

When Partisan Loyalty and Performance Evaluations Conflict: a Study of Cross-Pressured Partisans in the US



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Thesis submitted in partial fulfilment of the requirements for the
degree of DPhil in Politics

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Abstract

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A key expectation of elections is that voters hold elected officials to account for their performance in office. A contradiction exists in the literature between those who believe voters are incapable of exercising accountability and those who believe voters are successfully able to do so. This thesis addresses this contradiction and presents four empirical case studies in which I test the conditions under which voters exercise accountability. In order to explore these conditions I focus on situations in which partisan voters in the US receive a performance signal that directly contradicts their partisan loyalty. I refer to these voters as cross-pressured partisans. In the thesis I examine who these voters are and how they behave at election time. I find that partisans are responsive to signals about the quality of economic management of the incumbent government and that candidate competence matters at the local level. I find that the electoral response to corruption is more mixed. Although some partisans punish their party in light of a corruption scandal, corruption also depresses turnout and a lab experiment shows that voters do not punish candidates who embezzle funds if the candidate shares their group identity. The thesis finds that three factors moderate the extent to which partisan voters hold their own party to account: (1) individual voter characteristics; (2) the electoral context and; (3) the type of government performance. First, partisan voters with strong levels of affiliation to a party are less likely to exercise accountability whereas those with moderate levels are most likely to do so. Second, if the partisan stakes of an election are high, democratic accountability is low. Third, although partisan voters exercise accountability in the domain of economic management and candidate competence, the electoral response to corruption is more mixed. The thesis shows that there are many partisans who are conflicted between their loyalty to their party and their evaluations of incumbent performance. Under certain conditions this conflict incites behavioural change at election time. Partisans are therefore not incapable of holding their preferred candidate or party to account for their performance in office, but neither should they always be expected to do so.

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Chapter 1

Introduction

Understanding the circumstances under which retrospective voting achieves effective democratic accountability and when it fails to do so is an important task for subsequent research

- Healy and Malhotra (2013, p.285) *Annual Review of Political Science* -

The retrospective vote theory is based on the premise that voters hold politicians to account for their performance in office.¹ The intuition underlying this theory of voting is simple: governments are punished for bad times and rewarded for good times. If voters use elections in this manner, then elections ensure the quality of democratic government via two ways. First, retrospective voting ensures that ‘bad apples’ are removed from office. Second, if retrospective voting occurs, then politicians are provided with a clear incentive to perform to a high standard whilst in office or risk losing re-election. Retrospective voting has been extensively documented and studied in the field of political science.² As a result, we have a sound understanding of the process. Retrospective voting requires several steps in which voters first need to receive information about past performance of the incumbent, voters then need to attribute responsibility to particular government officials for their performance and,

¹Key (1966); Fiorina (1981)

²See for early works on the topic Fiorina (1981); Key (1966); Fearon (1999) and for more recent formal models Besley (2007)

as a final step, voters need to use this information to inform their voting decision.³ This process is conditioned by the institutional context and is easier for voters when governing power is concentrated with a small or cohesive group of actors and lines of accountability are clear.⁴ If voters use information on past performance in this manner then the accountability mechanism functions properly and ensures the selection of a high-quality government.

Different opinions exist however as to how well this mechanism of retrospective voting works in practice. Although several branches of research confirm the functioning of the retrospective vote,⁵ other studies have shown that the mechanism often fails.⁶ It is important to understand whether the retrospective vote mechanism works or not because, together with ideological representation, it is one of the two pillars underlying most theories of democratic governance.⁷ Most importantly, because retrospective voting fosters democratic accountability, it ensures the quality of democratic governance. If retrospective voting fails, then politicians are not held to account for their performance in office and the quality of democratic governance decreases.

Partisan loyalty has been identified as one of the main obstacles to retrospective voting.⁸ In both the UK and the US context, many voters identify strongly with a

³See for a helpful overview of the retrospective vote literature Healy and Malhotra (2013) and for applications of the retrospective vote model see for instance Gomez and Wilson (2008); Marsh and Tilley (2009); Tilley and Hobolt (2011); Duch and Stevenson (2008); Becher and Donnelly (2013)

⁴Powell and Whitten (1993); Hobolt et al. (2013)

⁵Most notably the field of economic voting, Duch and Stevenson (2008); Lewis-Beck (2006); Lewis-Beck et al. (2008); Nadeau et al. (2012). In case of voters' responsiveness to natural disasters see Malhotra and Kuo (2008); Healy and Malhotra (2009, 2013); Bechtel and Hainmueller (2011)

⁶With regards to the economic vote Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013) for more general criticisms of voters' ability to act in a retrospective fashion see Bartels (1996); Bartels and Achen (2004); Achen and Bartels (2016); Healy and Lenz (2012); Taber and Lodge (2006); Gerber et al. (2010)

⁷The other pillar is ideological or mandate representation see Przeworski et al. (1999) and the discussion on models of voting behaviour in Chapter 2 of this thesis.

⁸Achen and Bartels (2016, 2013); Bartels and Achen (2004); Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013); Huber et al. (2012)

particular political party. These affiliations or loyalties have been shown to hamper the functioning of the retrospective vote mechanism. Partisan bias may distort any or all steps in the accountability process described above. Partisans are selective in the information they consume and show a tendency to read news reports that confirm their partisan views whilst they avoid watching or reading news stories that challenge their worldview.⁹ Partisan loyalty also affects the ways in which voters attribute responsibility to government actors. Partisans attribute responsibility of positive government outcomes to their preferred party for instance, but avoid the attribution of responsibility for negative outcomes.¹⁰ Finally, partisan preferences affect the way in which voters evaluate performance. Partisans provide more positive evaluations of the economy when their preferred party is in power, regardless of objective measures of economic prosperity.¹¹ Partisan loyalty thus clouds objective judgments and aligns performance evaluations with party preferences, jeopardizing the effective functioning of democratic accountability.

In this thesis I focus on this interaction between partisan loyalty and the retrospective evaluation of government performance and its implications for (the lack of) democratic accountability. Put simply, I explore under which conditions (if at all) partisans hold the party they identify with to account. To test this, I present a theory of cross-pressured voting in the US context. Throughout the thesis I focus on situations in which partisan loyalties are *challenged* by incumbent performance. I thus zoom in on situations in which voters are *conflicted* between their loyalty toward a party and their evaluation of the quality of the performance of this party. I refer to these voters as *cross-pressured partisans* and in this thesis I explore who these voters are and how they behave at election time in light of various types of cross-pressure.

⁹Taber and Lodge (2006); Lodge and Taber (2013)

¹⁰Tilley and Hobolt (2011)

¹¹Bartels (2002); Gerber and Huber (2010); Evans and Andersen (2006); Evans and Pickup (2010)

The research question I address is the following:

under what conditions (if any) do partisans hold their in-party accountable when faced with negative performance signals - and what form does this response take?

Here the in-party refers to the party that the voter identifies with, the out-party is the opposing party. Thus, for a voter who identifies as Democrat, the in-party is the Democratic party and the Republican party is the out-party. The reverse is true for a voter who identifies as Republican. Throughout the thesis I focus on cross-pressured partisans and I compare these with non cross-pressured partisans. Cross-pressured partisans are voters whose partisan identification is *contradicted* by the signal of incumbent performance. Non cross-pressured partisan are voters whose partisan identification and evaluations of incumbent performance *align*. Cross-pressured partisans are thus conflicted between their partisan identification and their evaluation of government performance, non cross-pressured partisans are not conflicted.

The thesis contributes to debates on retrospective voting behaviour, partisanship and democratic accountability. I advance on the existing literature in several ways. First, much of the literature on partisanship and retrospective voting focuses on the *bias* that partisan loyalty exerts on performance assessments and the consequent *alignment* of partisan identification and retrospective evaluations.¹² This thesis aims to offer a fresh approach and focuses on the *conflict* between these elements of vote choice instead. Second, existing studies have explored the behaviour of persuadable voters and ambivalent partisans and show that the partisan electorate is more volatile than is oftentimes thought.¹³ Yet, these studies do not focus specifically on the conflict

¹²See for instance Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013); Bartels (2000, 2002); Achen and Bartels (2016)

¹³Hillygus and Shields (2008); Lavine (2001); Lavine et al. (2012); Basinger and Lavine (2005)

between partisan loyalty and retrospective performance evaluations. Rather, these studies focus either on the conflict between party identity and issue positions,¹⁴ or on the conflict between party identity and positive and negative appraisals of both parties.¹⁵ What is more, the existing studies on conflicted partisans focus either on how political parties strategically target conflicted voters, or on the effects of partisan ambivalence on the way in which voters process information. In this thesis however I focus specifically on the behaviour of conflicted partisans at election time. To my knowledge there is no systematic study of the conflict between partisan identification and retrospective performance evaluations and its effects on voting behaviour and democratic accountability.

There is good reason to believe however that partisan-performance conflict is an important dynamic in many elections. Studies in the field of the economic vote have shown for instance that partisans at times vote against their preferred party in light of economic information and that partisans are able to overcome their biases in times of an economic recession.¹⁶ In this thesis I therefore extend the concept of cross-pressure to include the conflict between partisan loyalty and retrospective performance evaluations. In addition, I focus not only on economic performance but broaden my view to also include candidate competence and political corruption. I also present a more complete theory of cross-pressure that sets out both contextual and individual level factors that might affect the electoral response of partisans in light of cross-pressure.

¹⁴Hillygus and Shields (2008)

¹⁵Basinger and Lavine (2005); Lavine (2001); Lavine et al. (2012)

¹⁶Chzhen et al. (2014); Bermeo and Bartels (2014); Alvarez and Nagler (1995); Parker-Stephen (2013); Dickerson (2016)

1.1 Voter Responsiveness to the Quality of Incumbent Performance

The existing literature on retrospective voting provides two contradictory accounts as to the response of partisans to the quality of incumbent performance. On the one hand, a wide range of studies show that (partisan) voters are strongly biased when they evaluate the performance of incumbent governments and therefore ignore or misconstrue information about the quality of the government to avoid having to punish their preferred party, or reward their non-preferred party.¹⁷ This has led some authors to conclude that the system of democratic accountability is broken beyond repair.¹⁸ On the other hand, studies that explore the electorate's response to the incumbent government's management of the economy or voters' responses to natural disasters find that voters are capable to attribute blame and reward to government officials in line with objective facts.¹⁹ A contradiction thus exists.

1.1.1 Responsive Voters: The Economic Vote and Natural Disasters

The retrospective vote paradigm finds its most clear application in the field of the economic vote. This literature illustrates the fact that governments are removed from office in times of economic downturn and are re-elected in times of economic growth.²⁰ This literature follows the 'rational voter' paradigm and finds that voters act in a rational, self-interested manner in response to economic reality. Studies on the recent economic recession confirm this pattern: both governments on the left- and right-end

¹⁷Taber and Lodge (2006); Lodge and Taber (2013); Gerber et al. (2010)

¹⁸Achen and Bartels (2016)

¹⁹For evidence on voters' responsiveness to economic management see Duch and Stevenson (2008); Lewis-Beck (2006); Lewis-Beck et al. (2008); Nadeau et al. (2012) For studies on natural disasters see Malhotra and Kuo (2008); Healy and Malhotra (2009, 2013); Bechtel and Hainmueller (2011)

²⁰Powell and Whitten (1993); Duch and Stevenson (2008); Lewis-Beck (2006); Lewis-Beck et al. (2008); Nadeau et al. (2012)

of the ideological spectrum were removed from office in these economically difficult times.²¹ Empirical evidence thus exists to confirm the fact that voters punish and reward incumbent governments for their economic stewardship.

A second body of research that explores the electorate’s responsiveness to the performance of the incumbent government focuses on the topic of natural disasters. Similar to the economic vote literature, these studies find that the electorate as a whole holds governments accountable for their actions in office and is responsive to information about the quality of the incumbent.²² To give an example; a study on Germany finds that voters rewarded the incumbent SPD government for their disaster relief efforts in the wake of the flooding of the Elbe a few weeks prior to the country’s general elections in 2002.²³ Another study on the US finds that citizens initially responded in a biased manner to the government’s relief efforts in the wake of Hurricane Katrina, with Democratic voters attributing higher levels of blame for the event to President Bush compared to Republican voters. Yet, once citizens were informed of actual responsibilities of various government actors, the effects of party bias disappeared. The authors conclude from this that: ‘citizens are not absolute party loyalists, blind to relevant information’.²⁴ Similar evidence has been presented in relation to citizens’ responses to government actions in light of tornadoes.²⁵ There are thus clear examples in which voters use information about the performance of the incumbent government

²¹Chzhen et al. (2014); Bermeo and Bartels (2014); Dickerson (2016)

²²Bechtel and Hainmueller (2011); Malhotra and Kuo (2008); Healy and Malhotra (2010)

²³Bechtel and Hainmueller (2011)

²⁴Malhotra and Kuo (2008, p.131)

²⁵Healy and Malhotra (2010) find that voters do not blame the government for tornado-related deaths, but only for tornado-induced economic costs. To the authors, this suggests that voters are able to distinguish between events that are within a politician’s control and those that are not. Although note Healy et al. (2010) which suggests that voters are susceptible to mood and punish incumbents when their football team has lost, which appears to contradict Healy and Malhotra (2010). Yet, in Healy et al. (2010, p.12807) the authors suggest that the negative side-effects of mood disappear when people are made aware of their own mood because people realize their own mood is unrelated to the performance of the incumbent. This again, in line with the study of Katrina, suggests that biases may be overcome when sufficient information is provided and results in a rather optimistic view on voters’ retrospective abilities by Healy and Malhotra (2010)

to inform their voting decision. In these cases, the democratic accountability mechanism performs as expected.

Criticisms have been leveled against this view however. With respect to the economic vote literature, research shows that economic evaluations are endogenous to partisan preferences and that consequently the effects of the economic vote have been over-estimated.²⁶ Put simply, economic evaluations are shaped by partisan preferences and hence do not exert an independent effect on vote choice. In addition, we might wonder whether studies on the economic recession or responsiveness to natural disasters are representative of the accountability mechanism in general. Recessions, hurricanes and tornadoes are high-profile events in which the stakes for individual voters are high. It is often easy for voters to attribute responsibility in response to natural disasters because the incumbent government will aim to benefit electorally from its disaster response.²⁷ In the majority of elections however, voters will not be responding to government actions in light of floods, hurricanes, tornadoes, or even economic recessions, but to government policies in the domains of education, general economic management, healthcare or national security. It is likely much more difficult for voters to form opinions on these matters and the attribution of responsibility in cases like these is often complex and messy.²⁸

A second body of research thus argues that there are many ways in which voters do not live up to the normative ideal of the democratic voter and fail to hold governments to account for their actions in office. Concerns exist both about the general ability of voters to make retrospective judgments as well as about the negative effects

²⁶Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013)

²⁷See Bechtel and Hainmueller (2011) who show that the German government responded with an extraordinary amount of effort and money.

²⁸As evidenced by the literature on clarity of responsibility Powell and Whitten (1993); Hobolt et al. (2013); Tillman (2008)

of partisan loyalty on the accountability mechanism.

1.1.2 Unresponsive Voters: The Detrimental Effects of Partisan Loyalty

There are many ways in which voters do not live up to the normative ideal of the democratic voter. Most voters are less informed about political events than political scientists like to believe.²⁹ Many studies in political science furthermore find that voters (not just partisans) are sub-optimal decision-makers. For instance, when voters retrospectively evaluate the performance of an incumbent government, they tend to focus more on the most recent years rather than construct an average over all four years of government.³⁰ Also, most voters are swayed not just by facts, but are strongly influenced by emotions and are susceptible to rhetoric.³¹ Other research shows that voters hold politicians accountable for events that are entirely outside of their control, such as shark attacks or the loss of a football game.³² These findings confirm research in other disciplines such as behavioural economics and psychology, which have found that most people do not adhere to the principles of rational decision-making.³³

In theory, the retrospective vote paradigm should alleviate some of these concerns. Most importantly, retrospective voting should be easy for voters because they can use their own lived experience as a benchmark against which to evaluate the incumbent government. Voters need not be very informed about politics in order to judge whether or not they are better off now than they were four years ago.³⁴ The retrospective vote paradigm thus puts a much lower set of demands on voters and is

²⁹Bartels (1996)

³⁰Huber et al. (2012); Bartels and Achen (2004); Healy and Lenz (2012); Healy and Malhotra (2013)

³¹Neuman et al. (2007)

³²Achen and Bartels (2013); Healy and Malhotra (2010)

³³See for instance in the field of economics Kahneman (2003)

³⁴Fiorina (1981)

often seen as a low-cost, high-benefit, method for voters to make relatively accurate decisions about matters of politics.

One important criticism remains however: partisan loyalties can severely hamper the retrospective vote mechanism and, consequently, jeopardize the functioning of democratic accountability. Voters who feel an allegiance to a particular political party often evaluate government performance in a biased fashion. Mechanisms such as *motivated reasoning*³⁵, *selective attribution*³⁶ and *selective evaluation*³⁷ result in the unwarranted *alignment* of performance evaluations to fit a voter's partisan preferences. A well-documented example of this is the fact that partisans rate the economy much more positively when the president belongs to their favoured party, regardless of objective economic indicators.³⁸ Alternatively, voters may attribute positive government outcomes to their in-party, the party they identify with, but neglect to do so when the incumbent belongs to the other side.³⁹ Other studies have shown that partisans update their interpretation of facts such that they comply with their partisan loyalties.⁴⁰ These distorting effects of partisan loyalty tend to be stronger for voters with high levels of interest and knowledge of politics.⁴¹ Because of this, Achen and Bartels (2016) argue that: 'driving the automobile of the state simply by looking in the rearview mirror (...) works about as well in government as it would on the highway.'⁴²

From this, we must conclude that voters' - and particularly partisans' - ability to exercise accountability is limited at best, non-existent at worst. Yet, although these studies clearly show how voters might avoid exercising accountability, they do not in-

³⁵Taber and Lodge (2006)

³⁶Tilley and Hobolt (2011)

³⁷Gaines et al. (2007); Gerber et al. (2010)

³⁸Gerber et al. (2010); Bartels (2002)

³⁹Tilley and Hobolt (2011); Marsh and Tilley (2009)

⁴⁰Gaines et al. (2007)

⁴¹Taber and Lodge (2006); Gaines et al. (2007)

⁴²Achen and Bartels (2016, p.4)

form us as to how voters, at times, *overcome* partisan bias and punish their preferred party (or reward the opposition). However, both anecdotal and empirical evidence suggest that voters *do* at times punish their preferred party and overcome their partisan biases. Anecdotally, voters must vote against their preferred party at least occasionally, or otherwise electoral shifts would never occur. Empirically, a study on the electoral response to the 2008 financial crisis in Britain suggests that voters responded to the economic crisis regardless of their partisan loyalties.⁴³ Also in the wake of the 2008 financial crisis, both left- and right-wing incumbent governments across Europe were ousted from office.⁴⁴ Although the importance of partisan loyalty in shaping vote choice should therefore be acknowledged, it seems both unhelpful and unrealistic to assume that partisans *never* waiver in their loyalty.⁴⁵ What is more, this viewpoint does not provide any information as to the conditions under which the accountability mechanism works, or when it fails to do so.

1.2 When Does Democratic Accountability Occur?

The literature on the interaction between partisan loyalty and retrospective performance evaluations thus presents a contradiction. Should we conclude that voters are: ‘cognitively and motivationally incapable of fulfilling the requirements of rational behaviour in democracy’⁴⁶, or, are they: ‘well-equipped to handle the important task of appropriately holding incumbents responsible’?⁴⁷ In other words, are voters ‘myopic stooges’⁴⁸ or: ‘reasonably rational fellows’?⁴⁹ These two bodies of work ex-

⁴³Chzhen et al. (2014) For evidence in the US context of the fact that negative economic signals incite an electoral response see Dickerson (2016); Parker-Stephen (2013)

⁴⁴Bermeo and Bartels (2014)

⁴⁵See also Hillygus and Shields (2008) and Lavine et al. (2012) who offer empirical evidence to support the statement that partisans *do* waiver in their loyalty. I discuss this in more detail in Chapter 2, section 2.5.

⁴⁶Taber and Lodge (2006, p.767)

⁴⁷Malhotra and Kuo (2008, p.133)

⁴⁸Achen and Bartels as characterised by Malhotra and Kuo (2008)

⁴⁹Key (1966)

ist in parallel, but represent fundamentally different perspectives on voters' abilities and behaviour and have strongly divergent implications for our understanding of the workings (or lack thereof) of the retrospective vote theory and the consequent (lack of) democratic accountability.

I do not believe that the truth lies somewhere in the middle. Rather I believe the truth lies at the extremes. I assume that there are times when voters will gravitate to one extreme and act according to the principles of the retrospective vote paradigm. At other times, I expect that voters find themselves on the other end of the spectrum and are unable to assess information objectively because they are blinded by the loyalty they feel toward their party. The question that then remains is *under which conditions* voters behave as 'stooges', failing to hold parties and candidates accountable, and when they behave as 'rational fellows', accurately attributing responsibility and ensuring democratic accountability. In order to address this question I explore four case studies in this thesis in which the partisan loyalties of voters are *challenged* by the quality of the performance of the incumbent. I refer to voters who experience such a conflict as 'cross-pressured partisans.'

