivers at high rate reported receiving co-partisan cues (mainly Fidesz voters). Therefore, the results could not present the comparison of two internally coherent groups along the manipulated variable, non-partisan or co-partisan environment of sanctioning. In this supplementary analysis, I show that despite of the limits of the experimental results, we can actually find an evidence for a strong moderating effect of co-partisanship in the electoral sanctioning, however, this test is non-experimental and not causally interpretable - as I put aside the information about the randomized factor, the treatment assignment which allowed the causal measurement, and instead I take a test on those subjects who regardless of their treatment assignment status reported the co-partisan link of the mayor in the manipulation check. This approach is non-causal, because we have no knowledge on what background variables lead to the co-partisan identification of the mayor besides or instead of the manipulation itself.

In this descriptive test, I use the corruption perception mediator measured in the outcome section of the survey (1-100, where 1 is the lowest perceived corruption and 100 is the highest), and interact with the reported co-partisan identification of the mayor - with no respect for the treatment groups (binary variable, 0 - no co-partisan mayor, 1 - co-partisan mayor). Finally, I regress the variables and their interaction term on the main outcome vote likelihood (1-100, 1 - least likely, 100 - most likely). The results (Table H1) of the interaction prove the moderator role of co-partisanship on sanctioning: By every

unit (percentage point) increase in the perceived corruption, subjects who identified the mayor as co-partisan were 0.15 percentage points more likely to vote for the mayor than subjects who did not make a co-partisan link to the mayor ($p < 0.05$). In other words, the more serious is the corruption sanctioning situation, the more important is the role of co-partisanship as a moderator of sanctioning effects. The interaction plot in Figure H1 also shows that the moderator starts to elicit its effect on sanctioning around the 40-60 unit of perceived corruption which in the original scale of responses (1-5) meant a moderate, but perceived corruption, whilst lower scores corresponded to rather no corruption perception by the subjects.

Table H1: Interaction Effects of Co-Partisan ID x Corruption Perception on Vote Likelihood

	Vote Likelihood for the Mayor
	β/SE
Coruption Perception	-0.512*** (0.042)
Co-Partisan Mayor	3.146(3.882)
Corruption Perception:Co-Partisan Mayor	0.145** (0.056)
Constant	91.566*** (3.042)
Observations	699
R^2	0.336
Note:	*p<0.1; **p<0.05; ***p<0.01