To give an example, in the 2006 midterm elections many Republican partisans could be classified as cross-pressured when they learnt that a large group of Republican House representatives were involved in a lobbying and bribery scandal.⁵⁰ Republican partisans' loyalty toward their party was thus challenged by the behaviour displayed by their representatives. Alternatively, Democratic partisans may have experienced cross-pressure throughout the 2008 financial crisis if they felt that Democratic President Obama was doing a poor job in handling the financial crisis. In these situations a voter's partisan identification is in direct conflict with their evaluation of the per-

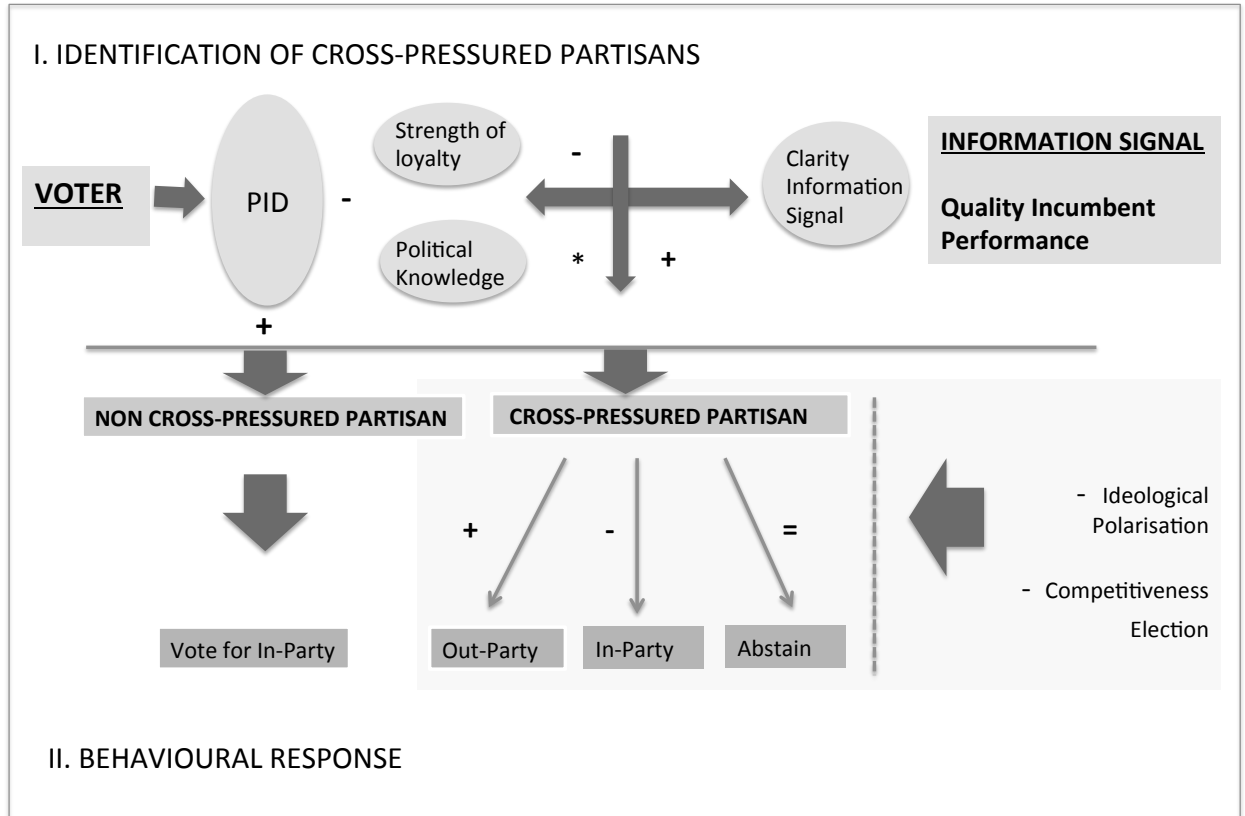
⁵⁰Mondak and Mitchell (2009)

formance of the incumbent government or candidate. In the latter scenario of the 2008 financial crisis a Democratic voter's partisan affiliation would suggest voting for the Democratic party in the 2010 midterms or the 2012 elections, yet her evaluation of Obama's economic performance would advise against this. Cross-pressured partisans thus have to engage in a difficult decision: do they prioritise their loyalty toward their party, or do they hold the incumbent party or candidate to account for their performance in office? The case-studies on cross-pressured partisans presented in the thesis are thus a tool to explore when partisans hold their preferred party or candidate accountable, when they fail to do so, and which factors affect this process.

1.2.1 A Framework of Cross-Pressured Voting

I develop a full theory as to the identification and behaviour of cross-pressured partisans in Chapter 2, but the visual representation of this theory of cross-pressured voting is shown here in Figure 1.1. Figure 1.1 consists of two parts. Part I, above the line, focuses on the emergence of cross-pressure and highlights factors that I expect will influence the likelihood that a voter experiences cross-pressure. Part II, below the line, focuses on the consequent behavioural response of cross-pressured partisans at election time. At the top of Figure 1.1 are the two main factors that are potentially in conflict: partisan loyalty and the information signal about the quality of the performance of the incumbent party, president or candidate. At the individual level, this interaction is thought to be moderated by two factors (left-side Figure 1.1): (1) the strength of a voter's partisan affiliation and (2) the level of interest in politics, or the knowledge of politics of the individual voter. These two factors are expected to affect the likelihood that a partisan voter receives and accepts the contradictory information signal and thus affect the likelihood that a partisan voter becomes cross-pressured or not.

Figure 1.1: Framework of Cross-Pressured Voting



The final factor that is thought to affect the likelihood that a voter experiences cross-pressure is depicted on the right-hand side of Figure 1.1 and is the clarity of the performance signal that a voter receives. Existing literature on the economic vote suggests that partisans are better able to overcome their bias when the economic signal is either clearly negative or clearly positive as opposed to ambiguous.⁵¹

In the bottom half of Figure 1.1 I explore the *behavioural* response of cross-pressured partisans. Note that I assume that voters who are *not* cross-pressured vote for the

⁵¹Chzhen et al. (2014); Healy and Malhotra (2013); Dickerson (2016); Parker-Stephen (2013)

in-party, the party with which they identify. Based on the information available to them, there is little reason to do otherwise. The cross-pressured voters however, have three potential behavioural responses: they can vote for the in-party, they can vote for the out-party, or they can abstain. The experience of cross-pressure is thought to *decrease* the likelihood of an in-party vote, but *increase* the likelihood of an out-party vote. This is the most important expectation of the framework of cross-pressured voting and will thus be tested in every empirical chapter. If it is true that cross-pressured partisans are *less* likely to vote for their in-party, then we may conclude that cross-pressured partisans accurately exercise accountability. Based on the existing literature (further discussed in Chapter 2), I expect that cross-pressure has a neutral effect on the likelihood to vote; the experience of cross-pressure is thus thought to neither increase nor decrease voters' likelihood to vote.

Finally, the right-hand side of Figure 1.1 lists two contextual factors that are thought to affect the behaviour of cross-pressured voters at election time. In general, I expect cross-pressured partisans to be *less* likely to vote for the party they identify with in response to the experience of cross-pressure. However, in line with existing research, I expect that this response is *moderated* by the electoral context in which partisans vote.⁵² Specifically, I expect that cross-pressured partisans are *less* likely to switch parties when the partisan stakes of an election are *high*. I thus expect that high levels of ideological polarisation and increased levels of electoral competitiveness will result in *lower* levels of democratic accountability and higher levels of partisan adherence. All hypotheses are discussed more in-depth in the theory chapter (Chapter 2).

⁵²For instance Eggers (2014); Stone and Simas (2010); Buttice and Stone (2012)

1.3 Empirical Case Studies and Methodological Approach

As mentioned, the core of the thesis exists of four empirical case studies. It is important to note that all these case studies focus exclusively on the US. The main reason for this is directly related to my research question. I am interested to explore the conditions under which partisans exercise accountability toward their in-party. It is thus helpful to focus my attention on a case study where partisan identities are indeed salient to voters. Although party loyalty is important to voters in many Western democracies, it is by far most prominent in US elections. This leads to the second reason to focus on the US: the majority of the literature on which this thesis builds uses the US as its case study.⁵³ I share this geographical focus so that I can relate my findings to the existing literature without having to account for geographical differences between my framework of cross-pressured voting and existing studies on retrospective voting.

The fact that I focus exclusively on the US implies that the findings of this study are limited to this context and should not be generalised beyond the US. The interaction between party loyalty and retrospective voting might indeed take a very different form in multi-party or coalition systems. In these systems, lines of accountability are much more complex and party loyalties tend to be much weaker. Furthermore, in systems where multiple parties compete, it is less clear which party constitutes the in-party and the out-party to a voter. We might therefore expect that voters in these systems experience conflict more often, both because their partisan filters might be weaker, but also because parties will govern in coalitions which require compromise.

⁵³Such as other works on cross-pressured voters Brader et al. (2014); Hillygus and Shields (2008); Lavine et al. (2012); Lavine (2001) but also literature on the interaction between partisan loyalty and performance evaluations Taber and Lodge (2006); Gerber and Huber (2010); Evans and Pickup (2010); Huber et al. (2012) and literature on natural disasters Healy and Lenz (2012); Malhotra and Kuo (2008)

On the other hand, it could also be easier for voters to respond to cross-pressure in multi-party systems because multiple ideologically similar parties are available as vote options. It is interesting to consider whether retrospective voting toward the in-party therefore works better in these systems compared to the American system. Although I do not address this question empirically in this thesis, I discuss extending the model presented here to different contexts in the final chapter of this thesis (Chapter 7 - Discussion).

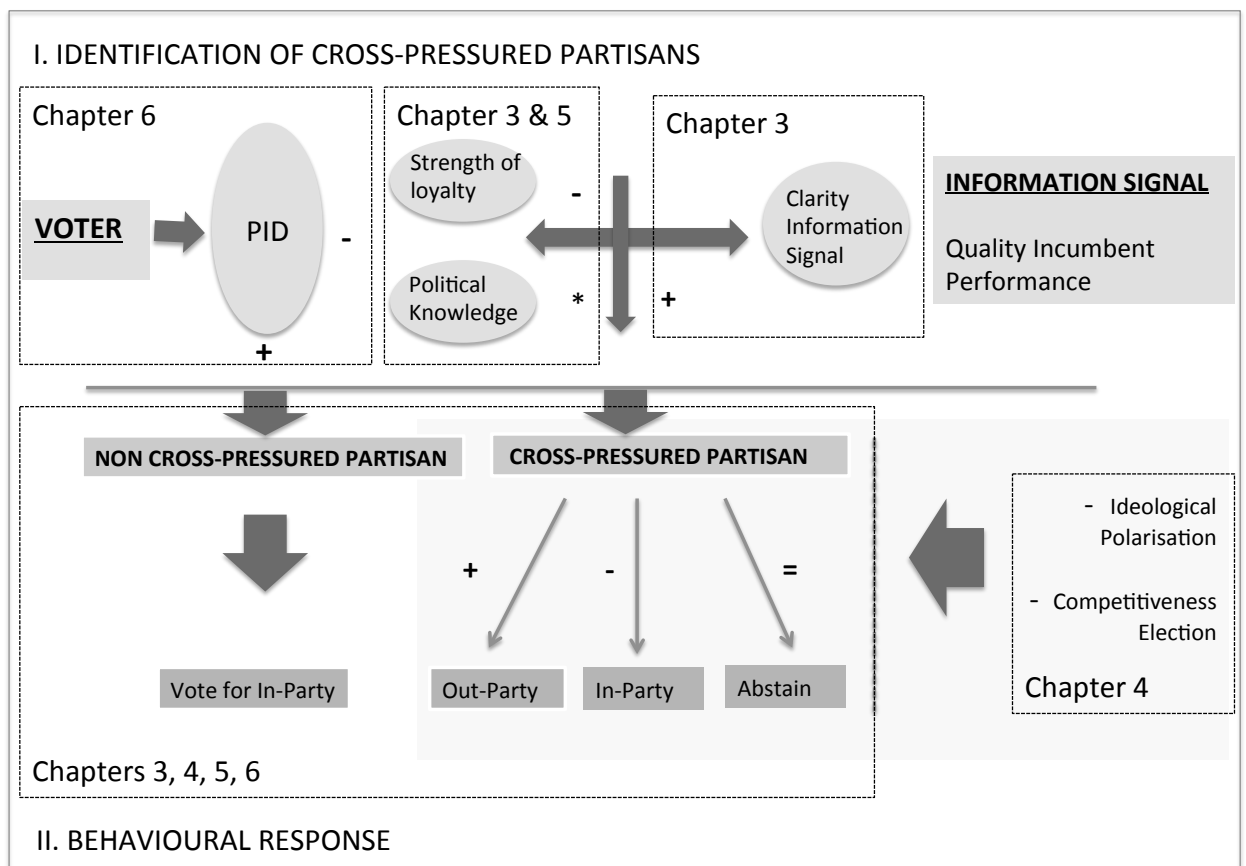
1.3.1 Empirical Case Studies

The thesis is structured as follows. First, I present the theoretical framework in Chapter 2. In this chapter I discuss the existing literature on retrospective voting, democratic accountability and its interaction with partisanship and voting behaviour. In Chapter 2 I develop the theory of cross-pressured voting as presented in Figure 1.1 and set out the main hypotheses that will be tested in the thesis. After this, I present four empirical case studies, these are listed in Table 1.1. The empirical case studies each relate to particular elements of the framework of cross-pressured voting. This is depicted in Figure 1.2.

Table 1.1: Empirical Case Studies

Chapter	Case Study
1	Introduction
2	Theoretical Framework
3	Economic Cross-Pressure in the 1992, 2000 and 2004 Presidential Elections
4	Candidate Competence in the 2006 Midterm Elections
5	The Watergate Scandal
6	Lab Experiment on Corruption
7	Conclusion

Figure 1.2: Thesis Chapters Related to the Framework of Cross-Pressured Voting



Across the empirical chapters, I explore different types of cross-pressure. In line with the majority of studies on retrospective voting, the first case study focuses on voters' evaluations of the president's management of the national economy (Chapter 3). Because the national economy is a topic that is particularly salient to most voters, we might expect that voters are particularly observant of performance signals in the economic domain. I therefore also explore two other types of cross-pressure. In Chapter 4, I explore cross-pressure between a voter's partisan identification and the quality signal a voter receives about the competence of House candidates. In Chapter 5, I focus on a high profile scandal, Watergate, which revolves around political corruption. In Chapter 6, I present a lab experiment in which I explore a different type of corruption and test whether voters punish candidates who embezzle funds both in the absence and presence of group identities. The clarity of the performance signal also varies across chapters. Voters may struggle to attribute responsibility to incumbents in light of information about economic performance⁵⁴ and it may also be difficult for voters to estimate the competence of House candidates, especially challengers.⁵⁵ As such, the information requirements are high for voters in Chapters 3 and 4. In Chapter 5, I therefore focus on a case study, the Watergate scandal, in which there was a very clear performance signal. In Chapter 6, I furthermore present a lab experiment in which voters operate under conditions of full information. Chapters 5 and 6 thus present case studies in which it is easy for voters to pick up the negative signal about incumbent performance.

1.3.2 Methodological Approach

In addition to varying the type of cross-pressure and the clarity of the performance signal, I also use a range of different methodological approaches across the empirical

⁵⁴Powell and Whitten (1993); Duch et al. (2000)

⁵⁵Buttice and Stone (2012)

chapters. I use both observational data, a quasi-experimental and an experimental approach. I vary my methodological approach because I am conscious of the fact that there are methodological challenges with the study of partisan-performance cross-pressure, especially when using observational data. First, as discussed, studies have shown that retrospective evaluations are potentially endogenous to partisan preferences.⁵⁶ This suggests that measures of economic performance - or candidate competence - are likely not objective, but rather are biased by partisan attachments. Although disagreement exists as to the extent of this problem, it is likely that survey-based measures of incumbent performance contain measurement bias.⁵⁷ A second problem that may exist is that partisans can *update* their partisan affiliation in light of negative quality signals about the performance of their preferred party. A third potential problem with the measurement of cross-pressure is the fact that it is difficult - or impossible - to control or measure the level of information that voters actually receive about the quality of incumbent performance. I use several strategies to address these issues, these are discussed below.

In Chapter 3 I focus on economic cross-pressure in the 1992, 2000 and 2004 Presidential Elections. In the chapter I use two different panel studies from the American National Election Study (ANES); one that covers the period 1990-1992 and one that covers the period 2000-2004. I use these two panel studies to explore whether there are partisan voters in these two time periods that report retrospective evaluations of the President's handling of the economy that contradict their partisan identification. This data has several benefits for my research purposes. First, in these periods the US experienced both economic decline (1992), economic prosperity (2000) and a middle-ground economic period (2004) where the economic signal was less clear to voters.

⁵⁶Evans and Andersen (2006); Evans and Pickup (2010)

⁵⁷See Lewis-Beck (2006); Lewis-Beck et al. (2008); Nadeau et al. (2012) who are less concerned about partisan bias.

Second, the two panels cover both Republican and Democratic presidents. Third, from a methodological perspective the panel nature of this data allows me to conduct various robustness checks to control for the effect of partisan bias on retrospective economic evaluations as well as potential partisan updating. These robustness tests include panel models with individual fixed effects and the inclusion of lagged partisanship to account for partisan bias. I also replicate all my findings across the three different presidential elections, 1992, 2000 and 2004, to test whether the findings are robust across election cycles.

In Chapter 4, Candidate Competence Cross-Pressure in the 2006 House Elections, I focus on a different type of cross-pressure and explore how partisan voters behave when the out-party candidate for the US House of Representatives is more competent than the candidate of the party the voter identifies with. In the chapter I use the *2006 Congressional Election Study* and focus specifically on the elections for the House of Representatives.⁵⁸ The main reason to use data on House elections is because it allows me to assess whether the electoral context of a particular election affects the behaviour of cross-pressured partisans. Specifically, I explore whether variation in levels of polarisation and electoral competitiveness of a district affect the extent to which partisan voters exercise accountability. The data I use in Chapter 4 has two additional important benefits. First, the data provides competence evaluations of *both* incumbents and challengers. I am thus able to generate a competence differential between incumbent and challenger, rather than rely on one-sided evaluations of the incumbent. Second, the data includes candidate evaluations of both voters and experts. This allows me to contrast expert and voter measures and control for potential bias in the voter measure. Potential downsides of the data are the fact that only a limited number of competence measures are available and the fact that the

⁵⁸Stone, Walter J. 2010. 2006 Congressional Election Study. University of California, Davis, CA. (<http://electionstudy.ucdavis.edu/>)

data is not a panel study. As such, it is not possible to observe voting behaviour across multiple elections.

In Chapter 5, I use the eruption of the Watergate scandal as an external signal of poor performance. I use two different types of data and two different research designs in Chapter 5. First, I have collected district level electoral outcomes for the 1974 midterm elections, the first elections in the post-Watergate era. Using this data, I construct a quasi-experimental research design in which I focus on the electoral outcomes for the Republican members of the House Judiciary Committee on Impeachment. This committee was tasked to advise the House as to whether or not President Nixon should be impeached in the wake of Watergate. The committee consisted of 20 Democratic House representatives and 17 Republican House representatives. The Republican representatives were divided on the impeachment question. In Chapter 5 I use this variation in the views of the Republican House representatives in a difference in difference analysis and compare whether Republican House representatives who voted against Nixon's impeachment (Nixon loyalists) fared worse in the 1974 elections compared to Republican representatives who voted in favour of Nixon's impeachment (Nixon skeptics). In addition to the district level analysis I also analyse individual level data, again of the American National Election Study (ANES). The benefit of the individual level data is that it consists of a panel study that includes a pre-Watergate wave (1972) and a post-Watergate wave (1974) which I use to track attitudinal and behavioural changes of the panel members in the period in which Watergate occurs.

Although the first three empirical case studies thus employ several methods to address issues of measurement bias, concerns might still exist with respect to the measurement of cross-pressure between partisan identification and retrospective performance evalu-

ations. In Chapter 6 I therefore present a lab experiment that is focused specifically on creating a condition of cross-pressure. In the lab experiment participants are divided into voters and candidates. The candidates and voters participate in repeated rounds of elections in which the candidates are presented with the opportunity to embezzle funds. The experiment tests whether voters punish corrupt candidates and does so in both the absence of group identities (the control group) and when group identities are present (the treatment group). From a methodological perspective the lab experiment offers several benefits. First, it presents the opportunity to directly measure cross-pressure. Second, voters operate in a situation of full information. Third, voters in the experiment are not able to update their group identification across the various elections and hence cannot escape cross-pressure by doing so. Fourth, candidates are not able to offer material benefits to in-group members to avoid electoral punishment of poor performance. The experiment hereby offers a causal test of the effect of group identification on the punishment of corrupt candidates.

Table 1.2 shows the four empirical case studies and lists the methods used and data used in each chapter. The final two columns of Table 1.2 displays the trade-off in each chapter between internal and external validity of the findings.

Table 1.2: Methods of Analysis in the Thesis

Chapter	Methodological Approach	Internal Validity	External Validity
3. Economic Cross-Pressure	Panel Study	Medium	High
4. Candidate Competence	Individual and Expert Survey	Low	High
5. Watergate Scandal	Diff-in-Diff and Panel	Medium	High
6. Lab Experiment on Corruption	Lab Experiment	High	Low

1.4 Summary of the Main Findings of the Thesis

As discussed, the four empirical case studies all explore a particular element of the framework of cross-pressured voting that forms the core of this thesis (Figure 1.2). The empirical findings of the case studies illuminate the conditions under which partisans hold their in-party to account for its performance in office. In this section I briefly highlight the main findings of the thesis.

In the first empirical case study, Chapter 3, I explore whether there are partisans who report retrospective evaluations of the president's handling of the economy that *contradict* their partisan preferences. I find that this is the case. In fact, the most remarkable finding of Chapter 3 is the high number of partisans who report performance evaluations of the incumbent government that contradict their partisan preferences. I find that in 1992 a total of 57% of partisans are cross-pressured, in 2000 this is true for 59% of the partisan electorate. This confirms the fact that even partisans are responsive to clear economic signals. In line with expectations, in 2004, when the economic signal is weaker, the number of cross-pressured voters is lower at 37% of partisans. I furthermore find that partisans with moderate levels of partisan affiliation are *most* likely to hold conflicting performance evaluations. Finally, I find that cross-pressured partisans are no more likely to abstain from voting but, when they vote, are significantly *more* likely to vote for the out-party in response to economic cross-pressure. The first case study thus offers evidence to suggest that partisans *do* exercise accountability toward their in-party in light of economic signals, especially when the economic signal is unambiguous.

In Chapter 4, the second case study, I define cross-pressure as a mismatch between a voter's partisan identification and the competence of the incumbent and challenger candidate for the US House of Representatives. I use both voter and expert evalu-

ations of candidate competence. A voter is cross-pressured when they identify with the incumbent, but either the voter or the expert view the challenger candidate as more competent (or vice versa). In line with the chapter on economic cross-pressure (Chapter 3), I again find that there is no effect of cross-pressure on turnout and find that cross-pressured partisans are *less* likely to vote for their in-party compared to non cross-pressured partisans. Again, this suggests that voters are able to overcome partisan loyalty in favour of performance or accountability concerns. However, in line with the framework of cross-pressured voting (Figure 1.1), I now also explore whether the effect of cross-pressure on vote choice is *moderated* by the level of ideological polarisation and the competitiveness of an election. I find that this is the case. Put simply, when the partisan stakes of an election are high, democratic accountability is low. The effect of cross-pressure on vote choice disappears when the partisan stakes in a district are very high, i.e. when the incumbent and challenger are ideologically far removed from each other, or when the election is very competitive. This suggests that there are important contextual factors that affect the likelihood that partisans hold in-party candidates to account.

In Chapter 5, I turn to explore a case study in which voters receive a clearly negative signal about the quality of the incumbent president: the Watergate scandal. Watergate revolved around a range of illegal activities conducted by senior members of the Republican party, including incumbent President Richard Nixon, who resigned as a result. The Watergate scandal challenged the partisan loyalties of Republican voters who had to face the fact that the party (and president) they voted for engaged in criminal activities and abused their trust. In the chapter I first explore whether Republican House representatives who tried to protect President Nixon against impeachment were punished by the electorate. I find no evidence to suggest this is the case. Rather, it appears that the electorate punished the Republican party across the

board. The analysis of individual level data confirms this. In line with Chapters 3 and 4 I find that cross-pressured partisans, in this case Republicans, were significantly less loyal to their party between 1972 and 1974 compared to non cross-pressured partisans (Democratic partisans and Independents). However, this is not true for all Republican partisans. Partisans with strong levels of affiliation to the party voted for the Republican party regardless of the events of Watergate. I furthermore find that the Watergate scandal depressed turnout among moderate Republicans in the 1974 midterm elections. This is in contrast with the findings of Chapter 3 and 4 and suggests that cross-pressure in light of a political corruption scandal may also have negative effects on democratic accountability. It furthermore suggests that the behaviour of cross-pressured partisans might differ across performance domains.

In the final empirical chapter, Chapter 6, I explore the trade off between partisan loyalties and performance evaluations in a lab setting. In the experiment I test whether voters punish candidates that engage in corrupt activities, specifically the embezzlement of funds. The treatment that is administered in the experiment is group identification. Groups are constructed on the basis of the minimal group paradigm and thus do not reflect any type of ideological or issue positions, but rather represent a shared social identity between candidates and voters (expressive partisanship). I find that once group identities are introduced, voters punish out-group candidates, but do *not* punish in-group candidates for embezzling funds. The lab experiment thus illustrates that voters neglect to hold an in-party candidate to account even when they are fully informed and group identities are recently constructed. It also confirms the fact that the electoral response in light of corruption might differ from the response to economic or competence cross-pressure. Finally, the fact that group identities that were created in the lab had a strong effect on the punishment of corrupt candidates suggests that the effects of social group identification are likely much

stronger in real life situations. The experimental findings hereby highlight the need for a better understanding of the different forms that partisan identification can take (expressive versus instrumental) and its effects on democratic accountability.⁵⁹

1.5 Cross-Pressured Partisans and Democratic Accountability

The majority of partisans in America vote for the party they identify with. Yet, in this thesis I argue that this observation obscures a more nuanced reality of decision-making among American partisans. Using four empirical case studies, this thesis shows that at times more than half of the partisan electorate faces a conflict between their partisan loyalty and their retrospective evaluations of the performance of the incumbent party or candidate. This number is higher when the performance or competence signal is stronger and becomes virtually unavoidable in times of a large-scale scandal. Cross-pressured partisans respond to the negative performance signal under certain conditions. This thesis finds that partisans who hold a negative opinion of the economic management of their in-party president or a positive opinion on the economic management skills of the out-party president are *less* likely to vote for their in-party candidate. Partisans are also responsive to competence differences between candidates running for the US House of Representatives and punish a party that is involved in a high profile political scandal. Under these circumstances, partisans exercise accountability effectively.

Yet, both individual level characteristics and electoral conditions affect the extent to which partisans exercise accountability toward the in-party. For instance, when partisan stakes are high, democratic accountability is low. These conditions can potentially

⁵⁹See for recent work on this Huddy and Aaroe (2015) and for an overview of the literature in the field of psychology Scheepers et al. (2006). I also discuss this further in Chapter 7 - Discussion.

be manipulated by politicians who can increase ideological polarisation to lower the importance of performance failures or competence differentials. Furthermore, there are important differences between partisans in the extent to which they exercise accountability. As may be expected, partisans with strong levels of attachment to the party are significantly less likely to hold their in-party or candidate accountable. Moderate and leaning partisans are more likely to do so however. Moderate partisans appear especially responsive to information on government performance that contradicts their partisan affiliation. The findings of the thesis furthermore suggest that different types of performance might incite different types of electoral responses. Economic and candidate cross-pressure are found to increase vote-switching but not abstention, whereas a large scale corruption scandal increases both. The embezzlement of funds did not increase vote-switching at all in a lab setting.

Partisans are often limited in exercising accountability due to their prior attachments and loyalties. Yet, not all partisans are loyal to their in-party under all conditions. The main contribution of this thesis is to illustrate that for a substantive group of partisans, the decision who to vote for reflects a *conflict* between their group loyalty and their retrospective evaluations of the performance of the incumbent. The study of this type of conflict matters because it contributes to our understanding of democratic accountability and the quality of democratic governance. Partisans who always vote for their preferred party may rightfully be seen as a hazard to democracy. Yet, until now, little is known about the partisans whose loyalties are challenged. This thesis sheds light on these voters and explores who they are, how they behave and which factors condition their behaviour. This is important because these voters are the potential safe-keepers of the system of electoral democracy.

Chapter 2

A Theory of Cross-Pressured Voting

Academic theorists of democracy sometimes dismiss the retrospective theory of political accountability as a hopelessly thin and uninspiring account of democracy. Our concern is different - that it simply does not work. (...) Where does this leave the current state of democratic theory? The short answer is: in a shambles.

- Achen and Bartels (2016, p.306) *Democracy for Realists: Why Elections Do Not Produce Responsive Government* -

In this chapter I present the theory of cross-pressured voting that I test in the empirical chapters. I start this chapter with a brief discussion of theories of voting behaviour in section 2.1. I highlight how elections may be used both to select representative governments, as well as to punish or reward the incumbent government. These two models are often referred to as either the ‘mandate’ or the ‘accountability’ model of democracy.¹ Although the distinction between these two models is clear on paper, it is often difficult to disentangle these models in real life situations. I conclude that in most cases, voters will simply do a bit of both. In sections 2.2 and 2.3 I discuss the

¹Przeworski et al. (1999)

accountability model, also the retrospective vote model, in more detail since it is the focus of this thesis. I first discuss the central theoretical underpinnings of this model and then assess the empirical evidence that supports it. In line with the argument set out in the Introduction chapter of this thesis, I show in section 2.4 that a contradiction exists in the literature on retrospective voting.² On the one hand, studies find that voters hold governments accountable for their performance in office, especially for economic management.³ On the other hand, many recent studies find that voters are myopic, irrational and biased and therefore fail to live up to the demands of the retrospective voting paradigm.⁴ One of the most important barriers to objective retrospective voting appears to be partisan loyalty because it affects the consumption, interpretation and evaluation of performance information.⁵

In section 2.5, I therefore argue that we need to increase our understanding of the response of voters to negative information signals about their preferred party in order to understand when and why the accountability mechanism fails. In these situations, partisans are cross-pressured and the retrospective vote paradigm is put to the test. Consequently, we can observe whether or not partisans act against their own party. Also in this section I discuss existing studies of ambivalent partisans and persuadable voters which both take a similar approach to the study of conflicted partisans and highlight similarities and differences between these studies and this thesis.⁶ In section 2.6, I present the framework of cross-pressured voting and introduce the main hypotheses that are tested in the subsequent empirical chapters.

²See also Healy and Malhotra (2013) who also identify this tension in the literature between their own work and the work of Achen and Bartels.

³Duch and Stevenson (2008); Lewis-Beck (2006); Lewis-Beck et al. (2008); Nadeau et al. (2012); Malhotra and Kuo (2008); Healy and Malhotra (2009); Bechtel and Hainmueller (2011)

⁴Bartels (1996, 2000); Bartels and Achen (2004); Achen and Bartels (2004, 2013, 2016); Huber et al. (2012); Healy and Lenz (2012)

⁵Taber and Lodge (2006); Lavine et al. (2012); Gerber and Huber (2010); Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013)

⁶Basinger and Lavine (2005); Lavine et al. (2012); Lavine (2001); Hillygus and Shields (2008)

2.1 The Two Main Functions of Elections

In essence, democratic governance reflects a principal-agent relationship that centres on the delegation of power from *electors* (the voters) to the *elected* (politicians) at regular intervals. The regular occurrence of elections is thought to lead to a representative system of governance. Here, a *representative* government is understood to be a government that acts in the interests of the people it serves.⁷ A representative government is thought to be normatively desirable because it aims to execute the will of the people who have brought it to power. Regular elections are thought to facilitate representative or high quality governance via two pathways: (1) elections provide governments with an ideological mandate; (2) elections ensure that the government is accountable for its actions.⁸

The mandate view of democratic behaviour suggests that: ‘citizens signal to governments what to do’, whereas the accountability view suggests that elections allow voters to: ‘judge whether governments have done what they should have.’⁹ Thus, the mandate view of elections centres on the fact that voters use elections to *select* politicians who (are thought to) represent a particular policy mandate. The accountability view suggests that voters use elections to either continue or discontinue the transfer of power to the incumbent. Importantly, *both* these pathways may lead to a representative government. Either voters successfully elect competent and honest

⁷Pitkin 1967 as cited in Przeworski et al. (1999, p.2)

⁸See Przeworski et al. (1999) Chapter 1 by Manin, Przeworski and Stokes, p.29-54. It is helpful here to note that a whole range of terms is used interchangeably in the existing literature to refer to models of elections and vote choice. Examples of terms used are “selection” and “sanctioning” by Fearon (1999); Duch and Stevenson (2008), “ideological” and “accountability” by Bermeo and Bartels (2014), “prospective” and “retrospective” by Downs (1957), “issues” and “performance” by Alvarez et al. (2000) and “spatial” and “behavioural” by Adams et al. (2005). I here follow the terminology used by Przeworski et al. (1999) because the terms “mandate” and “accountability” offer broad categories that encapsulate many of the other terms used.

⁹Przeworski et al. (1999, p.16)

candidates who execute policies in line with the mandate that has been provided to them, or elections are used to punish governments who have performed poorly. Those who see elections as a way of providing a *mandate* often focus on issue, ideological or party-based voting. These models are often prospective, that is future-oriented, in nature and focus on the congruence between a politician’s campaign platform and a voter’s ideological viewpoints or party attachments. In the context of American elections, most authors tend to focus on the importance of partisanship and partisan loyalty.¹⁰ Those who view elections as a tool of *accountability* on the other hand focus on past incumbent performance, often in terms of the performance of the national economy. These models are also referred to as ‘retrospective voting’ models because voters use information on the past performance of the incumbent government to decide whether or not to continue the transfer of power to the incumbent.

Although these two views are often viewed as mutually exclusive, they do not necessarily need to be seen as conflicting.¹¹ In fact, although these two models are neatly distinguishable in theory, in reality they often interact and overlap.¹² A by-product of sanctioning incumbents for instance is the selection of more promising challengers, just as the selection of high quality challengers automatically results in the sanctioning of low quality incumbents.¹³ It is thus problematic to make too strict a separation between future-oriented and past-oriented models, or between selection and sanctioning models of democracy. From the perspective of voters, the two models of voting are often not easy to distinguish. Rather, the majority of voters will want to satisfy *both* motivations of vote choice at election time. Voters display: ‘a generalized tendency

¹⁰See Downs (1957); Enelow and Hinich (1990) for ideological or spatial models. See Campbell et al. (1960); Lazarsfeld et al. (1944); Berelson et al. (1954) for the seminal works on the importance of partisanship to American elections.

¹¹For examples of studies that contrast the two see Alvarez and Nagler (1995, 2000); Alvarez et al. (2000); Blais et al. (2004)

¹²For supporters of this argument and examples see Duch and Stevenson (2008); Besley (2007); Przeworski et al. (1999); Fearon (1999); Adams et al. (2005); Grofman (1985); Downs (1957)

¹³Fearon (1999, p.71)

to reward (punish) the incumbent party (...) and a generalized tendency to vote for the party that best defends one's position on the major issue of the day.'¹⁴

The accountability mechanism is often portrayed as a model of 'punishment and reward', but there are actually three different pathways through which information on incumbent performance may result in political accountability and high quality governance. These different pathways also illustrate the interaction between prospective and retrospective vote models. First, information on incumbent performance can inform the *selection* of competent political officials. In this case, voters use information about the *past* performance of an incumbent to infer and predict its *future* competence.¹⁵ Information about past performance then informs future-oriented selection decisions. Second, retrospective voting may increase the quality of government *indirectly* via the principle of 'moral hazard'. Politicians aim to perform to a high standard because they know their re-election depends on it. If voters indeed punish poor performance at the polls, then the mere existence of regular elections should incite high quality governance.¹⁶ Third, retrospective voting may ensure high quality government *directly* through the mechanism of punishment and reward.¹⁷ A clear example of the punishment and reward model is the 'economic vote', through which voters punish governments for economic downturns and reward them for periods of economic growth. This latter view reflects what is commonly referred to as 'retrospective voting' in the literature. Regardless of the exact pathway, information on the quality of incumbent performance and voters' responsiveness to this information are highly important to ensure the quality of democratic governance.

¹⁴Blais et al. (2004, p.556) note that the 'reward and punish' comment in this quotation relates specifically to 'good (bad) economic times.'

¹⁵Downs (1957) and for a fully developed selection model of the economic vote see Duch and Stevenson (2008)

¹⁶Ferejohn (1986); Fearon (1999); Besley (2007)

¹⁷As per Key (1966); Fiorina (1981)

2.2 The Accountability Mechanism: Retrospective Voting

Traditional retrospective vote models view elections as a tool to establish accountability between those in power and those who elect them. The first authors to discuss these models reacted to the existing view in political science in the 1950's and 1960's that voting was mostly an expression of ideological preferences in the form of partisanship.¹⁸ Authors who introduced the retrospective vote models disagreed with these earlier models in two important ways. First, the early models of voting behaviour depended heavily on the mandate view of elections and assumed that voters held strong issue and party preferences. Yet, empirical evidence showed that most voters only had a very limited knowledge of (or interest in) political issues.¹⁹ A contradiction thus existed between the models and reality. The lack of political knowledge among the electorate posed a serious challenge to the models of voting behaviour that interpreted elections mainly as a rivalry between competing ideologies.

Second, authors who introduced the retrospective vote model criticized the fact that partisan loyalty was seen as the sole determinant of voting behaviour. This suggested that voters were unresponsive to any external information or events. Morris Fiorina (1981) characterized the view on partisanship at the time as: 'a virus that you contracted from your parents at an early age.'²⁰ The newly developed retrospective vote theories on the other hand suggested that voters used a different decision-mechanism when they voted. According to V.O. Key (1966):

voters may respond most assuredly to what they have seen, heard, experienced. (...) voters may reject what they have known; or they may approve

¹⁸First retrospective models were by Key (1966); Fiorina (1981) and the partisanship research was by Lazarsfeld et al. (1944); Campbell et al. (1960)

¹⁹See Bartels (1996) for a discussion on this topic.

²⁰Fiorina (1981, p.ix)

what they have known. (...) what if we would suppose that the electorate looked at the public scene, did not like what it saw, and performed the act of governance for which an electorate is superbly equipped, that is, it threw the “ins” - out?²¹

This rather intuitive statement that the ‘electorate is superbly equipped’ to throw the “ins” - out, forms the basis of our common understanding of the retrospective vote model. Perceived this way, retrospective voting ensures that governments are held accountable for their actions through either punishment or reward at election time. The retrospective vote model provides an intuitive, elegant and persuasive account of electoral behaviour. Furthermore, it offers a solution to the fact that most voters display low levels of knowledge about politics. According to the retrospective vote model, voters need not be fully informed about political events to make good decisions. Rather, voters may rely on their own ‘lived experience’ and simply ask themselves whether their welfare has improved under the rule of the incumbent.

2.2.1 Examples of Retrospective Voting: The Economic Vote

Perhaps the most convincing evidence in support of the retrospective vote model comes from studies on the so-called ‘economic vote.’ The economic vote literature studies the electorate’s response to economic conditions. The basic premise of the theory is that incumbent governments are voted out of office when the economy performs poorly and retained when they preside over economic growth. A substantive body of work exists that corroborates the fact that government popularity and incumbent vote share are correlated with the performance of the national economy in a variety of countries.²²

²¹Key (1966), first part quote p.52, second and third part p.63.

²²See for instance Duch and Stevenson (2008); Nadeau et al. (2012); Powell and Whitten (1993); Lewis-Beck (1980); Lewis-Beck and Paldam (2000); Lewis-Beck (1988); Lewis-Beck et al. (2008);

A study of Duch and Stevenson (2008) for instance finds evidence of retrospective voting on the basis of economic performance across 19 Western European countries.²³ The study finds that voters are able to discern between situations in which the government has control over the economy and when it does not. This finding is confirmed by other cross-country studies.²⁴ A recent study on 10 Western European countries by Nadeau et al. (2012) likewise finds that voters' economic perceptions are in line with real economic developments and significantly affect vote choice. Several studies have also shown that voters across Europe voted against governments who presided over the recent financial crisis.²⁵

Variation in the economic vote across countries may be explained by voters' ability to identify responsible government actors. The so-called 'clarity of responsibility thesis' explains why economic voting is more pervasive in some countries compared to others and also highlights the importance of the attribution of responsibility in the retrospective vote paradigm.²⁶ Most studies on the economic vote find that voters are more likely to base their vote on their impression of the state of the *national* economy as opposed to their *individual* financial status.²⁷ A small number of studies has ex-

Lewis-Beck and Stegmaier (2013). For helpful overviews of the ever-expanding literature on the economic vote see Lewis-Beck and Paldam (2000); Lewis-Beck and Stegmaier (2013)

²³Duch and Stevenson (2008) use a large body of survey evidence for this, collating 165 surveys across 19 countries.

²⁴See for instance Nadeau et al. (2012) who compare economic voting across 10 European countries over two decades. They furthermore show that the findings are robust to various methodological challenges. For specific case studies of the importance of the economy to the vote see Alvarez and Nagler (1995, 1998a); Alvarez et al. (2000); Blais et al. (2004) For a recent example of economic voting in light of the great recession see the edited volume by Bermeo and Bartels (2014)

²⁵Chzhen et al. (2014); Bermeo and Bartels (2014)

²⁶See for the seminal work on clarity of responsibility Powell and Whitten (1993), see for further development and refinement of the concept Hobolt et al. (2013); Tillman (2008) and for an application to coalition governments Fisher and Hobolt (2010)

²⁷Thus there is more evidence of sociotropic voting compared to egotropic voting. It is not entirely clear how this finding should be interpreted. It could be the case that voters care more about the national economy, or that they attribute responsibility for national events to the government and responsibility for individual conditions to themselves. See for discussions on sociotropic and egotropic voting Duch et al. (2000); Duch (2001); de Vries and Giger (2014); Gomez and Wilson (2001, 2006,

tended the study of performance based voting to other policy domains.²⁸ A study by de Vries and Giger (2014) suggests that voters hold governments accountable across a range of policy domains such as social welfare, immigration and national security, but that the extent to which voters do so depends on the importance they attach to the particular policy area.²⁹

Although a wealth of empirical evidence thus corroborates the economic vote at the macro-level, the economic vote remains contentious at the level of individual vote choice. Questions have been raised as to the ability of individual voters to objectively evaluate incumbent performance and accurately attribute responsibility to government officials.³⁰ I discuss these criticisms in more detail in section 2.3 below.

2.2.2 Further Examples of Retrospective Voting: Natural Disasters

A second body of literature which corroborates the idea that voters use elections to hold governments to account for their performance in office is the study of natural disasters. Natural disasters present helpful case studies to assess whether or not voters exercise accountability toward the government because they are highly visible events that are usually very important to voters. Natural disasters such as tornadoes, hurricanes, floods and droughts have severe consequences for the individual well-being of citizens and a government's response to these events may thus be expected to shape voters' electoral decisions. Several studies find that this is indeed the case.

2008, 2011)

²⁸See for instance de Vries and Giger (2014); Tilley and Hobolt (2011)

²⁹de Vries and Giger (2014) note that the study also finds that the extent to which voters hold governments accountable depends on individual level characteristics of the voter, most importantly the level of political sophistication. The sophistication gap decreases when a topic is very salient to voters.

³⁰With regards to evaluation of incumbents see Bartels (2000, 2002); Evans and Andersen (2006); Evans and Pickup (2010); Gerber et al. (2010). With regards to attribution of responsibility see Tilley and Hobolt (2011)

A study of Germany by Bechtel and Hainmueller (2011) explores the response of voters in light of the flooding of the Elbe river in 2002. As it happened, the Elbe flooded just prior to the 2002 elections. A comparison between areas that were affected by the flooding and those that were not suggests that the incumbent SPD government increased its vote share by 7 percentage points in response to its effective disaster relief operation in the affected areas.³¹ A study focused on the US by Healy and Malhotra (2009) of disaster relief spending programs across more than 3,000 counties also finds that voters rewarded the incumbent party for disaster relief spending, but notes that this has the perverse effect that governments are therefore incentivized to spend more on relief compared to prevention efforts. A potential problem with retrospective voting is thus that voters respond most strongly to directly visible, recent events, rather than cumulative or preventative efforts.³²

In another study on blame attribution in the wake of tornadoes, Healy and Malhotra (2013) offer a more positive image of voters' retrospective abilities. The study finds that voters are able to distinguish between events that occur outside the control of politicians (such as a tornado) and events that are within the control of elected officials (response to the tornado). This suggests that: 'voters do not appear to simply respond to tornadoes emotionally, they seem to separate out economic and non-economic concerns.'³³ In other words, voters separate the signal from the noise. Furthermore, a study on blame attribution in response to hurricane Katrina suggests that although voters were initially biased in their attribution of responsibility on the basis of their partisan preferences, once they were provided with accurate information

³¹Bechtel and Hainmueller (2011)

³²This confirms the issue raised by de Vries and Giger (2014) that salience affects the likelihood that voters exercise accountability. See Healy and Lenz (2012); Huber et al. (2012) for a discussion of the fact that voters tend to overweight recent to cumulative performance.

³³Healy and Malhotra (2013, p.296)

on lines of responsibility these biases disappeared.³⁴

Studies of natural disasters thus offer a more complex insight into the responsiveness of voters to government actions. Voters' retrospective judgements are at times clouded by partisan motivations and sometimes their tendency to respond to highly salient and visible acts of government performance has detrimental effects on voters' own well-being.³⁵ Yet, studies on the electoral response to natural disasters also suggest that voters are clearly responsive to government actions they observe and that they are able to overcome particular biases once provided with accurate information. The authors themselves conclude therefore that there is:

reason to be cautiously optimistic about the capacity of citizens to make unbiased blame attributions, an important responsibility in democratic systems.³⁶

2.3 Criticisms of the Retrospective Voting Paradigm

In contrast to the studies discussed above, a range of recent studies criticise the retrospective vote paradigm. These criticisms may be divided in two types. First, there are general concerns as to people's ability to develop retrospective evaluations and act accordingly. Second, there are specific concerns as to the detrimental effects of partisan attachments on the functioning of the retrospective vote mechanism.

³⁴Malhotra and Kuo (2008)

³⁵As described in Healy and Malhotra (2009) in the case of the lack of disaster prevention spending.

³⁶Malhotra and Kuo (2008, p.131)

2.3.1 Myopic and Irrational Voters

A comprehensive critique of the retrospective vote model is offered by Christopher Achen and Larry Bartels.³⁷ The main argument of their research is that retrospective voting is more difficult to achieve for voters than is oftentimes thought. Achen and Bartels (2016) find that most voters are myopic and irrational and thus do not live up to the demands put on them by the model of retrospective voting. In studies on shark attacks, floods, droughts, economic recessions and presidential elections, the authors show that although voters at times oust governments who have performed poorly, more often than not the retrospective response is not based on a careful calculation of changes in individual well-being and consequent blame attribution to the government. Rather, voters remove governments from office in response to general negative events that occur in their lives. Voters consistently blame governments for conditions that are outside of its control and are strongly influenced by party cues. Voters do not act as: ‘a rational seeking to better one’s status by the ballot’, but instead are: ‘merely blindly striking a blow at a scapegoat.’³⁸ Any type of substance that is attributed to these retrospective actions reflects post-hoc rationalizations of irrational behaviour.

A study by Huber et al. (2012) tests the retrospective vote mechanism in a lab setting and confirms the work of Achen and Bartels. The study finds that lab participants paid more attention to recent as opposed to cumulative incumbent performance and that participants allowed a random lottery to affect their decisions. Participants were furthermore easily influenced by rhetoric when they evaluated the performance of the incumbent. The most striking element of the lab experiment is the fact that voters received full information and were in fact incentivized to use a retrospective decision-

³⁷Bartels (1996, 2000, 2002); Bartels and Achen (2004); Achen and Bartels (2013, 2016)

³⁸Key (1966, p.628) as cited in Achen and Bartels (2016, pp.112-113)

rule. Other research has shown that this pattern also exists in real life where voters have a strong bias to focus on election-year economics.³⁹ Voters thus do not evaluate what a government has done for them over the past four years, but rather evaluate what the government has done for them *recently*, providing politicians with leeway to manipulate their record of economic stewardship. Studies in the field of behavioural economics also suggest that people often divert from the rational choice model when they make important decisions. The studies find for instance that people are very susceptible to framing effects, struggle to interpret probabilities and numbers and often rely on intuition over facts.⁴⁰

The retrospective vote mechanism puts several demands on voters: (1) it requires voters to evaluate changes in their own welfare; (2) it requires voters to attribute responsibility to relevant government actors for these changes in welfare and; (3) it requires voters to act on these inferences by either rewarding or punishing those government officials who are responsible. Although retrospective voting seems quite easy, it is thus difficult for most voters to accurately complete these steps. One very important impediment to the successful fulfilment of this process is partisan loyalty.