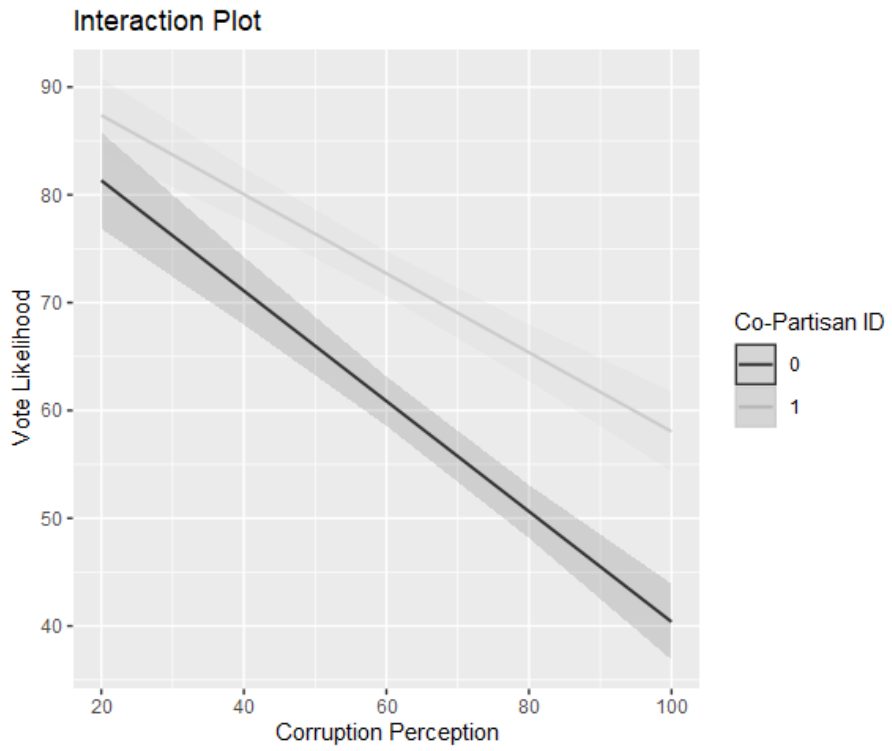

Figure H1: Interaction Plot: Co-Partisan ID x Corruption Perception

Appendix I - Survey Questionnaire

Final version submitted to TÁRKI (2020.01.14.) (Translated to English language)

CEU Survey – Bloc III.:
Corruption Sanctioning and Elite Communication (Bence Hamrák)

PRE-TREATMENT MEASURE
FROM EVERYONE!

1. Do you consider yourself rather a government-leaning or opposition voter?

1. Government-leaning
2. Opposition
3. Neither
4. I don't know
5. I don't want to answer

ASK IF Q1=3, 4 OR 5

2. If you would have to select one option, which political side do you feel yourself and/or you views closer?

1. Government-leaning
2. Opposition
3. Neither
4. I don't know
5. I don't want to answer

3. If the general election would be held tomorrow, which party's list would you cast your ballot for?

1. Fidesz-KDNP
2. MSZP – Magyar Szocialista Párt
3. Jobbik
4. LMP – Lehet Más a Politika
5. DK – Demokratikus Koalíció

6. Momentum
7. Párbeszéd
8. Other: ……..
9. I would not vote
10. I don't know
11. I don't want to answer

- If Q3 is answered, but Q1 or Q2 is not (>=3), should be assigned to Gov. or Opp. voter based on Q3 answer (if 1-7: 1 = Gov., 2-7 = Opp.)
- ONLY use Q3 as the assignment reference point, if Q1 OR Q2 is not available (>=3)
- If neither Q1 (>=3), Q2 (>=3), nor Q3 (>=7) can be used for the assignment (no party preference): RESPONDENT IS ELIMINATED FROM THE EXPERIMENT – THE REST OF THE SURVEY QUESTIONS

VIGNETTES
"We collected some information about the mayor of a small Hungarian settlement for you, to give you an overview:"

For Interviewer: Turn the questionnaire to the respondent and read the vignettes! [Each respondent was randomly assigned to one of the six types of the vignettes by the survey software. Vignettes' wording was adjusted to the pre-treatment partisanship answer in the corresponding treatment conditions]

I. Control Group without Partisan Cue:

BIOGRAPHY:
Németh László is the mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the politician.

II. Corruption News Exposure Group without Partisan Cue:

BIOGRAPHY:
Németh László is the mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of

the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the politician.

"Now, we would like to share with you the latest news article about the mayor:"

Trans-Tisza Herald
Nyírbánság's mayor, Németh László has manipulated several tenders to favour his economic circle

A judicial case has been initiated against Németh László the mayor of Nyírbánság, after he has been accused with the manipulation of several public procurements. According to the alleged evidence, the mayor influenced the results of the procurements for the new sport facility in Nyírbánság. He assumedly received bribe money in return. The bribe could be connected to a businessman close to the mayor.

III. Control Group with Partisan Cues: cues based on the pre-treatment answer of the respondent

BIOGRAPHY:

Németh László is the (*partisanship*) mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the (*partisanship*) politician.

IV. Corruption News Exposure Group with Partisan Cues: cues based on the pre-treatment answer of the respondent

BIOGRAPHY:

Németh László is the (*partisanship*) mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the (*partisanship*) politician.