2.3.2 The Detrimental Effects of Partisan Loyalty

Partisan identity is crucial to understanding voting behaviour in American elections. It has been argued that: ‘no other single variable comes close to accounting as well or as consistently for American political behaviour.’⁴¹ Although this is not a study of the nature of partisanship, it is important to briefly discuss the different conceptualisations of partisan identification since this affects our understanding of the effect of partisan loyalty on the retrospective vote paradigm. The authors of *the American*

³⁹Healy and Lenz (2012); Bartels and Achen (2004); Achen and Bartels (2016)

⁴⁰A helpful overview of this work is provided by Kahneman himself in Kahneman (2003)

⁴¹Huddy and Aaroe (2015, p.1)

Voter described partisanship, or party identification, as a ‘psychological attachment’ to either one of the two major parties.⁴² This attachment was usually introduced in the family setting and developed over the course of someone’s lifetime due to the process of socialisation with various social groups. As mentioned, this view on partisan identification lead Fiorina (1981) to suggest that partisanship was: ‘a virus that you contracted from your parents at an early age.’⁴³ In contradiction to the view held by the authors of *the American Voter*, Fiorina (1981) instead introduced the idea that partisanship functioned as a: ‘running tally’, a disposition to one of the two major parties that is updated in light of political events and decisions. The main difference between these two conceptions is that the traditional notion of partisanship views partisan identities as stable over time, whereas the revisionist view holds that partisan preferences are updated in light of new information.

In the literature a division has been made between ‘expressive’ and ‘instrumental’ partisan identification, which broadly corresponds to the traditional and revisionist views.⁴⁴ Most importantly, expressive partisanship is thought to be rooted in identification with particular social groups and is consequently durable and resistant to change. Instrumental partisanship on the other hand is based on ideological or issue convergence between voters and parties and is thought to be more malleable and responsive to external events. Although empirical evidence is available to support either view of partisanship, little is known yet as to how these two different conceptualisations manifest themselves with the electorate. Studies in the field of psychology suggest that different types of group identification may lead to different types of behaviours.⁴⁵ Specifically, instrumental group identification often leads to competitive behaviour, whereas expressive group identification often leads to symbolic types of

⁴²Campbell et al. (1960)

⁴³Fiorina (1981, p.ix)

⁴⁴Huddy and Aaroe (2015); Green et al. (2002)

⁴⁵Scheepers et al. (2006)

behaviour that are aimed at the creation of positive identities of the self and the group. In line with this, the study by Lavine et al. (2012) on ambivalent partisans suggests that ambivalent and univalent partisan identification facilitate two different modes of decision-making: critical versus non-critical.

Nevertheless, to date, it is not clear why some voters appear to engage with parties in terms of their social identity and others do so on the basis of their ideological preferences. It is also not clear whether voters are able to switch between the two types of partisanship or whether both may exist at the same time and are given priority under different conditions. In light of the discussion at hand, it is important to note that few of the studies that focus on the negative effects partisanship exerts on retrospective voting specify the nature of partisan identification. Often however the bias that originates from partisan loyalty is described in terms of voters' desire to maintain a positive image of their own party.⁴⁶ This suggests that in many of these studies partisan identification is conceptualised in line with social identity theory and is expressive in nature.⁴⁷

Several studies have shown that partisanship acts as a filter against negative information about the in-party, the party a voter identifies with. A study by Bartels (2002) for instance finds that Democratic and Republican partisans provide strongly diverging answers when asked to evaluate objective economic indicators such as the size of the budget deficit or the level of unemployment. People are not just uninformed, rather, their errors in prediction are strongly correlated with their partisan preferences. Bartels (2002) thus argues that partisanship is *not* a running tally. Rather, the causal arrow runs in the opposite direction: partisan preferences *shape* the perception

⁴⁶For instance in Taber and Lodge (2006); Achen and Bartels (2016)

⁴⁷See Tajfel (1981) for social identity theory and Chapters 6 and 7 of this thesis for a further discussion of the types of partisan identification.

of facts, not the other way around. Other studies have made similar claims.⁴⁸

In another example of the detrimental effects of partisan identification, Gaines et al. (2007) examine the effect of partisan bias in relation to voters' views on the Iraq war, an event about which voters were well-informed. The study consists of a survey experiment among students and finds that both Democratic and Republican identifiers were able to accurately estimate the number of war-related fatalities. Yet, Democrats and Republicans differed strongly in their *interpretation* of these facts. Thus, Democratic partisans were much more likely to state that the number of fatalities was 'very high', whereas Republicans were much more likely to state the number was 'very low' - even though both groups provided a similar estimate. This suggests that even when people are able to perceive objective facts, their interpretation or evaluation of these facts might be severely biased.

Another important source of criticism of the retrospective vote paradigm can be found in the debate on the economic vote. Here too questions have been raised as to the direction of causality between partisan preferences, retrospective economic evaluations and incumbent support. In contrast to the economic vote literature as discussed above, these studies show that retrospective economic evaluations are strongly affected by voters' partisan priors. Using evidence on both the UK and the US, these studies find that the effects of the economic - retrospective - vote have been overstated. Voters do not develop objective, retrospective evaluations of economic performance. Instead, partisan preferences (almost) perfectly predict voters' economic opinions. These studies therefore conclude that partisan loyalty exerts both a direct as well as an indirect effect on vote choice and remains the main determinant of vote choice.⁴⁹

⁴⁸For instance Lenz (2012)

⁴⁹Evans and Andersen (2006); Evans and Pickup (2010). These studies have been disputed by others who argue that although partisan loyalty no doubt affects the formation of economic evaluations, the economy still exerts an independent effect on party choice, for these studies see Lewis-Beck

In order to explain the mechanism through which partisan bias operates, Taber and Lodge (2006) introduce a model of ‘motivated skepticism’ which describes the mechanism through which citizens process information. The authors conduct various experimental studies and offer evidence of both confirmation and disconfirmation bias. Put simply, voters are selective in the information they consume, spend more time reading information that confirms their prior beliefs and actively counter-argue information that contradicts their prior attachments. As a result of these processes, attitudes among citizens polarise. This pattern is strongest for voters who hold strong party affiliations and those with well-developed cognitive abilities.

2.4 The End of Democratic Accountability?

The question is now what this means for our understanding of electoral democracy and the functioning thereof. A contradiction exists: on the one hand, Achen and Bartels (2016) conclude that the current state of democratic theory is: ‘in a shambles’⁵⁰ and state that: ‘the retrospective model of democracy simply will not bear the normative weight that its proponents place on it.’⁵¹ As discussed however, authors on the other side of the debate have expressed more optimism and have shown themselves: ‘cautiously optimistic’ that voters are able to adhere to the principles of the retrospective vote paradigm.⁵²

Questions can be raised about both sides of the debate. First, with respect to the positive examples of retrospective accountability, it should be noted that the major-

(2006); Lewis-Beck et al. (2008); Nadeau et al. (2012); Lenz (2012)

⁵⁰Achen and Bartels (2016, p.306)

⁵¹Achen and Bartels (2016, p.16)

⁵²Citation from Malhotra and Kuo (2008, p.131). Also see Healy and Malhotra (2013) who similarly identify this conflict in the literature between their own work and that of those who are critical of the retrospective vote paradigm.

ity of these examples relate to evidence of voter responsiveness to either the national economy or natural disasters. These two types of government performance might be particularly salient to voters because they have a clear impact on their lives. Furthermore, in the case of natural disasters, the events are usually extensively covered by the media which makes it easier for voters to respond to them. Hence, government performance in these domains might be especially likely to result in a response from the electorate and it might therefore be unsurprising that we find the electorate responsive to such events. Although many studies discuss the fact that the retrospective vote paradigm should be studied across a broad range of performance areas, only few studies to date have done so.⁵³

With respect to the detrimental effects of partisan loyalty on political decision-making, these studies highlight a very important dimension of vote choice, especially in the US context where partisan identities are strong and salient. Yet, in some ways, the focus on partisan bias might hide more than it reveals. Recent studies on the endogeneity of partisan identification and economic evaluations for instance suggest that this association is moderated by the economic context. When voters receive consistently positive or consistently negative signals, the levels of motivated reasoning (or partisan bias) are small. When voters receive mixed signals, the effects of partisan bias are stronger.⁵⁴ Thus: ‘the information environment can facilitate or inhibit partisan rationalization.’⁵⁵ A recent example of this is the fact that governments across the ideological spectrum were punished in the wake of the financial crisis.⁵⁶ Finally, it is difficult to imagine how electoral swings occur without at least part of the partisan electorate changing their behaviour. Thus, although partisan bias is important to

⁵³Examples of studies that look at other domains de Vries and Giger (2014); Tilley and Hobolt (2011)

⁵⁴Parker-Stephen (2013); Dickerson (2016)

⁵⁵Parker-Stephen (2013, p.1078)

⁵⁶Chzhen et al. (2014); Bermeo and Bartels (2014); Dickerson (2016)

explain electoral stability, it does not bring us much further when trying to explain electoral change. Currently, little is known in the field of political science as to which partisans defect or why and when they do so.⁵⁷

In line with the opening quotation of this thesis, it thus seems important to develop a better understanding of: ‘the circumstances under which retrospective voting achieves effective democratic accountability and when it fails to do so.’⁵⁸ It seems unhelpful to characterize voters as ‘myopic stooges’, but it is equally clear that they are not the coolheaded, unbiased decision-makers that the traditional model of retrospective voting requires. In this thesis I thus assume that voters act as blind partisans at times, but not always. Often voters will be unable to break through the bubble of partisan news and opinions in which they find themselves. Yet, at other times, at least some partisan followers break out of this bubble and exercise accountability toward their preferred party or candidate. In order to explore the conditions under which these two processes occur, I focus my attention on situations in which partisan attachments are explicitly *challenged* by the performance of the incumbent party. Examples I will look at include economic malperformance and involvement in corruption. Partisans who are conflicted in this manner are referred to in this thesis as ‘cross-pressured partisans.’ Cross-pressured partisans have to engage in a difficult decision at election time: do they follow the party line, or do they hold their party to account? And if so, what actions do cross-pressured partisans take at election time?

⁵⁷Exceptions to this are the works of Key (1966); Lavine et al. (2012); Hillygus and Shields (2008) on party dissenters.

⁵⁸Healy and Malhotra (2013, p.285)

2.5 Ambivalent, Persuadable and Cross-Pressured Partisans

The concept of ‘cross-pressured’ partisans was first introduced in the 1940’s and 1950’s in studies of voting behaviour in the US.⁵⁹ In these studies, cross-pressure reflected a conflict between a voter’s various social identity groups. A voter was cross-pressured for instance when their religious group was affiliated with the Republican party whereas their work peers were members of the Democratic party. This type of cross-pressure is therefore also referred to as ‘social cross-pressure’. Voters who experienced this social cross-pressure were found to be less engaged with politics, presumably to avoid facing conflicting loyalties. More recently the concept of social cross-pressure has been studied with the use of social network analysis.⁶⁰ These analyses also suggest that partisans who experience social cross-pressure are less interested and engaged in politics and are less likely to vote.

In this thesis I do not focus on social cross-pressure, but rather extend the concept to include a conflict between the partisan group and retrospective performance evaluations of the incumbent. There are two recent studies that also extend the concept of cross-pressure to include other types of conflicts that partisans might face, these are *The Persuadable Voter* by Hillygus and Shields (2008) and *Ambivalent Partisans* by Lavine et al. (2012).⁶¹ Both these publications share the belief set out above that although the majority of partisans vote for the party they identify with at election time, this reveals little about the actual trade-offs that partisans strike between their loyalty toward the party and other information signals they receive. Most importantly, both studies show that there is a large segment of the partisan electorate that admits to holding *conflicting* opinions of both parties.

⁵⁹Berelson et al. (1954); Lazarsfeld et al. (1944)

⁶⁰Brader et al. (2014)

⁶¹Also see Basinger and Lavine (2005); Lavine (2001)

In *The Persuadable Voter*, Hillygus and Shields (2008) argue that although the American electorate appears highly polarised, there are many partisans who are conflicted between their attachment to a particular party and the issue positions the party holds. The study finds that more than 25% of voters in the 2004 presidential campaign held issue positions that did not correspond with the issue position of the party with which they identified. The study shows that these ‘wedge issues’ are an important tool for political parties to pry away voters from the opposition and are a source of vote-switching for partisans. In line with Hillygus and Shields (2008) I also focus on the conflict that may exist between a voter’s partisan loyalty and external information signals. Yet, an important difference between the work of Hillygus and Shields (2008) and the research presented here is that persuadable voters are voters who experience a conflict between their loyalty toward a party and the *issue positions* that a voter and the party hold. The work of Hillygus and Shields (2008) thus focuses on elections in terms of mandate representation but does not reflect on the accountability function of elections as I set out to do. I do not focus on conflicts between partisan identity and issue positions, but rather looks at conflicts between a voter’s partisan identity and the government’s economic performance, candidate competence and government malfeasance (corruption). In addition, the study of Hillygus and Shields (2008) focuses on the behaviour of *political parties* in light of wedge issues and sets out a theory of how parties use wedge issues and targeted campaigns to persuade voters. In contrast to this I focus on the behaviour of *voters* at election time.

The perspective of *Ambivalent Partisans* is broader in scope. Lavine et al. (2012) classify partisans as ambivalent, or ‘internally conflicted’, when: ‘identity and evaluation do not point in the same direction.’⁶² The study finds that up to half of the American

⁶²Lavine et al. (2012, p.4)

electorate reports ‘likes’ and ‘dislikes’ toward both parties and suggests that partisans are thus influenced by negative information signals about their own party or positive signals about the opposition. In line with the study on persuadable voters, the study on ambivalent partisans also finds that voters who experience conflicting signals are more likely to change their opinions and behaviour in response to this information. The study on ambivalent partisans corresponds closely to the perspective taken in this thesis. Yet, several differences exist between the study conducted by Lavine et al. (2012) and the research presented here.

First, Lavine et al.’s operationalisation of ambivalence is very broad and relies on the measurement of likes and dislikes toward both parties across eight different domains. These domains include economic performance and general government performance, but also include party traits, evaluations of party leaders, general ideology, foreign and social policy positions and general views of social groups that are linked to either of the two parties. Importantly, all these likes and dislikes are aggregated to one single measure of ambivalence.⁶³ As such, ambivalence covers a range of different types of conflicts between party loyalty and retrospective evaluations, but also issue or policy positions or party loyalty and leadership affect. Ambivalence therefore does not exclusively capture the tension between partisan loyalty and retrospective accountability.

The second main difference is that the study of ambivalent partisans focuses on the effect that internal conflict has on the nature of voters’ information gathering and decision-making. Thus, the study explores whether ambivalent partisans are more likely to seek out new information, whether ambivalent partisans make more consid-

⁶³Specifically, as explained in Lavine et al. (2012, chapter3) respondents are asked: ‘is there anything you like/dislike about party x? What is that? Respondents who mention 4 conflicting and 2 consistent likes and dislikes are classified as ambivalent partisans. Those who report consistent likes and dislikes are univalent partisans.

ered decisions, whether they are more accurate in their perceptions of politics and whether they are more receptive to campaign information. My focus, on the other hand, lies with the effect of internal conflict on *electoral behaviour at elections* and the consequences for democratic accountability of public officials.⁶⁴ In this thesis I do not present a model of information processing, but rather present a framework of cross-pressured voting across various elections.

2.6 A Framework of Cross-Pressured Voting

The central research question this thesis seeks to address is:

under what conditions (if any) do partisans hold their in-party accountable when faced with negative performance signals - and what form does this response take?

In order to explore this question, I present a framework of cross-pressured voting that is displayed below in Figure 2.1. The visual model consists of two parts, Part I (above the line) sets out the factors that are thought to affect the likelihood that a partisan voter is cross-pressured or not in light of the information signal they receive about the performance of the incumbent. Part II (below the line) sets out the expected behavioural response of cross-pressured partisans and identifies contextual factors that are thought to affect this behavioural response. I test the framework of cross-pressured voting in the empirical chapters that follow. As discussed in the introduction, the four empirical chapters cover a range of different types of cross-pressure and explore different electoral contexts in which partisans are cross-pressured. The empirical chapters rely on a combination of observational, quasi-experimental and

⁶⁴Note they do study elections in Chapter 6 but focus on the effect of ambivalence on the relative weight that voters attribute to partisan identity versus evaluative information when deciding who to vote for.

experimental analyses. In the sections below I introduce the hypotheses that are derived from the model which will be tested in the empirical chapters. An overview of which parts of the model are tested in the various empirical case studies is provided in Figure 2.2.

Figure 2.1: Framework of Cross-Pressured Voting

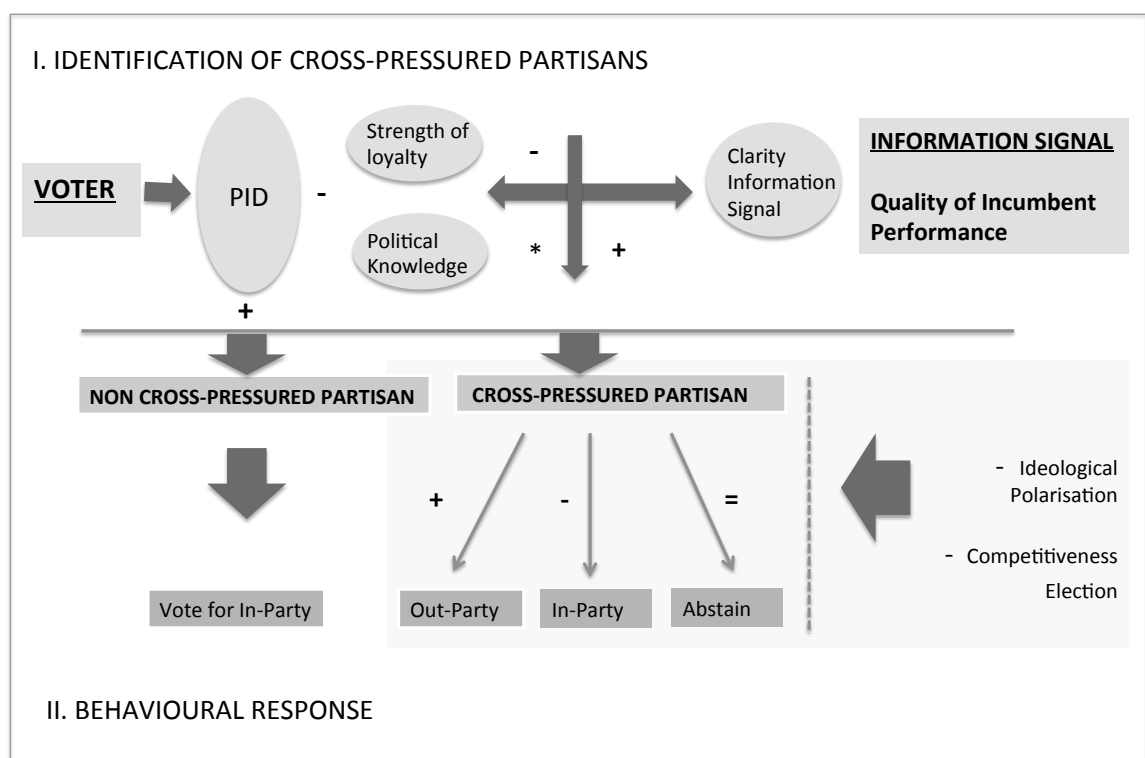
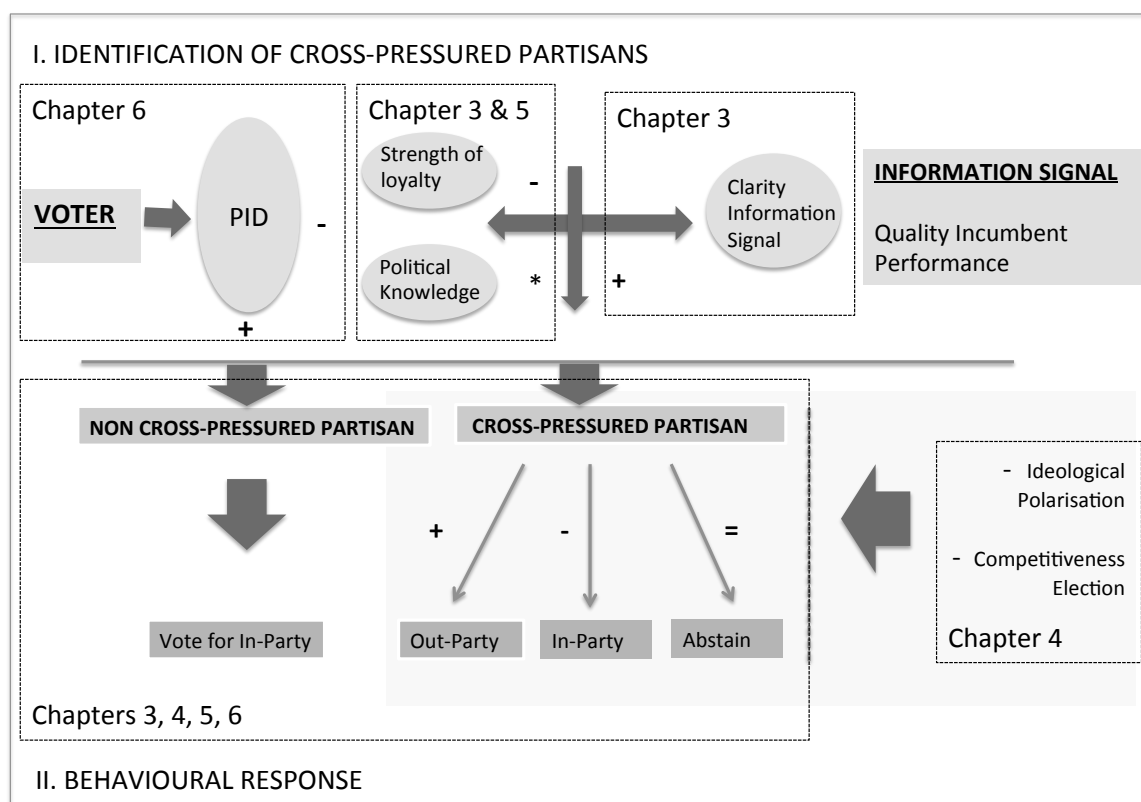


Figure 2.2: Thesis Chapters Related to the Framework of Cross-Pressured Voting



2.6.1 Strength of Partisan Affiliation

Starting at the top of Figure 2.1, the central idea of cross-pressure is visualised: a voter (left) is presented with external information about the performance of the incumbent government (right). From the perspective of the voter, there are several buffers that prevent the acceptance of this information, the most important of which is a voter's partisan identification. I identify two individual-level characteristics that I expect moderate the likelihood that a partisan voter is receptive to the information signal about the incumbent: (1) the *strength* of a voter's partisan affiliation and (2) the level of *political knowledge*, or political sophistication of the voter.

Rather intuitively, I expect that voters with *strong* levels of partisan identification will be *more* resistant to information about the performance of the incumbent that contradicts their prior held partisan beliefs and are hence *less* likely to be cross-pressured. As discussed in the Introduction and above, cross-pressured voters are voters whose partisan identification and retrospective performance evaluations of the incumbent do not align. Many studies have found that voters with strong attachments to a particular party are most resistant to change their opinions and attitudes about their preferred party.⁶⁵ This leads to my first hypothesis on the likelihood that a voter experiences cross-pressure:

Hypothesis 1: The strength of partisan identification moderates the likelihood that an individual voter experiences cross-pressure. Specifically, as the strength of partisan identification *increases* the likelihood of cross-pressure *decreases*.

⁶⁵For instance, this is shown to be the case by Bartels (2002); Taber and Lodge (2006); Hillygus and Shields (2008); Lavine et al. (2012); Achen and Bartels (2016); Gaines et al. (2007)

2.6.2 Political Knowledge

The second factor that I expect to affect the likelihood that a partisan voter experiences cross-pressure is the level of political knowledge of a voter.⁶⁶ Many studies have shown that levels of political knowledge affect whether or not voters receive information signals about incumbent performance and whether or not voters incorporate this information into their views on politics. Although almost all studies on the topic agree that levels of political knowledge affect the way in which voters process and respond to information about incumbent performance, disagreement exists as to the exact association.⁶⁷ Gomez and Wilson (2001) find for instance that voters with high levels of knowledge are more likely to hold governments accountable for changes in their personal welfare, whereas voters with low levels of political knowledge are more likely to exercise accountability in response to changes in the national economy. A study by Duch et al. (2000) finds the opposite however.

A model of the formation of mass opinion introduced by Zaller (1992) is helpful in untangling the complicated relationship between levels of sophistication or knowledge

⁶⁶It must be noted that different terms are used in the literature to reflect a voter's knowledge levels about the political process. The most commonly used terms are 'political knowledge' and 'political sophistication', although sometimes 'political interest' more broadly is used. Technically, political interest refers to the interest shown by an individual in politics. This thus covers a broad range of behaviours and is measured in variety of ways. Examples are measures that capture the level of news consumption of an individual, but also a voter's self-reported interest in politics, factual knowledge of politics or voters' participation or engagement in political events such as attending rallies or meetings. Political sophistication on the other hand is an often-used but somewhat more elusive term which usually refers to both a voter's knowledge of politics and their understanding of politics as a process. Political knowledge is perhaps the easiest to measure and refers to a person's factual knowledge of politics. Existing research on different measurements suggests that the use of a measure of factual political knowledge is preferable because it is less susceptible to bias when compared to self-reported measures of interest or engagement. This approach is advocated by Price and Zaller (1993) and discussed also in Stoker (1993). As such, I use the term 'political knowledge' in my model and, where possible, use facts-based questions about politics to ascertain a voter's level of knowledge of political affairs. Due to data limitations it is not always possible to use a measure of political knowledge that is based on facts-based questions about politics. I discuss these considerations in the relevant chapters.

⁶⁷See de Vries and Giger (2014) for a discussion of the literature on sophistication and for evidence of the fact that political sophistication is an important moderator of the attribution of responsibility.

and the potential for cross-pressure. Zaller’s model relies on two important components: awareness and resistance. In principle, people with higher levels of knowledge of politics are expected to be *more* receptive to new information compared to those with low levels of knowledge. This is true partially because people with high levels of knowledge are more likely to receive information signals and partially because people with high levels of knowledge are more likely to understand this information. As a consequence, we might expect that partisans with high levels of political knowledge are *more* likely to be cross-pressured. Yet, as the second element of Zaller’s model indicates: as political knowledge increases, so does the potential for *resistance* to received information. Consequently, there might thus be a *curvilinear* association between political knowledge and the likelihood of cross-pressure where voters with *low* and *high* levels of knowledge of politics are expected to be *less* likely to be cross-pressured compared to those with moderate levels of knowledge: ‘the former because of their lower degree of exposure, and the latter because of their greater resistance to new information.’⁶⁸ This mechanism has been shown to hold true both in real-life settings as well as in experimental studies.⁶⁹ I thus expect a curvilinear association between political knowledge and the likelihood of cross-pressure. This is formalized below in Hypothesis 2 and indicated in Figure 2.1 with an ★:

Hypothesis 2: Partisans with *low or high* levels of political knowledge are expected to be *less* likely to be cross-pressured compared to partisans with *moderate* levels of political knowledge.

⁶⁸Stoker (1993, p.197)

⁶⁹Stoker (1993); Taber and Lodge (2006); Gaines et al. (2007) For instance, in their study of ‘motivated skepticism’, Taber and Lodge (2006, p.757) find that participants with high levels of cognitive abilities were better able to counter-argue information that contradicted their prior partisan attachments because they: ‘possess greater ammunition with which to counter-argue incongruent facts.’

2.6.3 The Clarity of the Performance Signal

The final factor that I explore with respect to the identification of cross-pressured partisans is displayed on the right-hand side of Figure 2.1: the clarity of the information signal about incumbent performance. I expect that the number of cross-pressured partisans will be higher when there is a clear signal about incumbent performance. Put simply, voters are thought to be more likely to respond to an information signal (i.e. experience cross-pressure) when the signal about incumbent performance is unambiguous and is either clearly positive or clearly negative. Studies on the economic vote clearly document the fact that voters are more responsive to information about incumbent performance when lines of responsibility for economic performance are clear as opposed to complex or ambiguous.⁷⁰ In the framework of cross-pressured voting that I present I do not focus on lines of responsibility, but I similarly expect that unambiguous signals are easier to receive for voters and that voters are more likely to respond to these signals.

Research on the recent economic recession of 2008, when voters received a clearly negative signal, suggest that this is indeed the case.⁷¹ Furthermore, two recent studies by Dickerson (2016) and Parker-Stephen (2013) both show that the association between retrospective economic evaluations and partisan preferences is moderated by the economic context. These studies find that:

an environment with facts moving in opposite directions will *facilitate* partisan disagreement and an environment with facts moving in a single direction will *inhibit* disagreement.⁷²

As a result: ‘voters become more likely to experience similar conflicts between their

⁷⁰Powell and Whitten (1993); Hobolt et al. (2013)

⁷¹Bermeo and Bartels (2014); Chzhen et al. (2014); Dickerson (2016)

⁷²Parker-Stephen (2013, p.1078)

partisan identities and their evaluations of government performance' when information is unambiguous.⁷³ This conflict might be facilitated both by the fact that unambiguous signals are easier to understand, but also by the fact that unambiguous signals are likely to reflect the fact that voters either experience clearly positive or clearly negative effects of the performance of the incumbent. Thus, voters might respond strongly to an economic recession both because they are aware of the recession on the basis of media reporting, or because they themselves experience the negative consequences directly. The opposite pattern also appears to be true, research shows that partisan bias is strongest when economic performance is 'middling', as opposed to being either clearly good or bad.⁷⁴ I thus hypothesize that the number of cross-pressured voters will be higher when there is a clearly positive or a clearly negative signal about incumbent performance:

Hypothesis 3: The number of cross-pressured partisans will be higher when the performance signal is *clear* as opposed to ambiguous and conveys either clearly negative or clearly positive information about the performance of the incumbent.

2.6.4 Voting Behaviour of Cross-Pressured Partisans

The second aim of the model is to explore the behavioural response of cross-pressured partisans, this is depicted below the line in Figure 2.1. Put simply, do cross-pressured partisans act differently at election time compared to non cross-pressured partisans? My expectation is that they do. As can be seen from Figure 2.1, I expect that *non* cross-pressured partisans vote *in line* with the party they identify with. Based on the information that is available to them, there is little reason to do otherwise. Cross-pressured partisans however, have three potential behavioural responses: they can

⁷³Dickerson (2016, p.1044)

⁷⁴Healy and Malhotra (2013, p.293) Also see Parker-Stephen (2013); Dickerson (2016)

vote for the in-party, they can vote for the out-party, or they can abstain. The in-party refers to the party that shares the voters' partisan identification, the out-party here refers to the party that does *not* share the voter's partisan identification. Thus, for a Republican voter, the Republican party is the in-party and the Democratic party is the out-party. The reverse is true for a voter who identifies with the Democratic party.

If cross-pressured partisans vote for the out-party, then this is a clear example in which partisans exercise accountability toward their preferred party. Thus, if I find that cross-pressured partisans are *more likely* to change their vote choice compared to non cross-pressured partisans, this suggests that cross-pressured partisans hold their in-party accountable in light of the information signal they receive. Abstention is a more complex behavioural response and one that is traditionally difficult to study due to issues of under-reporting.⁷⁵ Mostly, abstention is seen as a sign of disengagement from politics.⁷⁶ Yet, it has been argued that:

abstention can be understood not as the result of indifference or alienation, but rather as a deliberate means to partially punish or reward a coalition. If a voter suffers high expressive costs from voting for the other coalition, abstention becomes a rational middle-way solution, especially if his/her ideologically preferred coalition is the incumbent, and has not performed as well as expected.⁷⁷

Interpreted this way, abstention could also be a way in which partisans protest against their preferred party without actively supporting the out-party. It is not really possible however to untangle the *intention* of voters when they abstain using observational

⁷⁵For an insightful study on abstention in relation to the economic vote see Tillman (2008) and for a helpful discussion on the functions that abstention may serve Fauvelle-Aymar et al. (2000)

⁷⁶As in the traditional works on cross-pressure Lazarsfeld et al. (1944); Berelson et al. (1954) but Brader et al. (2014) also interpret it in this manner.

⁷⁷Fauvelle-Aymar et al. (2000, pp.410-411)

data. I can therefore only observe the outcome: abstention, but not the motivation behind this behaviour.

The existing literature on cross-pressured partisans presents two contradictory expectations as to their behaviour at election time. On the one hand, the traditional studies that focus on social cross-pressure find that cross-pressured partisans are disengaged from politics and are less likely to vote at election time.⁷⁸ The revisionist studies (*Persuadable Voters* and *Ambivalent Partisans*), which focus on cross-pressure in a broader context, find the opposite. Persuadable and ambivalent partisans are no less likely to vote compared to their non-ambivalent counterparts. With regard to party choice, Hillygus and Shields (2008) find that partisans who hold an issue-conflict toward their in-party are *more likely* to vote for the opposition at election time as a result. Lavine et al. (2012) find that voters who experience a mixture of likes and dislikes, and hence are ambivalent, are more likely to seek out new information and more likely to update their views accordingly. The authors also show that ambivalent partisans weigh performance information more strongly at election time, at the expense of partisan concerns. In light of these findings, I expect that cross-pressure between partisan loyalty and performance evaluations will have a similar effect to ambivalence and persuadability and I thus develop the following hypotheses on the behaviour of cross-pressured partisans:

Hypothesis 4: Cross-pressured partisans will be *equally* likely to abstain from voting compared to non cross-pressured partisans.

Hypothesis 5: Cross-pressured partisans will be *less* likely to vote for their in- party and *more* likely to vote for the out-party compared to non cross-pressured partisans.

⁷⁸Lazarsfeld et al. (1944); Berelson et al. (1954); Brader et al. (2014)

Hypotheses 4 and 5 are essential to answer the main research question set out in this thesis because they test whether or not voters actually exercise accountability toward their preferred party in light of contradictory information signals about the party’s performance in office. I therefore test these two hypotheses in all empirical chapters.⁷⁹ It is important to note that in the hypotheses I compare cross-pressured partisans with *non* cross-pressured partisans. In line with other studies on cross-pressure I thus focus exclusively on partisans and do not include independents or non-partisans.⁸⁰

2.6.5 Ideological Polarisation

The final two hypotheses relate to the electoral context in which cross-pressured partisans find themselves. This is depicted below the line on the right-hand side of Figure 2.1. In line with existing research, I assume that voters do not vote exclusively on the basis of their individual-level motivations, but rather *strategically* respond to the electoral context in which they vote.⁸¹ This behaviour is most apparent in multi-party systems when voters elect coalition governments and hence try to affect the formation of a particular coalition with their vote.⁸² Yet, voters in two-party systems also strategically interact with the political environment in which they find themselves.⁸³ Existing research finds that voters in US elections are aware of the *partisan stakes* of the election in which they vote and act accordingly. A study on House elections in the US finds for instance that the ideological distance between incumbents and challengers affects how much weight voters attribute to other candidate qualities such as competence and valence.⁸⁴ The study shows that voters are willing to overlook a

⁷⁹With the exception of the final empirical chapter in which I only test Hypothesis 5.

⁸⁰Lavine (2001); Hillygus and Shields (2008) take a similar approach. I discuss the role of Independents in the Discussion Chapter of this thesis, Chapter 7.

⁸¹For an overview see Alvarez and Nagler (2000)

⁸²See for instance Meffert and Gschwend (2010); Bowler et al. (2010)

⁸³Abramson et al. (2009) for instance find that the behaviour of voters in PR and FPTP systems is remarkably similar.

⁸⁴Buttice and Stone (2012) and also Stone et al. (2010) who show that candidates with a valence advantage tend to take ideological positions that are close to the district preference. Candidates

candidate's lack of competence if the ideological distance between the two candidates is very large. Similar evidence exists for elections in the UK where voters were less likely to punish candidates who were involved in an expenses scandal if they lived in districts where a Labour and Conservative candidate competed. Voters were *more* likely to respond to the expenses scandal when they lived in districts where Labour or Conservative candidates faced Liberal-Democratic candidates, who were perceived to be ideologically more similar.⁸⁵

I thus expect that cross-pressured partisans are *less* likely to hold their in-party accountable for potential performance failures if the partisan stakes of the election are very high. I measure partisan stakes through the level of ideological polarisation between candidates. Put simply, if candidates are ideologically very dissimilar, then the partisan costs of voting for the out-party are very high and consequently I expect partisans to be less likely to vote for the out-party under these conditions, even when they receive information that puts their in-party in a negative light. I thus hypothesize that:

Hypothesis 6: Higher levels of ideological polarisation will have a *negative* effect on the behavioural response of cross-pressured voters, such that these voters will be *less* likely to vote for the out-party compared to cross-pressured voters who vote in elections with low levels of ideological polarisation.

with a strong valence disadvantage may try compensate for this by taking extreme positions which allow them to gather activist support.

⁸⁵Eggers (2014)

2.6.6 Competitiveness of an Election

In addition to the level of ideological polarisation, the partisan stakes of an election are also increased if the electoral race is very *competitive* and hence the in-party candidate has a high chance of losing the election. In this scenario, voters realise that their individual vote might be highly consequential. Surprisingly little research exists that explores the effects of the competitiveness of an election on the behaviour of voters. Often, studies focus on the ‘paradox of voting’ and explore why people vote considering the fact that an individual vote is rarely consequential. Yet, there are clear situations in which individual votes *do* matter, especially in elections at the local level where the electorate is much smaller and the potential effect of a single vote increases. Also, many elections in the US are decided by small vote margins, suggesting that small electoral swings can have a large impact.

Acknowledging this, several studies explore the effect of electoral competitiveness on the behaviour of governments, parties and candidates. These studies find that parties and governments alter their ideological positions if they expect an election to be very close.⁸⁶ Specifically, parties move to an ideological position they think will maximise the potential number of votes they can receive. For larger, or mainstream, parties, this means the party moves to the centre to capture the median voter. For fringe parties, this means the party moves to the extreme to capture partisan hardliners.⁸⁷ If parties and candidates take the perceived competitiveness of an election into account, then we might expect voters to do the same. Specifically, if an election is thought to be highly competitive, voters realise that the chances that their in-party candidate will

⁸⁶For a study on US House candidates see Lindgren and Southwell (2014). Hobolt and Klemmensen (2008) furthermore show that executives are more responsive to the policy preferences of the public if they expect the next election to be highly competitive. This supports the idea that there is an interaction between the competitiveness of an election and the policy or ideological agenda of parties and governments.

⁸⁷Chadi-Abou and Orlowski (2016)

lose are high. I expect that voters prioritise their partisan preferences under these conditions and are less likely to vote against their own party in a competitive race, regardless of cross-pressured signals:

Hypothesis 7: Higher levels of electoral competitiveness will have a *negative* effect on the behavioural response of cross-pressured voters, such that these voters will be *less* likely to vote for the ‘out’-party compared to cross-pressured voters who vote in elections with low levels of competitiveness.

In conclusion, Table 2.1 presents an overview of the hypotheses that are tested in the chapters that follow. Together the empirical case studies explore the conditions under which partisans hold their in-party to account and when they fail to do so. The final chapter of the thesis offers a discussion of the main findings and reviews these in light of the expectations set out in this chapter as well as the broader literature on voting behaviour in the US.

Table 2.1: Overview of Hypotheses per Thesis Chapter

Identification of Cross-Pressured Partisans	
H1: As strength of party identification <i>increases</i> , the likelihood of cross-pressure <i>decreases</i>	
H2: Partisans with <i>low</i> or <i>high</i> levels of knowledge are less likely to be cross-pressured compared to those with <i>moderate</i> levels of knowledge	
H3: The number of cross-pressured partisans is higher when the information signal is clearly positive or clearly negative as opposed to ambiguous	
Behavioural Response of Cross-Pressure Voters	
H4: Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans	
H5: Cross-pressured partisans are more likely to vote for the ‘out’ party compared to non cross-pressured partisans	
Contextual Effects on the Behaviour of Cross-Pressured Partisans	
H6: High levels of ideological polarisation <i>reduce</i> the likelihood that a cross-pressured partisan votes for the out-party	
H7: High levels of competitiveness <i>reduce</i> the likelihood that a cross-pressure partisan votes for the out-party	
Empirical Case Study	
Ch 3 - Economic Cross-Pressure in 1992, 2000, 2004 Presidential Elections	H1, H2, H3, H4, H5
Ch 4 - Candidate Cross-Pressure in 2006 Midterm Elections	H4, H5, H6, H7
Ch 5 - Watergate Scandal and the 1974 Midterm Elections	H1, H2, H4, H5
Ch 6 - Lab Experiment on Corruption	H5

Chapter 3

Economic Cross-Pressure in the 1992, 2000 and 2004 Presidential Elections

To support the Ins when things are going well; to support the Outs when they seem to be going badly, this, in spite of all that has been said about tweedledum and tweedledee, is the essence of popular government

- Walter Lippmann (p.126,1925) *The Phantom Public* -¹

3.1 Introduction

In this first empirical case study I test the framework of cross-pressured voting in three different US presidential elections, those of 1992, 2000 and 2004. Throughout the chapter I focus on cross-pressure in the economic domain. Thus, a voter is cross-pressured when they identify with either the Democratic or Republican Party, but report retrospective evaluations of the president's handling of the national economy that *contradict* these partisan loyalties. In the chapter I explore both Part I and Part II of the framework of cross-pressured voting, displayed in Figure 3.1. I thus seek to first identify whether there are any individual level characteristics that increase

¹As cited in Achen and Bartels (2016, p.90)

the likelihood that a partisan voter reports conflicting retrospective economic evaluations. Second, I explore the behaviour of cross-pressured partisans across these three election cycles.

The chapter proceeds as follows. First, in section 3.2 I briefly discuss the existing literature on the economic vote, specifically the relationship between partisan identification and economic voting. I then introduce the data used for the analyses in section 3.3. This is followed by the main results in section 3.4. As a first step, I explore whether partisan identifiers actually update their economic evaluations in line with economic reality. I find that they do. I then examine whether the number of cross-pressured partisans is larger when the economy is performing either very poorly or very well. I then test whether there are any individual level characteristics that increase the likelihood that a partisan voter is receptive to information about the economy that contradicts their partisan identification. In keeping with the theory of cross-pressured voting that is central to the thesis, I focus specifically on the strength of partisan affiliation and levels of political sophistication. Finally, I explore the effect of cross-pressure on actual voting behaviour, both its effects on turnout and vote choice. The final section 3.5 discusses the main findings in light of the overall research question posed in this thesis. The chapter tests hypotheses 1 through 5 as set out in the previous chapter (Chapter 2). An overview of the hypotheses that are tested in this chapter is provided in Table 3.1.