"Now, we would like to share with you the latest news article about the mayor:"

Trans-Tisza Herald
Nyírbánság's (*partisanship*) mayor, Németh László has manipulated several tenders to favour his economic circle

A judicial case has been initiated against Németh László the (*partisanship*) mayor of Nyírbánság, after he has been accused with the manipulation of several public procurements. According to the alleged evidence, the mayor influenced the results of the procurements for the new sport facility in Nyírbánság. He assumedly received bribe money in return. The bribe could be connected to a businessman close to the (*partisanship*) mayor.

V. Elite Rebuttal Group without Partisan Cues:

BIOGRAPHY:

Németh László is the mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the politician.

"Now, we would like to share with you the latest news article about the mayor:"

Trans-Tisza Herald
Nyírbánság's mayor, Németh László has manipulated several tenders to favour his economic circle

A judicial case has been initiated against Németh László the mayor of Nyírbánság, after he has been accused with the manipulation of several public procurements. According to the alleged evidence, the mayor influenced the results of the procurements for the new sport facility in Nyírbánság. He assumedly received bribe money in return. The bribe could be connected to a businessman close to the mayor. The mayor firmly rejected the accusations, because those – according to his statement - „are a result of an organized political witch-hunt without any substantial evidence". According to the mayor, his political opponents are „envy of his successes as a leader of the town", and "his accomplishments that helped the city and its citizens" (...)

VI. Elite Rebuttal with Partisan Cues: cues based on the pre-treatment answer of the respondent

BIOGRAPHY:

Németh László is the (*partisanship*) mayor of Nyírbánság since 2010. Originally, he started his career as an agricultural engineer. During his time as a mayor, he introduced several agricultural programs that aimed at increasing the yield of the local farmers and create additional income for the town. Regardless the dire economic situation of the municipality, as a result of the stimuli program, the local farmers output increased, as well as the incomes for the town, and new jobs were created. These successful terms as a mayor could led to the numerous re-election of the (*partisanship*) politician.

"Now, we would like to share with you the latest news article about the mayor:"

Trans-Tisza Herald
Nyírbánság's (*partisanship*) mayor, Németh László has manipulated several tenders to favour his economic circle

A judicial case has been initiated against Németh László the (*partisanship*) mayor of Nyírbánság, after he has been accused with the manipulation of several public procurements. According to the alleged evidence, the mayor influenced the results of the procurements for the new sport facility in Nyírbánság. He assumedly received bribe money in return. The bribe could be connected to a businessman close to the (*partisanship*) mayor. The (*partisanship*) mayor firmly rejected the accusations, because those – according to his statement - „are a result of an organized political witch-hunt without any substantial evidence". According to the mayor, his political opponents are „envy of his successes as a leader of the town", and "his accomplishments that helped the city and its citizens" (...)

OUTCOME MEASURE
FROM EVERYONE WHO READ THE VIGNETTES!
Please answer the following questions with regards to the mayor whom you read about previously:

4. Please rate the mayor on the following aspects, from 1 to 5, where 1 means the least and 5 the maximum. How much do you think the mayor is . . .

- competent?
- accomplished?
- corrupt?
- experienced?

1 - 5; I don't know; I don't want to answer

5. Now please imagine that you are a resident of the settlement the mayor of which you have just read about. If municipal elections would be held tomorrow, how likely do you think you would cast your vote for the mayor?

1. I would definitely vote for him
2. It is possible, I would vote for him
3. I can't decide whether I would vote for him
4. I would rather not vote for him
5. I would categorically refuse to vote for him
6. IDK

7. X

'Finally we would like to ask you some questions with regards to the material you have just read:'
6. Could you remember what was the political affiliation of the mayor?

1. Fidesz politician
2. An opposition politician
3. No affiliation
4. IDK
5. X

7. Could you remember who was cited in the news article?

1. The mayor
2. A judge
3. A resident
4. No one was cited
5. IDK
6. X

OPEN QUESTION, SHORT ANSWER!
8. What is the first word or impression that comes to your mind with regards to the mayor?

.....

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