Figure 3.1: Framework of Cross-Pressured Voting - Chapter 3

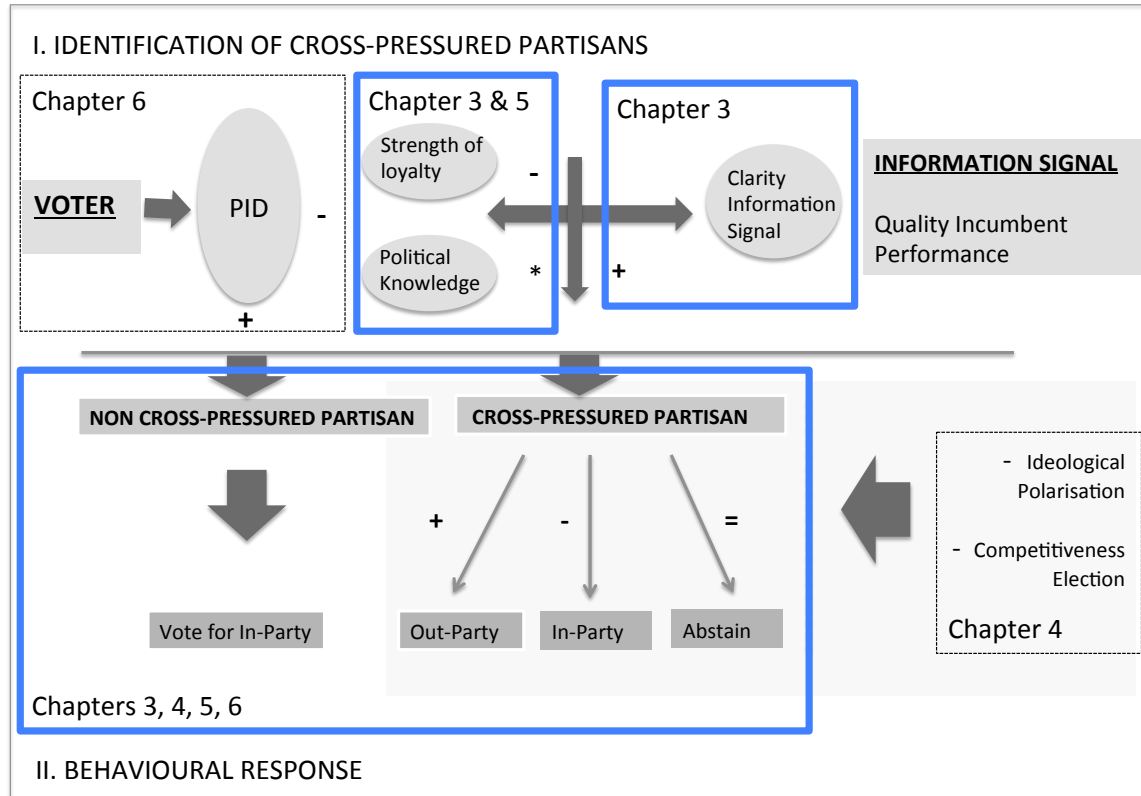


Table 3.1: Hypotheses Tested in Chapter 3

Identification Cross-Pressured Partisans
H1: As strength of party identification <i>increases</i> the likelihood of cross-pressure <i>decreases</i>
H2: Partisans with <i>low</i> or <i>high</i> levels of knowledge are less likely to be cross-pressured compared to those with <i>moderate</i> levels of knowledge
H3: The number of cross-pressured partisans is higher when the information signal is clearly positive or clearly negative
Behavioural Response of Cross-Pressured Partisans
H4: Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans
H5: Cross-pressured partisans are more likely to vote for the out-party compared to non cross-pressured partisans and less likely to vote for the in-party

3.2 Cross-Pressured Partisans and The Economic Vote

3.2.1 The Economic Vote

The principle assumption underlying the retrospective vote paradigm is the fact that voters punish and reward politicians for their accomplishments in office. The main domain in which this mechanism has been studied is the national economy. Studies on the economic vote explore whether voters punish politicians for poor economic performance and reward them for economic prosperity.² The interaction between partisanship and economic voting is a widely debated topic. Disagreement exists in both the US context and beyond whether party preferences or economic performance matter most to vote choice and if economic evaluations exert an independent effect on vote choice or whether they are mere partisan proxies.³

A review of the field of economic voting suggests that, in general, voters punish governments in poor economic times.⁴ Specifically, voters tend to respond to national level economic developments such as employment figures and inflation. Voters also tend to respond more strongly to negative as opposed to positive economic developments.⁵ One of the most pressing puzzles in the field is the fact that the size of the economic vote varies across both time and countries. Various explanations for the variability of the economic vote exist. Electoral institutions and the structure of government, the so-called ‘clarity of responsibility’ or ‘clarity of governance’, affect

²The literature on the economic vote is extensive, see for a helpful overview of the field Lewis-Beck and Paldam (2000)

³For a discussion on which element matters more to vote choice see Alvarez and Nagler (1995, 2000); Blais et al. (2004). For a discussion on the endogeneity of economic evaluations see Evans and Andersen (2006); Evans and Pickup (2010); Lewis-Beck (2006); Lewis-Beck et al. (2008); Pickup and Evans (2013); Nadeau et al. (2012); Chzheng et al. (2014); Dickerson (2016); Becher and Donnelly (2013)

⁴Lewis-Beck and Paldam (2000) also see Bermeo and Bartels (2014); Chzheng et al. (2014); Nadeau et al. (2012); Duch and Stevenson (2008); Dickerson (2016)

⁵Lewis-Beck and Paldam (2000)

voters' ability to attribute responsibility to government officials and moderate the signal about the economy that voters receive.⁶ Electoral institutions and particularly the number of parties that compete furthermore affect voters' ability to act in response to economic signals.⁷ Finally, the response to national economic performance is dependent on the way in which voters select and receive information.⁸ Both institutional and individual level factors thus affect voters' ability to hold elected officials to account for their handling of the national economy.⁹

The main individual level obstacle to achieve democratic accountability in the economic domain is thought to be a voter's partisan identification. As discussed in the previous chapter, party affiliation affects all steps of the accountability mechanism: the selection, reception and interpretation of information as well as the attribution of responsibility.¹⁰ Thus, partisans tend to focus on information that confirms their prior beliefs and ignore or downplay information that contradicts their partisan views. Furthermore, partisans tend to attribute positive economic outcomes to their in-party, but neglect to do this for negative economic outcomes. In light of these findings, it has been suggested that the effects of the economic vote have been over-estimated. Several studies on the US and UK suggest that retrospective economic evaluations are strongly influenced by partisan views. When this partisan bias is accounted for, the effect of retrospective economic evaluations disappears.¹¹ Other authors have rejected this view and hold that economic evaluations *do* exert an independent effect on vote choice.¹² It seems difficult to settle this question, but it is clear that it is

⁶Powell and Whitten (1993); Hobolt et al. (2013)

⁷Schleiter and Voznaya (2012); Tillman (2008)

⁸Gerber and Huber (2010); Taber and Lodge (2006)

⁹See Hobolt et al. (2013) who show that government clarity also conditions the accountability mechanism in other policy domains such as healthcare.

¹⁰Evidence of these mechanisms are is Taber and Lodge (2006); Tilley and Hobolt (2011); Gaines et al. (2007); Gerber et al. (2010); Marsh and Tilley (2009)

¹¹Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013)

¹²Lewis-Beck (2006); Lewis-Beck et al. (2008)

important to account for the fact that retrospective economic evaluations are to some extent affected by voters' partisan preferences.

Although much has been written about the interaction between partisan preferences and economic evaluations, the literature on this topic focuses almost exclusively on the *alignment* of party affiliation and national level economic evaluations. Consequently, much less is known as to how voters deal with a situation in which their partisan preferences and their evaluations of the state of the national economy *conflict*. This potential conflict is interesting for multiple reasons. First, as discussed in Chapter 2, existing studies on conflict between partisan preferences and issue positions suggests that the number of conflicted partisans is much more substantive than is oftentimes thought. Upwards of 25% of the electorate has been found to support issue positions that contradict their party affiliation for instance.¹³ Second, although it is easy to predict the behaviour of partisans whose economic evaluation *align*, this is more difficult for voters whose economic evaluations *contradict* their partisan viewpoints. Cross-pressured partisans might be instrumental in explaining electoral swings and are potentially important safe-keepers of democratic accountability.¹⁴

In this chapter I thus focus on voters for whom their partisanship and their evaluations of the president's handling of the economy conflict rather than align. In other words, how does a Republican voter respond if an incumbent Democratic president has presided over a period of strong economic growth? Similarly, how does this voter respond to a period of economic decline under a Republican president? Through the study of these cross-pressured partisans I aim to come to a more complete understanding of the conditions under which the accountability mechanism of elections

¹³Hillygus and Shields (2008)

¹⁴See Lavine et al. (2012) who portray ambivalent partisans as acting according to the principles of normative democracy.

fulfills the expectations that are placed upon it by the theory of retrospective voting and when it fails to do so.

3.3 Data

The analyses presented in this chapter rely on two separate panel studies: one that covers the period 1990-1992 and a second that covers the period 2000-2004. Note that I only focus on panel respondents and thus exclude respondents who were added to the panels as cross-sectional additions.¹⁵ There are several reasons to use this data. First, the panel studies cover both periods of economic growth (2000) as well as economic decline (1992) and an economically more moderate period (2004).¹⁶ Other research has shown that partisan bias *decreases* when the economic signal is more clear and this data offers the opportunity to study both types of settings.¹⁷ Second, the panel studies span both Democratic and Republican periods of governance and thus offer the ability to study positive and negative cross-pressure for both Republican and Democratic voters. Third, the panel studies are helpful from a methodological perspective. Taking into account the fact that there are important concerns as to the endogeneity of partisan identity and economic evaluations, as well as with the updating of partisanship in response to economic information, the panel nature of the data offers the opportunity to conduct robustness checks for the analyses presented below.¹⁸ Finally, I use the two panel studies as a check on each other and explore whether I find similar answers to my hypotheses across three different presidential election cycles (1992, 2000, 2004).

¹⁵For the 1990-1992 panel this results in $N = 748$, for the 2000-2004 panel $N = 985$.

¹⁶See Healy and Malhotra (2013), Campbell (2005), Parker-Stephen (2013) and Alvarez and Nagler (1995) for similar characterisations of these election years.

¹⁷Chzhen et al. (2014); Parker-Stephen (2013); Dickerson (2016). Also see Healy and Malhotra (2013) Figure 1, p.291 which shows decreased partisan bias in economic moderate times.

¹⁸See for these criticism Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013)

The main explanatory variable of interest is cross-pressure. The variable is constructed using two separate survey questions: one on partisan identification and one on evaluations of the president's handling of the economy. Partisan identification reflects the standard 7-point identification scale. This allows me to distinguish between weak, moderate and strong partisan affiliates. Note that in the analyses presented in this chapter I *exclude* Independents because my main interest lies with the difference between cross-pressured and non cross-pressured partisans. This is in line with other studies on the topic.¹⁹

Studies on economic voting use a range of different survey questions to capture voters' responses to economic indicators. Some rely on real economic indicators, whereas others use voters' perceptions of these indicators, or voters' more direct evaluation of the president's performance.²⁰ In this thesis I follow the latter approach and use the question: "Do you approve or disapprove of the way President X is handling the economy?" I use this question because I am specifically interested in the direct attribution of responsibility of economic performance to the incumbent president. This is different from asking voters their opinion on the economy generally, which does not necessarily result in the attribution of presidential responsibility. Rather, a voter might hold a negative opinion about the state of the national economy but avoid cross-pressure by attributing responsibility of the state of the economy not to the president but to other (external) actors or conditions.²¹ As such, I use the more di-

¹⁹See Hillygus and Shields (2008); Lavine et al. (2012) Note I only exclude 'true' independents. These are voters who state they are 'independent' in the initial 3-category question and again state to be 'independent' in the follow-up question whether they have a partisan leaning. In 1992 11.7% of panel respondents identified as Independent, in 2000 this was 15.1% and in 2004 14.5% of the panel respondents identified as independents. Please refer to the Discussion chapter of this thesis for a brief discussion of Independents and retrospective voting.

²⁰Lewis-Beck and Paldam (2000)

²¹See Tilley and Hobolt (2011) who draw the distinction between selective evaluation and selective attribution and show that partisanship actually exerts a stronger effect on attribution when compared to evaluation.

rect survey question on presidential economic performance which asks voters directly about the president's competence in handling the economy.

Both partisan identification and economic evaluations are recorded in the pre-electoral waves of the survey. For the behavioural analyses below, vote choice and turnout (the dependent variables) are recorded in the post-electoral wave. An overview of the operationalisation of the cross-pressure variable is provided below in Table 3.2. Republican partisans are cross-pressured when they provide a negative evaluation of George H.W.Bush in 1992 or of George W. Bush in 2004 and when they provide a positive evaluation of Bill Clinton in 2000. Democratic partisans are cross-pressured when they provide a negative evaluation of Bill Clinton in 2000, or a positive evaluation of either George H.W. Bush or George W. Bush.

Table 3.2: Operationalisation Economic Cross-Pressure

Partisanship Voter	1992 Survey George H.W Bush Evaluation Economic Performance	Cross-Pressured
Republican	Positive	No
Republican	Negative	Yes
Democrat	Positive	Yes
Democrat	Negative	No
2000 Survey Bill Clinton Evaluation Economic Performance		
Republican	Positive	Yes
Republican	Negative	No
Democrat	Positive	No
Democrat	Negative	Yes
2004 Survey George W. Bush Evaluation Economic Performance		
Republican	Positive	No
Republican	Negative	Yes
Democrat	Positive	Yes
Democrat	Negative	No

3.4 Results Part I: Who Are the Cross-Pressured Partisans?

In this section I do four things: first, I explore the stability and fluidity of both partisan identification and economic retrospections in the two panel studies. Second, I test whether there are in fact partisans who report economic evaluations of the president's handling of the economy that contradict their partisan loyalties and whether the number of cross-pressured partisans is higher when the economic signal is unambiguous (H3). Third, I test whether partisans with strong levels of party affiliation are less likely to be cross-pressured (H1). And fourth, I test whether there is an association between cross-pressure and levels of political knowledge (H2).

3.4.1 Partisan Identification and Economic Evaluations: Stability and Change

As a first descriptive step I explore the stability (or fluidity) of partisan identification and economic evaluations in the two panel studies (1990-1992 and 2000-2004). I do this to test whether partisans actually *alter* their economic evaluations of the president's economic performance over time or whether these economic evaluations are static. Table 3.3 presents the percentage of panel respondents that identifies with the same party in both panel waves. As expected, partisan identification is highly consistent over time in both panel studies. In the 1990 panel-wave, 380 respondents identified with the Republican party. Table 3.3 shows that in 1992 80% of these 380 respondents again identified with the Republican party. For Democratic identifiers the percentage of consistent identifiers is 86.3% in this period.²² The number of consistent identifiers is even higher in the panel that covers the period 2000-2004: 89.3% for Democratic partisans and 90.6% for Republican partisans. This confirms

²²With 519 respondents who identified with the Democratic party in 1990.

that the vast majority of panel respondents hold stable partisan identifications.

Table 3.3: Stability Partisan Identification in Panel Studies

	1990-1992	2000-2004
Consistently Identifies with Republican Party	80%	90.6%
Consistently Identifies with Democratic Party	86.3%	89.3%

In terms of the development of the US economy in the two periods under study (1990-1992 and 2000-2004), the year 1992 has been referred to as: ‘the end of the worst four-year stretch of economic performance.’²³ As such, if members of the panel responded accurately to the economic developments at the time, they should either consistently offer negative evaluations of the president’s handling of the economy in the 1990 and 1992 panel waves, or move from positive to negative evaluations in this period. The period 2000-2004 is more difficult to assess because there was a less clear cut economic development. In general however, the year 2000 is seen to mark the end of a successful stretch of economic stewardship of President Clinton.²⁴ If economic evaluations follow the economic reality I thus expect that panel respondents are relatively optimistic about the president’s economic performance in the 2000 survey wave. With respect to the 2004 elections, the image is more muddled. A study by Campbell (2005) that compares election-year GDP growth across more than six decades concludes that: ‘the economic fundamentals of the 2004 elections might be best described as neutral or tilting slightly in favour of the incumbent.’²⁵ I thus expect economic evaluations to be relative stable between the 2000 and 2004 panel waves.

²³Alvarez and Nagler (1995, p.715)

²⁴For instance see Zaller (1998) who references this as one of the main reasons that Clinton was largely unaffected by the Clinton-Lewinsky scandal. Also see Table 1 in Campbell (2005, p.78) which shows that the second quarter GDP growth in 2000 was among the top 5 of growth rates in the period 1948-2004.

²⁵Campbell (2005, p.79)

Table 3.4 largely confirms these expectations. Most importantly, there is a clear shift toward negative economic evaluations between the 1990 and 1992 survey. This is important because panel respondents evaluate the performance of Republican President George H.W. Bush in both panel waves. Thus, despite the fact that respondents evaluate a Republican president at both points in time, there is a clear change in their evaluations of his economic performance. Specifically, in the period 1990-1992 disapproval of George H.W. Bush's economic performance increases from 54% to 78%. This shift is in line with the economic reality. Also in line with economic reality, a large majority of panel respondents, 72%, is positive about the performance of Democratic President Bill Clinton in 2000, this number seems to defy partisan polarisation. In 2004 the economic evaluations middle out to approximately 50% of the panel approving of economic performance and 50% disapproving. This suggests that panel respondents align their economic evaluations with their partisan preferences in light of the change over in power between 2000 and 2004.

Table 3.4: Stability Economic Evaluations in Panel Studies

	1990	1992	2000	2004
Approve of Economic Performance	40.5%	20.5%	72.3%	49.6 %
Disapprove of Economic Performance	54.2%	78.5%	20.6%	48.9%
Don't Know / No Answer	5.3%	1%	7.2%	1.5%

This is confirmed by Table 3.5 which cross-tabulates the economic evaluations of partisan panel respondents only. Note that in these calculations I only include respondents who identify with *the same party* in both waves of the panel study. Table 3.5 suggests that partisan respondents show fluidity in their economic evaluations which are reflective of the economic reality when the economic signal is clearly positive or negative, but follow partisan lines in the absence of such a clear signal. This

confirms Hypothesis 3.

Table 3.5: Fluidity Economic Evaluations Partisans in Panel Studies

Republican Partisans	1990	1992	2000	2004
Disapproves of Economic Performance	33.5%	54.6%	35.9%	14.1%
Approves of Economic Performance	62.5%	44.1%	53.7%	84.6%
Don't Know / No Answer	4%	1.3%	10.4%	1.3%
Democratic Partisans	1990	1992	2000	2004
Disapproves of Economic Performance	27%	94.4%	2.9%	83.1%
Approves of Economic Performance	69%	5.6%	93.5%	15.3%
Don't Know / No Answer	4%	0	3.6%	1.6%

The best example of partisans updating their economic evaluations in Table 3.5 are Republican partisans in the 1990-1992 panel. In 1990, 33.5% of Republican partisans disapprove of President George H.W. Bush's handling of the economy whereas 62.5% approve. In 1992, the number of Republican partisans who disapprove of his handling of the economy rises to 54.6%. This suggests that in this period Republican partisans *updated* their economic evaluations in line with the objective state of the economy but *maintained* their identification with the Republican party. In 2000, the majority of the Republican partisans (53.7%) is positive about President Clinton's handling of the economy in 2000. This contradicts the partisan preferences of these panel respondents, but corresponds to the economic situation at the time. The change in economic evaluations between 2000 and 2004 follows partisan lines however: Republican partisans become *more positive* about the economy in this period whereas Democratic partisans become *more negative*.

These findings confirm existing research which has shown that the association between partisan preferences and economic evaluations is moderated by the economic context.

Most importantly, when voters receive consistently negative information about the economy, the effect of partisanship on economic evaluations is diminished and the chances of cross-pressure increase. Yet, when the economic signal is less clear, partisan bias has a stronger effect on the alignment of evaluations and identification.²⁶ The 1990-1992 panel and the 2000-2004 support the fact that: ‘the information environment can facilitate or inhibit partisan rationalization.’²⁷

3.4.2 Estimating the Percentage of Cross-Pressured Partisans

After having thus established that economic evaluations change across the panel, even among partisan respondents, I now move to an estimation of the number of cross-pressured partisans in each election year. For these estimates I focus on single election years (1992, 2000 and 2004). A respondent is considered cross-pressured when they identify with a particular party (i.e. Republican) in a particular panel-wave (1992, 2000 or 2004), but report an evaluation of the president’s handling of the economy that *conflicts* this partisan identification.

Figure 3.2 below displays the number of voters who can be categorized as cross-pressured in the 1992, 2000 and 2004 US presidential elections.²⁸ Voters in bars 2 and 4 are cross-pressured. In 1992 I find that 28% of all partisans can be classified as cross-pressured, in 2000 the number is 29% of partisans and in 2004 19% of partisans report contradictory evaluations. What stands out from Figure 3.2 is the fact that there is a large group of Republican cross-pressured partisans in both 1992 and 2000. This reflects the economic context at the time of these elections: the 1992 elections

²⁶Dickerson (2016); Parker-Stephen (2013)

²⁷Parker-Stephen (2013, p.1078)

²⁸Data is weighted using panel weights included in the data-set that control for sampling and attrition.

took place in the context of a recession, whereas the year 2000 signified a period of economic prosperity and an unprecedented boost in employment. In both these years, Democratic voters saw their partisan bias confirmed by economic reality: in 1992 the state of the national economy confirmed their negative opinion of incumbent President George H.W.Bush and in 2000 it confirmed their positive evaluation of President Clinton. Consequently, the number of cross-pressured Democratic voters is low in both years.

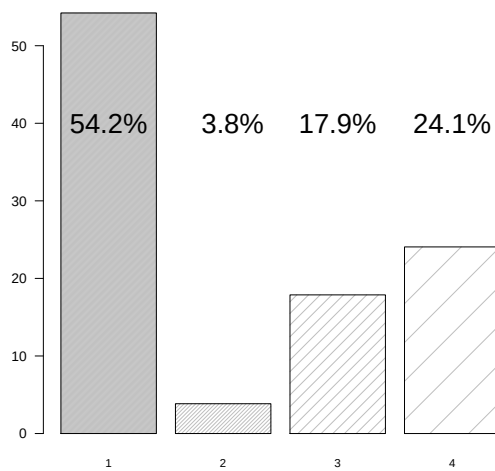
For Republican partisans however, the economic reality is in contradiction with their partisan beliefs in both 1992 and 2000. Figure 3.3 suggests that Republican partisans are quite receptive to this fact. If I look at Republican partisans only (Figure 3.3), I find that in 1992 57% of Republican partisans provide a negative evaluation of President George H.W. Bush's handling of the economy. Likewise, in 2000, 59% of these Republicans state that President Clinton did a good job in handling the American economy. In line with Hypothesis 3, the situation is more nuanced in 2004. In this year, the majority of voters on both sides of the partisan divide reported economic performance evaluations that were in line with their partisan identity (see both Figure 3.2c and Figure 3.3). This is in line with other studies that have found that partisan bias is most pronounced when the economy is neither very good nor bad.²⁹ Still, even in a year where the economy is neither good nor bad, 17% of Republican partisans and 20% of Democratic partisans are conflicted between their partisan identity and their evaluation of the president's economic competence. If I look at the number of cross-pressured partisans as a figure of the entire electorate, thus including Independents, I find that in 1992 and 2000 a total of 25% of the entire electorate was cross-pressured, this figure was again slightly lower at 17% in 2004.³⁰

²⁹Healy and Malhotra (2013) specifically see Figure 1, p.292

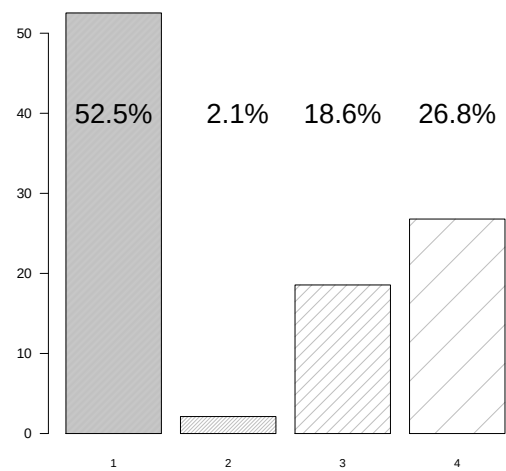
³⁰Independents are automatically classified as not cross-pressured in this calculation.

Figure 3.2: Percentage of Cross-Pressured Partisans 1992, 2000, and 2004 Presidential Elections

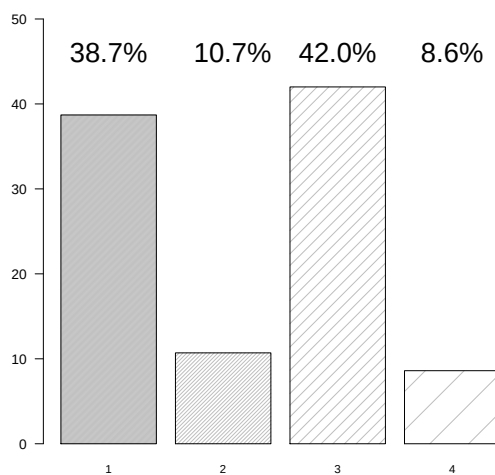
(a) 1992 Presidential Elections



(b) 2000 Presidential Elections



(c) 2004 Presidential Elections



(d) Figure Legend

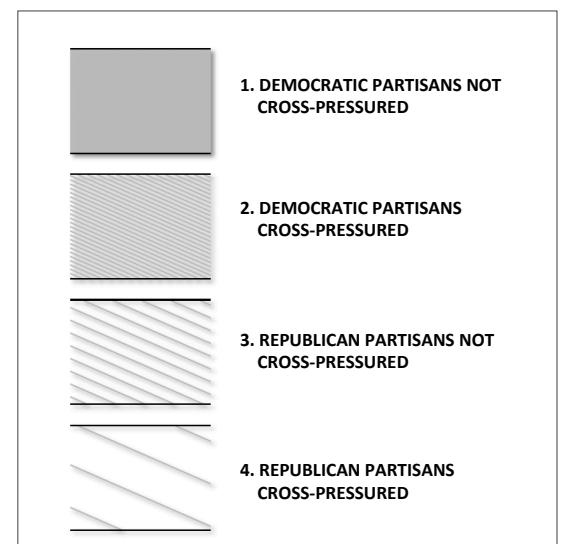
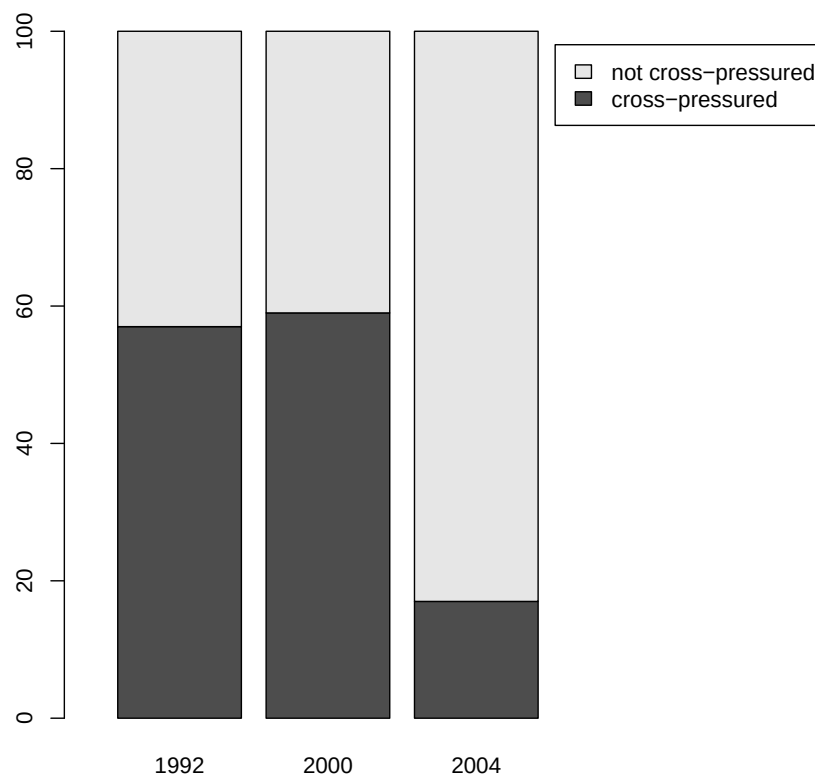


Figure 3.3: Cross-Pressured Republican Partisans 1992, 2000, and 2004 Presidential Elections



One potential problem with these estimates is the fact that partisan identification and economic performance evaluations are measured in the same survey wave. Existing research suggests that partisan identification may bias economic evaluations, which makes it difficult to disentangle whether economic evaluations reflect a voter’s objective evaluations of the economy or are merely a proxy for partisan preferences.³¹ If this bias exists, this suggests that the number of cross-pressured partisans as presented here is *under-reported*. To account for this, the Appendix provides estimates of the percentage of cross-pressured partisans that use a lagged version of partisan identification to ensure more exogeneity between partisan identification and economic evaluations (Figure A.1 in the Appendix). Thus, to estimate cross-pressure in 1992 [2004], partisan identification from the 1990 [2000] wave is used. This method provides very similar results to the method used in this section but indeed shows that the estimated number of cross-pressured voters is several percentage points *higher* than the estimates provided here when using a lagged version of partisan identification.

Based on these descriptive analyses, it is clear that the number of cross-pressured partisans is *higher* when there is a clear economic signal (1992 and 2000) compared to a situation when the economy is performing in a more ‘average’ fashion (2004). This suggests that democratic accountability is most likely to occur when the economic information signal voters receive is unambiguously positive or negative.³² It is furthermore interesting that I do not find that the number of out-party cross-pressured voters (positive about out-party) is consistently higher than in-party cross-pressured voters (negative about own party). Rather, although in 2000 the number of out-party cross-pressured voters is much higher [Republicans positive about Clinton], in 1992 the percentage of in-party cross-pressured voters is much higher [Republicans negative

³¹Evans and Andersen (2006); Evans and Pickup (2010)

³²See for similar findings Healy and Malhotra (2013); Chzhen et al. (2014); Bermeo and Bartels (2014)

about Bush]. As such it appears that cross-pressure is driven mostly by the objective state of the economy rather than by an a-priori stronger in-party bias. Finally, it is worth noting that even in economically moderate times approximately 17% of the *entire* electorate and 19% of the partisan electorate reports economic performance evaluations of the president that *conflict* with their party affiliation.³³

3.4.3 The Association Between Strength of Partisan Affiliation and Cross-Pressure

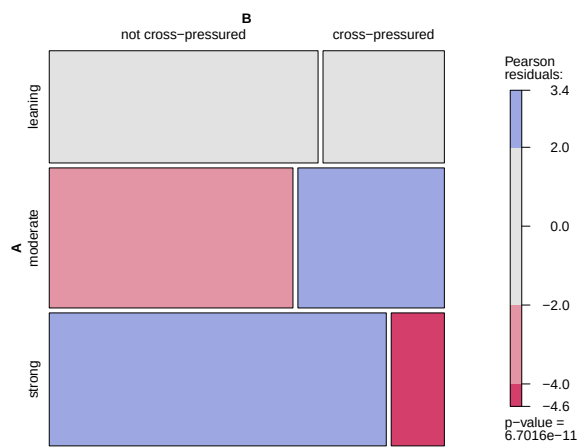
I now turn toward testing Hypotheses 1 and 2 and explore the potential moderating effects of the strength of partisan identification and political knowledge on the likelihood that a partisan voter experiences cross-pressure.

With regard to levels of partisan identification, the expectation is that partisans with *stronger* levels of identification will be *less* likely to experience cross-pressure. Figure 3.4 displays the association between partisan strength [y-axis] and cross-pressure [x-axis] in the form of mosaicplots. Mosaicplots provide a visual representation of a contingency table and a Pearson's Chi-Squared test of Independence. The size of each tile represents the frequency in this cell of the table. The coloring indicates whether a cell contains a larger (blue) or smaller (red) number of observations when compared to a situation of complete independence. Basically, the assumption against which the contingency tables are tested is one of chance: how much does the observed distribution of respondents in the cells of the table (tiles of the mosaic) differ from the situation in which respondents were allocated to cells by chance.

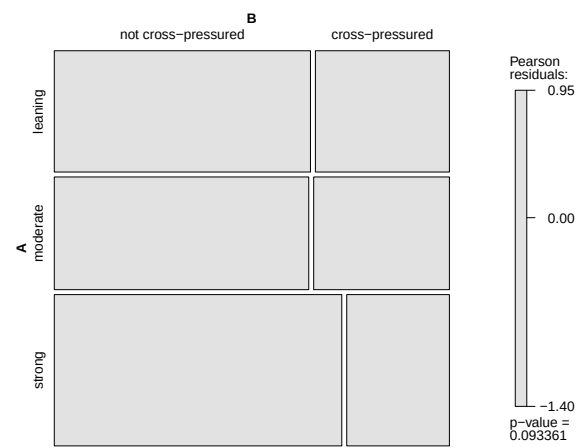
³³Note that Lavine et al. (2012) find quite similar estimate of ambivalent partisans and estimate the number to be around 25%

Figure 3.4: Association Cross-Pressure and Partisan Strength

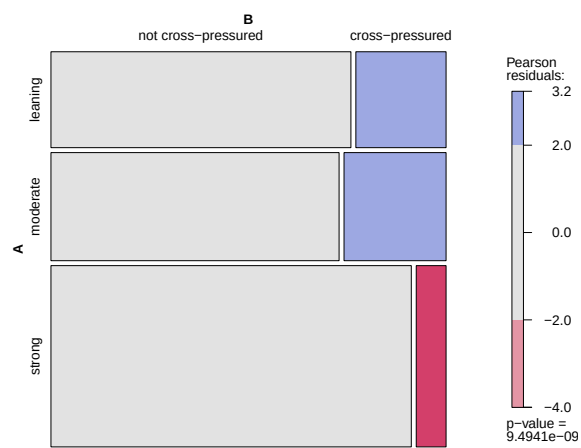
(a) 1992 Survey



(b) 2000 Survey



(c) 2004 Survey



In line with expectation, Figure 3.4 shows that the tile that represents *strong* partisans who are cross-pressured is smallest in all survey years (bottom right tile). This suggests that this is the category with the lowest number of observations. The association is significant in 1992 and 2004, where the tile is shaded red (bottom right corner); which suggests that the number of observations in this category deviates from the assumption of independence. In other words, in 1992 and 2004, there is an association between strength of partisan identification and cross-pressure whereby strong partisan identifiers have a lower likelihood of being cross-pressured. This confirms Hypothesis 1. Similarly, in 1992, the number of strong partisans that is *not* cross-pressured is higher than expected under independence. This is not true for 2000 however, in this year there is no clear association between partisan strength and cross-pressure (all tiles are grey).

Importantly, moderate partisan identifiers are significantly over-represented in the cross-pressured category [blue tile in middle row] in both 1992 and 2004. Weak partisans are over-represented in the cross-pressured category only in 2004 but not in 1992 and 2000. This might suggest that partisans with moderate levels of attachment to a party are more likely to be cross-pressured due to a combination of heightened interest and exposure to political information when compared to weak partisans, and a level of resistance that is lower than that of strong partisan affiliates.³⁴

I thus mostly find confirmation of Hypothesis 1: strong partisans are under-represented among cross-pressured voters [1992 and 2004] and over-represented among non cross-pressured voters [1992]. Yet, the relationship between strength of partisan association is not as straightforward as may be expected. Rather, it varies across years and categories of partisan attachment. In 2000, there is no significant association at all

³⁴This would be an extension of Zaller (1992) on political knowledge and receptiveness and acceptance to information.

between strength of partisan identification and cross-pressure, even though this is a year in which there is a strong, positive, economic signal. In 1992 and 2004 I furthermore find a positive association between moderate levels of partisan identification and cross-pressure. I further explore the association between levels of partisan strength and cross-pressure in section 3.4.5 with regression analyses. In section 3.4.5 I also control for other potential associations such as political knowledge, education and general alienation from the political process which might be correlated to a voter's strength of partisan affiliation as well as their likelihood of cross-pressure.

3.4.4 The Association Between Political Knowledge and Cross-Pressure

Hypothesis 2 states that voters with *moderate* levels of interest in politics are expected to be *more* likely to experience cross-pressure compared to voters with either *low* or *high* levels. The hypothesized reason for this is that voters with moderate levels of political knowledge are thought to have a higher level of exposure compared to voters with low levels of political knowledge, but lower levels of resistance when compared to voters with high levels of knowledge.³⁵ In this chapter I use a measure of political knowledge that is based on respondents' knowledge of factual politics questions and test whether voters with moderate levels of knowledge differ from those with low and high levels of knowledge. The main reason to use a measure of objective political knowledge is that this has been shown to be less susceptible to response bias.³⁶ The specific questions used to construct this variable are reported in Table A.3 in the Appendix. Examples of questions that are asked are whether respondents can identify domestic and foreign leaders (1990-1992), whether respondents know which party holds the majority in both houses (1990-1992) and their knowledge of the candidates

³⁵This is in line with Zaller (1992), see section 2.6.2 for a discussion of this.

³⁶See Price and Zaller (1993) and also Stoker (1993)

of the two main parties (2000). Unfortunately, the 2004 survey does not include any facts-based questions on politics. I therefore use political knowledge as measured in the 2000 wave for these analyses.

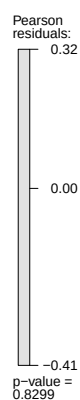
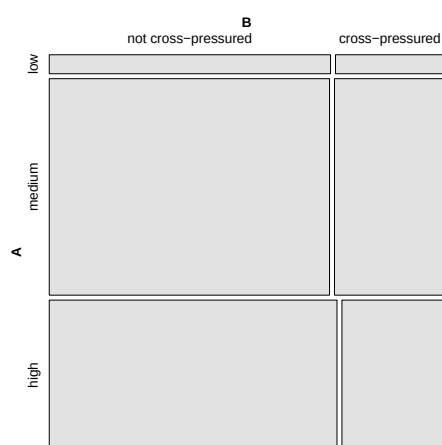
In the 1990 and 1992 survey, respondents could score a total of 6 points on the political knowledge score, in 2000, respondents could score a total of 14 points on the political knowledge score. In reality, the highest score in 2000 was only 12 points.³⁷ The mean scores are relatively low, 2.5 in 1990 (out of 6), 1.7 in 1992 (out of 6) and 3.3 in 2000 (out of 14). As a first descriptive test, I again present mosaicplots of levels of political knowledge and cross-pressure, this is shown in Figure 3.5. In order to do so, I divide respondents into three categories: “low”, “medium” and “high” levels of political knowledge. I take the mean score as the dividing point between respondents. Those who score within 1 standard deviation of the mean (lower or higher) are classified as ‘medium’ levels of knowledge. Those who score more than 1 standard deviation below the mean are classified as ‘low’ and those who score more than 1 standard deviation higher than the mean are classified as possessing ‘high’ levels of knowledge. This leads to some variation across the survey years, specifically in 1992 there is a much higher number of respondents with low levels of knowledge (43%) compared to both 1990 (20%) and 2000 (20%). This reflects the fact that the questions posed vary per survey year.³⁸ In the regression analyses below in section 3.4.5 I use a continuous measure of political knowledge to avoid any concerns about categorisation.

³⁷Note that no missing data is recorded for these factual questions which suggests that in the coding of the interview no distinction was made between non-responses and those who provided an incorrect answer.

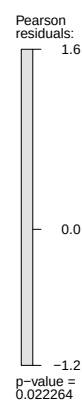
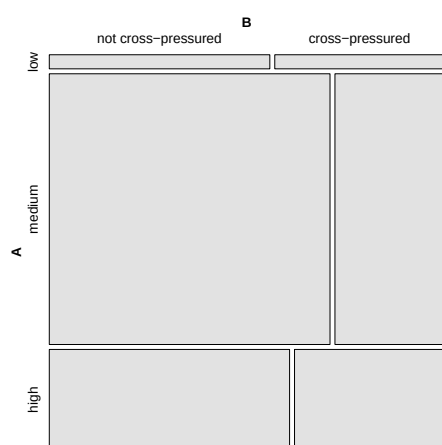
³⁸For 1990 this means that 13.5% of the sample is classified as low; 78.5% as medium and 8% as high. For 1992 this means 43% is classified as low, 36% as medium and 21% as high. In 2000, 20% is low; 65% is medium and 15% is high.

Figure 3.5: Association Cross-Pressure and Political Knowledge

(a) 1992 Survey



(b) 2000 Survey



(c) 2004 Survey

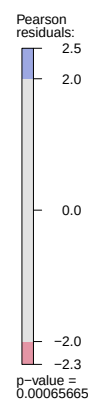
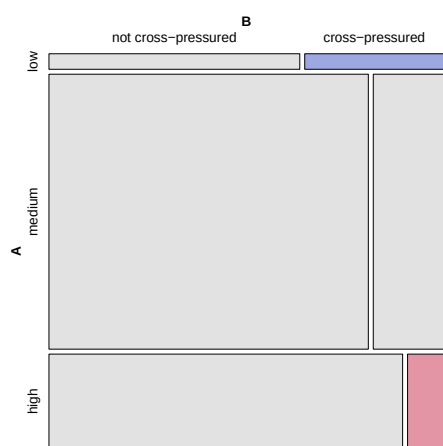


Figure 3.5 presents the mosaicplots for the associations between the political knowledge score and the likelihood of cross-pressure. The expectation of Hypothesis 2 is that partisans with *moderate* levels of political knowledge will be *more* likely to experience cross-pressure when compared to those with low and high levels of knowledge. This is true to the extent that for the cross-pressured partisans (right columns) the tiles that reflect ‘moderate knowledge’ (middle row of tiles), are larger than the ‘low’ and ‘high’ knowledge tiles. This suggests that there are more cross-pressured partisans with moderate levels of knowledge compared to low or high levels. Yet, seeing as the shading is grey in all plots, this association is not statistically significant. The number of cross-pressured partisans with moderate levels of political knowledge is no higher than would be expected in a situation where there is no association between knowledge and cross-pressure. As such there is no clear evidence in favour of Hypothesis 2.

The only significant differences from a statistical point of view appear in 2004 when those with *high* levels of knowledge were significantly less likely to experience cross-pressure and partisans with low levels of knowledge were somewhat *more* likely to experience cross-pressure compared to a situation of independence. One side-note to the 2004 figure (Figure 3.5c) is the fact that 2004 is the survey year in which no political knowledge questions were available, so knowledge is actually measured in the 2000 survey.³⁹ I do not find clear or consistent evidence in favour of hypothesis 2 across the various election years.

³⁹This might actually be beneficial because there is a lower likelihood that there is a reverse association where the experience of cross-pressure leads a voter to withdraw from politics altogether and hence score low on the knowledge measure. The Appendix provides a robustness test for the 1992 association which uses cross-pressure measured in 1992 and the knowledge questions from 1990, this does not alter the (lack of) association in 1992 as depicted here in Figure 3.5.

3.4.5 Regression Analyses of the Likelihood of Experiencing Cross-Pressure

I now move to a regression analysis of the likelihood that a partisan voter is cross-pressured between their party affiliation and their evaluation of the president's economic competence. The benefit of this as compared to the mosaicplots is that it allows me to control for other potential correlating factors. As a first step I run a simple binary logistic regression model in which I use a range of demographic and attitudinal variables to predict the likelihood that a voter experiences cross-pressure in a particular survey year. The demographic, attitudinal and cross-pressure variables are all measured in the same survey wave. The dependent variable cross-pressure takes the value of 1 = cross-pressured or 0 = not cross-pressured. I include a standard range of demographic variables (age, gender, education and income levels).⁴⁰ I of course also include my two main variables of interest: strength of partisan identification and political knowledge. Contrary to the mosaicplots, I include political knowledge as a continuous variable such that I can examine the effect of various levels of political knowledge on the likelihood of cross-pressure without arbitrarily categorizing respondents as either low/medium or high levels of sophistication. In order to test for a non-linear effect of political knowledge I include a squared version of this variable as well. Finally, I include a measure of political alienation to control for the fact that cross-pressure might be driven by general sentiments of disengagement with politics. For this variable, higher levels reflect higher levels of alienation from politics.⁴¹

⁴⁰Note that a variable denoting respondents ethnicity is excluded due to the large amount of missing data for this variable. The Appendix presents a replication of these models with the ethnicity variable included (Table A.4) the inclusion of the race variable does not alter the substantive interpretation of the results presented here.

⁴¹Please refer to the Appendix for a description of the operationalisation of all variables in the model.

Table 3.6: Binary Logistic Regression Model: Likelihood Experiencing Cross-Pressure in 1992, 2000, 2004 Presidential Elections

Incumbent President Survey Year	George H.W. Bush (R) 1992		Bill Clinton (D) 2000		George W. Bush (R) 2004	
	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>
Age	0.02 (0.02)	−0.02 (0.01)	−0.02 (0.02)	0.01 (0.01)	−0.02 (0.01)	0.00 (0.01)
Female	−0.79 (0.52)	−0.10 (0.40)	0.40 (0.60)	0.21 (0.33)	−0.74 (0.39)	0.05 (0.39)
College degree	−0.33 (0.76)	−0.38 (0.43)	−0.03 (0.73)	0.57 (0.33)	−1.25** (0.43)	−0.02 (0.47)
Income above median	0.12 (0.60)	0.18 (0.38)	−0.79 (0.65)	0.24 (0.34)	0.04 (0.46)	−0.51 (0.41)
<i>base cat: leaning</i>						
Moderate partisan	−0.07 (0.63)	0.15 (0.44)	−1.24 (0.70)	0.45 (0.41)	0.83 (0.50)	−0.81 (0.45)
Strong partisan	−1.13 (0.75)	−1.77*** (0.43)	−1.24 (0.72)	−0.51 (0.36)	−0.75 (0.51)	−1.57*** (0.41)
Political knowledge	−0.86 (0.66)	−0.10 (0.46)	−0.22 (0.38)	0.06 (0.20)	0.28 (0.29)	−0.07 (0.24)
Political knowledge squared	0.07 (0.13)	0.02 (0.07)	0.01 (0.04)	−0.01 (0.02)	−0.05 (0.03)	0.00 (0.02)
Political alienation	−0.68* (0.29)	0.08 (0.18)	0.27 (0.26)	0.02 (0.15)	−0.45* (0.19)	0.45** (0.16)
Constant	−0.63 (1.18)	1.69 (0.87)	−0.97 (1.47)	−0.78 (0.94)	1.39 (1.10)	−1.32 (1.07)
N	274	165	213	186	280	296
Pseudo R-Squared	0.15	0.15	0.14	0.04	0.17	0.15
Wald Chi-Square Statistic	23.93***	31.24***	18.69*	9.96	30.27***	37.38***

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

Robust Standard Errors in Parentheses

Table 3.6 shows the results of the analyses. Note that I have split the sample into Democratic partisans and Republican partisans because both sets of partisans experience a different type of cross-pressure (either toward the in- or the out-party) which may have different explanations. The first thing to note from these models is the fact that there is no clear association between cross-pressure and a range of demographic variables (age, gender, income, education). This holds for both Democratic and Republican partisans and across all presidential election years.

With regard to Hypothesis 2, I find no significant effect of political knowledge on the likelihood that a partisan voter experiences cross-pressure. This is true for both Democratic partisans as well as Republican partisans and again holds across all three presidential elections under study (1992, 2000, 2004). Neither the regular nor the squared term is significant in any of the models.⁴² In this chapter I thus reject Hypothesis 2 and conclude that levels of political knowledge do not significantly affect the likelihood that a partisan voter holds economic evaluations that contradict their party loyalty.

With regard to the effect of the strength of partisan affiliation, Table 3.6 offers mixed results. I find no significant effect of the strength of partisan identification for Democratic partisans in 1992 and 2004 and for Republican partisans in 2000, columns 1, 4, 5 of Table 3.6. This suggests that when partisans receive *positive* information about the out-party, there is no effect of the strength of partisan affiliation on the likelihood of cross-pressure. Yet, as columns 2 and 6 show, when there is a *negative* information signal about the in-party, strong levels of partisan affiliation reduce the likelihood of experiencing cross-pressure. Note that the effect of strength of partisan affiliation on cross-pressure is significant at the 10 percent level ($p < 0.08$) for

⁴²The same is true when I test for a simple linear association and exclude the squared term.

Democratic partisans in 2000, column 3. This confirms the pattern that negative sentiments toward the in-party are conditioned by the strength of partisan affiliation. This does not appear to be the case however for positive evaluations of the out-party. On the basis of these analyses I accept an adapted version of Hypothesis 2: *compared to weak partisans, strong partisans are less likely to hold negative evaluations of the economic performance of their in-party president, but there is no effect of strength of partisan identification on cross-pressure toward the out-party President.*

Finally, it is interesting to point out the effect of political alienation. This variable denotes whether panel respondents indicated that they feel disillusioned with politics. High values indicate a high level of alienation. Similar to partisan strength, alienation affects cross-pressure toward the in- and out-party differently. There is a negative association with out-party cross-pressure (column 1 and 5) but a positive association with in-party cross-pressure (column 6). This association makes intuitive sense: voters who feel disenchanted by the political system, are *less* likely to report positive evaluations about the opposition, but are *more* likely to express negative sentiments about their own party.

Another modeling strategy that is often used with panel data is an individual level random effects model.⁴³ The benefit of this strategy is that it accounts for the fact that individuals are sampled at multiple points in time. Thus, rather than use the panel as separate cross-sections (as in Table 3.6), I estimate the effect of the strength of partisan affiliation and political knowledge across the two panel studies. The results of this analysis are displayed in Table 3.7. This modeling strategy offers the same substantive result as displayed in Table 3.6. Thus, using a random effects logistic regression model, Table 3.7, I again find no significant association between political

⁴³See for an application Marsh and Tilley (2009) and a discussion of random effects models Clark and Linzer (2012)

knowledge and the likelihood of cross-pressure, neither linear nor non-linear. With respect to the strength of partisan affiliation, I find a consistent *negative* association between the strength of partisan affiliation and cross-pressure. I do not find a significant association for Democratic partisans in 1990-1992 which is in line with the fact that partisan strength mostly moderates negative information signals about the in-party. This confirms the findings of Table 3.6. The only difference between the two modeling strategies is that Table 3.7 also finds that moderate Democratic partisans are *more* likely to experience cross-pressure in the period 1990-1992 compared to leaning partisans, which strengthens the evidence for Hypothesis 2.

The Appendix provides two further robustness checks to the findings presented here. First, in Appendix Table A.4, I show a replication of the models presented here in Table 3.6 but account for the fact that respondents are nested geographically in states. The benefit of this approach is that it provides a more robust control for state-level factors that might affect a voters' likelihood of experiencing cross-pressure, the downside is that it reduces the number of observations. The second robustness check I present in the Appendix (Table A.5) is a replication of the models shown here but uses a lagged version of the predictive (independent) variables. Thus, I use demographic variables, political knowledge and partisan strength as measured at t-1 to predict cross-pressure at time-point t. I thus predict cross-pressure in 1992 with 1990 survey variables and 2004 cross-pressure with 2000 survey variables. These alternative modeling strategies support the substantive conclusions presented here.

Table 3.7: Robustness Check: Binary Logistic Regression Random Effects Panel Model - Likelihood Experiencing Cross-Pressure

	1990-1992		2000-2004	
	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>
Age	0.01 (0.01)	-0.01 (0.01)	-0.02 (0.01)	0.00 (0.01)
Female	-0.63*** (0.18)	0.17 (0.25)	-0.41 (0.31)	0.14 (0.24)
College degree	-0.28 (0.22)	-0.29 (0.28)	-0.74* (0.35)	0.26 (0.27)
Income above median	-0.04 (0.17)	-0.00 (0.24)	-0.29 (0.35)	-0.07 (0.25)
<i>base cat: leaning</i>				
Moderate partisan	0.44* (0.21)	-0.34 (0.27)	0.20 (0.37)	-0.19 (0.30)
Strong partisan	-0.34 (0.22)	-1.93*** (0.40)	-1.07** (0.41)	-1.03*** (0.30)
Political knowledge	-0.01 (0.17)	0.17 (0.27)	0.01 (0.22)	0.01 (0.15)
Political knowledge squared	-0.03 (0.03)	-0.03 (0.05)	-0.03 (0.02)	0.00 (0.01)
Political alienation	-0.49*** (0.10)	0.33** (0.12)	-0.17 (0.12)	0.21* (0.09)
<i>ref: 1990 / 2000</i>				
Year 1992 / 2004	-2.30*** (0.35)	1.32*** (0.52)	1.30*** (0.36)	-2.07*** (0.34)
Constant	0.34 (0.35)	-0.38 (0.51)	-0.39 (0.87)	-0.06 (0.69)
N	1249	835	493	482
Wald Chi-Square Statistic	54.59***	33.54***	32.85***	42.80***

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

Robust Standard Errors in Parentheses

3.4.6 Summing Up: Who are the Cross-Pressured Partisans

To conclude this section, perhaps the most striking finding is the fact that there is a substantive group of partisans whose evaluations of the economic stewardship of the president do not *align* with their partisan sentiments across the 1992, 2000 and 2004 presidential elections. In line with expectation, the number of cross-pressured partisans is higher when economic signals are unambiguous. This suggests there are conditions under which partisans are receptive to economic information even when this contradicts their party preferences.

I do not find any evidence to suggest that the receptiveness to this information varies across levels of political knowledge. I therefore reject Hypothesis 2 in this chapter. I *do* find evidence however to suggest that voters with *strong* levels of partisan identification are more resistant to information that contradicts their partisan preferences. Yet, I find that this is true mostly for information that reflects negatively on the in-party. Information that sheds a positive light on the out-party is equally likely (or unlikely) to be accepted by leaning, moderate and strong partisans. This suggests that it might be easier to convince partisans that the out-party president has done a good job than to convince them their in-party president has performed poorly.

The first part of the framework of cross-pressured has now been explored. The next question is whether these cross-pressured partisans actually behave differently at election time. I thus now ask: does cross-pressure affect voting behaviour and abstention?

3.5 Results Part II: How Do Cross-Pressured Partisans Behave at Election Time?

In this section I test Hypotheses 4 and 5: are cross-pressured partisans more likely to either abstain (H4) or switch parties (H5) when compared to partisans whose economic evaluations align with their party preferences?

3.5.1 Do Cross-Pressured Partisans Abstain?

I first test Hypothesis 4 (Abstention) and run a binary logistic regression model where the dependent variable takes the value of 1 when a voter reported not voting, and 0 if the voter reports voting in the presidential election (1992, 2000, 2004). The results are shown below in Table 3.8. First, the analysis confirms well-known patterns of the likelihood of abstention: there is a negative association with political knowledge, education levels and income levels. A positive association exists between the likelihood to abstain and levels of alienation with politics. The effects of cross-pressure on abstention are substantively small and statistically insignificant, this confirms Hypothesis 4: cross-pressured partisans do not appear to more likely to abstain from voting compared to their non cross-pressured counter-parts. This is true for both Republicans and Democrats and holds in all presidential election years under study (1992, 2000, 2004).

It might be problematic for the analyses presented in Table 3.8 that the prediction variables are measured in the same year as the outcome variables, even though they are measured in pre- and post-electoral surveys respectively. I therefore conduct a robustness check where I utilize the fact that the same respondents were questioned at multiple points in time. I run an individual level fixed effects model with dependent variable abstention. In this model (also referred to as a conditional logit model),

individuals basically act as their own control-group. Variables that are stable over time are held fixed at the individual level, the model thus measures within individual variation over time. The main benefit of this type of model is that it effectively controls for potential omitted variable bias. The main downside of this type of model is the fact that it requires variation in *both* outcome and prediction variables. This requirement severely restricts the sample size.

Table 3.9 presents the individual level fixed effects model for the 1990-1992 and 2000-2004 panel and tests whether voters who became cross-pressured between the 1988 and 1992 elections, or between the 2000 and 2004 elections, were more likely to move from voting in 1988 to not voting in 1992, or from voting in 2000 to not voting in 2004. Table 3.9 suggests that does not appear to be the case in either of these periods. Note that in Table 3.9 all variables that do not vary on both the dependent and independent variable are dropped from the model. This is true for gender, education and strength of partisan identification. I furthermore exclude the remaining independent variables political knowledge, income and alienation in order to maximize the number of observations on which to test the effect of cross-pressure. Including the other variables in the model does not alter the substantive outcomes but severely reduces the sample size and inflates the standard errors. Based on the results displayed in Table 3.9 I again conclude that there is no significant effect of cross-pressure on abstention and accept Hypothesis 4: cross-pressured partisans are no more or less likely to abstain compared to non cross-pressured partisans.

Table 3.8: Binary Logistic Regression Model: Likelihood of Abstention in 1992, 2000 and 2004 Presidential Elections

Incumbent President Survey Year	George H.W. Bush (R) 1992		Bill Clinton (D) 2000		George W. Bush (R) 2004	
	Democrats	Republicans	Democrats	Republicans	Democrats	Republicans
Cross-pressured	0.57 (0.70)	0.25 (0.59)	0.83 (1.17)	1.49 (1.00)	0.76 (0.55)	0.03 (0.72)
Age	-0.02 (0.01)	-0.01 (0.02)	-0.06 (0.03)	-0.09* (0.04)	0.00 (0.01)	0.02 (0.02)
Female	-0.33 (0.41)	-0.16 (0.58)	0.32 (0.73)	0.58 (0.94)	0.12 (0.53)	-0.28 (0.71)
College degree	-1.41* (0.56)	-1.12 (0.84)	-0.94 (0.86)	-0.50 (0.90)	-0.15 (0.61)	-0.06 (0.71)
Income above median	-0.93* (0.42)	-1.83** (0.61)	-1.05 (0.76)	0.34 (0.78)	-0.28 (0.63)	-0.60 (0.69)
Political knowledge	-0.22 (0.14)	-0.35 (0.23)	-0.22 (0.18)	-0.59 (0.31)	-0.36** (0.13)	-0.70*** (0.20)
Political alienation	0.15 (0.18)	-0.06 (0.26)	0.43 (0.31)	0.90* (0.45)	0.33 (0.20)	0.54* (0.25)
Moderate partisan	0.10 (0.44)	1.60* (0.70)	1.04 (0.99)	0.48 (1.07)	0.16 (0.66)	-0.81 (0.81)
Strong partisan	-0.83 (0.54)	0.35 (0.81)	1.94 (1.03)	0.03 (0.14)	0.01 (0.66)	-1.10 (0.73)
N	232	124	104	112	167	178
LR Chi-Squared	33.35***	33.23***	25.63**	41.27***	23.17**	46.42***
State Level Fixed Effects	yes	yes	yes	yes	yes	yes

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

Standard errors in parentheses

Table 3.9: Robustness Check: Binary Logistic Regression Model with Individual Level Fixed Effects - Likelihood of Abstention in 1992, 2000 and 2004 Presidential Elections

	1988-1992 Elections		2000-2004 Elections	
	Democrats	Republicans	Democrats	Republicans
Cross-pressured	0.54 (0.62)	0.82 (0.67)	1.54 (1.25)	0.25 (1.10)
Year dummy	-0.82** (0.28)	-1.65*** (0.51)	-0.44 (0.44)	-0.85 (0.71)
N	132	84	54	36
Pseudo R-Square	0.14	0.28	0.06	0.15
Wald Chi-Square Statistic	10.98**	11.88**	1.90	3.13

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

Standard errors in parentheses, clustered at the individual level

3.5.2 Do Cross-Pressured Partisans Switch Parties?

I now turn to the question of whether partisans who hold economic evaluations of the president's handling of the economy that contradict their partisan affiliation are more likely to switch parties at election time, compared to partisans whose evaluations and partisan identity align. This is an important question because if cross-pressured partisans are in fact more likely to vote for an out-party, this suggests that these cross-pressured partisans are more receptive to economic information and more likely to exercise democratic accountability toward their in-party president at election time.

First, I explore whether there are partisans who alter their party choice across the various presidential elections to ensure that there is in fact meaningful variation to explain. This appears to be the case. Simple cross-tabulations of the panel respondents show that between 1988 and 1992 only 64% of Democratic identifiers and 67% of Republican identifiers voted for their in-party at both these presidential elections. This increases in subsequent elections to 81% (Democratic partisans) and 85% (Republican partisans) between 1996 and 2000. In the period 2000-2004 this is true for 79% of Democratic partisans and 85% of Republican partisans. Thus, over the period

1990-1992 and 2000-2004 between 15-30% of partisans did not consistently vote for the party they identified with.⁴⁴

A second question is *where* these partisans switch to. In 1992, the majority of the Democratic partisans switched from voting for the Democratic party in 1988 to a vote for third party candidate Ross Perot in 1992. For Republican partisans however, the largest switch occurred between the Republican Party (1988) and the Democratic party (1992). This was true for 7% of respondents who identified as Republican in the 1992 survey.⁴⁵ In 2000, the largest switch for Democratic identifiers was to the Republican party: 3.4% of Democratic identifiers switched from voting Democrat in 1996 to voting Republican in 2000. For the Republican party the largest proportion of switchers went to a Third party (3.5%). The same pattern is true for switchers between 2000 and 2004.

As a next step I explore whether cross-pressure between party affiliation and evaluations of the president's handling of the national economy is an important explanatory factor of vote-switching across elections. Table 3.10 below provides binary logistic regression models with a dependent variable 'party-switch'. This variable takes the value of 1 if a respondent switched between parties across presidential elections. Thus the 1992 model reflects a switch between 1988 and 1992 vote choice, the 2000 model reflects a switch between the 1996 and 2000 elections and the 2004 model reflects a switch between the 2000 and 2004 presidential elections.⁴⁶ Non-voters are excluded from these analyses.

⁴⁴Note that here I only focus on partisans who report the *same* party identification across the survey waves to avoid including respondents who updated their partisan identity in response to performance evaluations.

⁴⁵See work by Alvarez and Nagler (1995) which also finds that Bill Clinton mainly won over Republican voters on the topic of the economy.

⁴⁶Partisans are identified in the relevant survey-years (1992, 2000 and 2004). Note that respondents who identify with a particular party but consistently vote for the out-party are not considered switchers.

Table 3.10: Binary Logistic Regression Model: Likelihood Vote-Switching in 1992, 2000 and 2004 Presidential Elections

Vote Switch	1988-1992 Elections		1996-2000 Elections		2000-2004 Elections	
	Democrats	Republicans	Democrats	Republicans	Democrats	Republicans
Cross-pressure	1.33 (0.81)	2.09** (0.79)	0.40 (1.67)	2.29** (0.83)	2.14** (0.71)	1.88*** (0.58)
Age	-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.03)	-0.04 (0.03)	-0.03 (0.02)	-0.02 (0.02)
Female	-0.50 (0.41)	-0.31 (0.71)	1.11 (0.71)	-0.21 (0.58)	0.27 (0.53)	0.81 (0.65)
College degree	-1.06* (0.50)	0.08 (0.82)	-0.63 (0.82)	-0.16 (0.63)	-0.44 (0.56)	0.01 (0.67)
Income above median	-0.06 (0.44)	1.23 (0.96)	-2.05* (0.80)	-0.21 (0.66)	-0.52 (0.57)	-0.14 (0.64)
Political knowledge	-0.18 (0.14)	-0.08 (0.28)	-0.21 (0.21)	-0.09 (0.12)	0.08 (0.10)	0.00 (0.10)
Political alienation	-0.45* (0.21)	0.67 (0.36)	0.24 (0.30)	0.07 (0.32)	0.17 (0.23)	-0.27 (0.29)
Moderate partisan	-0.73 (0.50)	-0.83 (0.78)	-1.18 (0.78)	0.50 (0.69)	-1.46* (0.71)	-0.91 (0.62)
Strong partisan	-1.61** (0.51)	-1.44 (0.89)	-3.34** (1.18)	-1.35 (0.74)	-0.91 (0.60)	-2.13** (0.68)
N	164	68	111	109	137	167
LR Chi-Squared	29.67***	17.11*	33.40***	28.47***	19.35*	30.32***
State Level Fixed Effects	yes	yes	yes	yes	yes	yes

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

*Robust standard errors in parentheses**Party identification of voters as reported in 1992, 2000, 2004 surveys*

The regression analyses presented in Table 3.10 largely confirm Hypothesis 5: there is a positive and statistically significant effect of cross-pressure on a party-switch in 4 out of 6 models. The exceptions are Democratic partisans in 1992 and 2000. This may reflect the fact that there were very few cross-pressured Democrats in these years. As discussed above, the 1992 and 2000 presidential elections provided an economic context that facilitated cross-pressure among Republican partisans, but reduced the likelihood of cross-pressure for Democratic voters. The economic tide was in line with the partisan affiliation of Democratic voters, in 1992 it confirmed their low opinion of incumbent President George H. W. Bush and in 2000 the economic growth confirmed their positive opinion of incumbent President Clinton.

For Republican partisans however, the story is different. As discussed above, in 1992 approximately 59% of Republican partisans held a *negative* opinion of President Bush' handling of the economy. Table 3.10 now confirms that this party-performance cross-pressure *significantly increased* the likelihood that Republican partisans who voted for the Republican party in 1998 voted for another party in 1992. The same is true in 2000 and 2004: Republican partisans who held positive opinions of President Clinton in 2000 or negative opinions about George W. Bush in 2004 were more likely to vote for either the Democratic party or a Third party in the 2000 and 2004 presidential elections, whereas they voted for the Republican party in 1996 and 2000 respectively.

Substantively, we might be most interested in Republican partisans in this period since the number of cross-pressured Republicans was much larger in the period under study, especially in 1992 and 2000, compared to the number of Democratic cross-pressured partisans. I thus calculate the predicted likelihood of a party-switch for Republican voters in these years. I use the estimates presented in Table 3.10 for these calculations and set all other variables to their mean value. A voter who iden-

tifies with the Republican party in the 1992 survey wave and reported to have voted for the Republican party in 1988 and who holds a *positive* evaluation of incumbent President George W.H. Bush' handling of the economy has a likelihood of switching to a non-Republican vote in 1992 of 44.8%. Note that this is remarkably high, but corresponds with other research on these elections which suggests that Republican partisans were especially likely to switch in these elections. This was partially due to the presence of a strong third party candidate (Ross Perot).⁴⁷ The same voter who holds a *negative* evaluation of incumbent President Bush' handling of the economy has a likelihood of switching of 81.2%.⁴⁸ In 2000, a Republican partisan who voted for the Republican party in 1996 and has a *negative* opinion on President Clinton (not cross-pressured) has a likelihood of switching of 6.4%; for a cross-pressured Republican this is 34.1%. The pattern is very similar in 2004 when a Republican partisan who voted for George W. Bush in 2000 but holds a *negative* evaluation of his economic performance in 2004 has a likelihood of switching of 34.8% in 2004, compared to only 9.6% for a non cross-pressured Republican. Cross-pressure between partisan loyalty and retrospective economic performance evaluations thus has a substantive effect on the likelihood of partisans to switch parties across elections.

Figure 3.6 displays the predicted likelihood of a vote-switch in the 1992, 2000 and 2004 elections for cross-pressured and non cross-pressured Republicans at various levels of partisan affiliation using the models of Table 3.10.⁴⁹ Light grey bars reflect non cross-pressured Republican partisans, dark grey bars represent cross-pressured Republican partisans. Note that the interaction between cross-pressure and strength of partisan affiliation is not significant, hence the overlapping error bars in Figure 3.6. This suggests that the effect of cross-pressure does not vary across levels of partisan

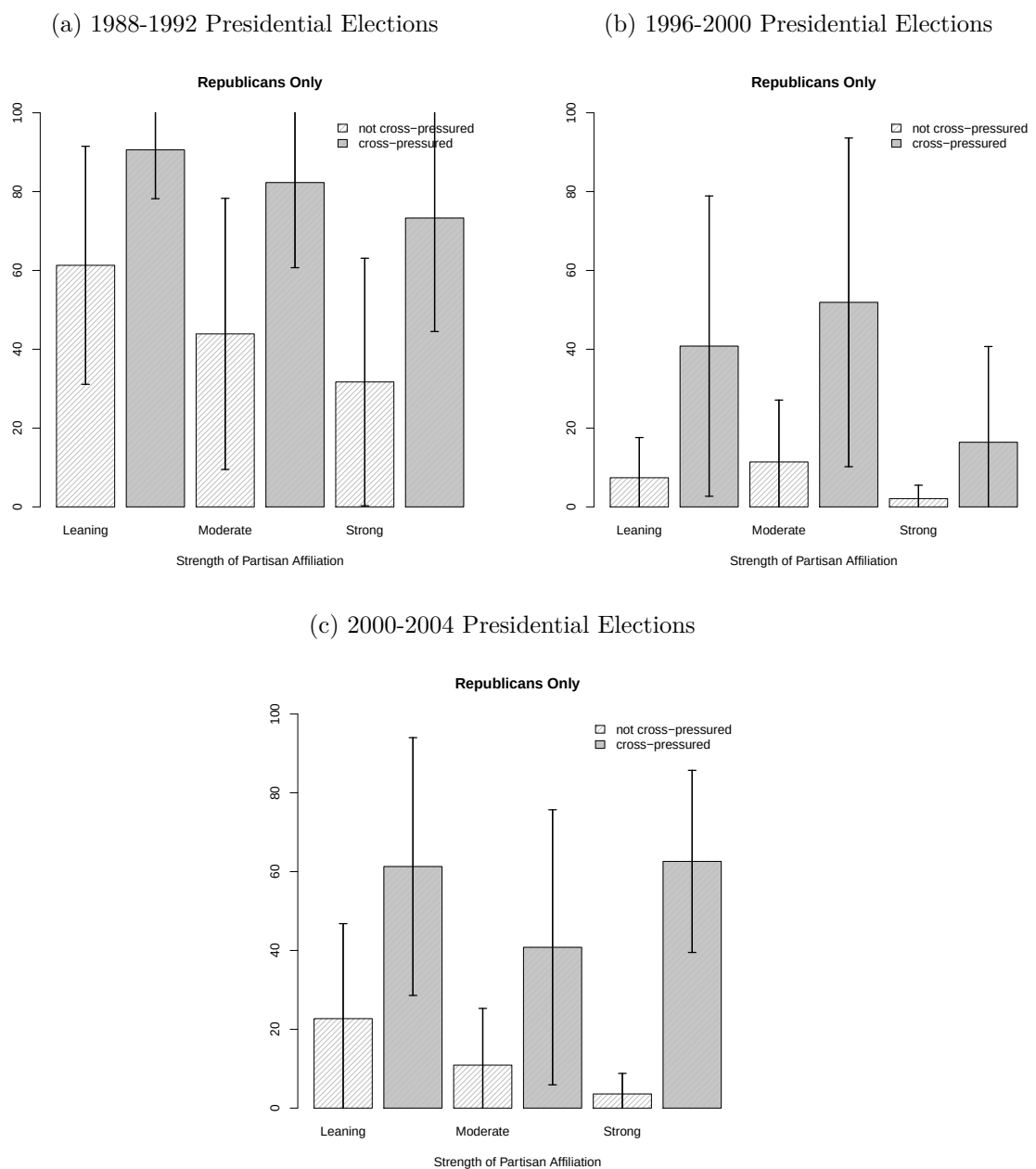
⁴⁷Alvarez and Nagler (1995)

⁴⁸Predicted likelihood, all other variables set to their mean.

⁴⁹Note that for the calculation of predicted probabilities all other variables are set to the mean and the fixed effect is assumed to be zero.

affiliation. The main aim of Figure 3.6 is to show the range of the predicted likelihood of a vote-switch for Republican partisans across the three presidential elections and allow for a comparison between cross-pressured and non cross-pressured partisans.

Figure 3.6: Predicted Likelihood of Vote-Switching Cross-Pressured Republicans in 1992, 2000 and 2004 Presidential Elections



3.5.3 Robustness Checks Vote-Switching Cross-Pressured Partisans

A concern that could exist with regards to the models displayed in Table 3.10 is the fact that the economic evaluations that I use as part of the cross-pressure variable are themselves affected by partisan bias.⁵⁰ Although the premise of cross-pressure suggests that at least part of the respondents have overcome this bias and thus report evaluations of the president's economic competence that contradict partisan identification, this does not preclude the fact that for the non cross-pressured voters, partisan bias in economic evaluations still exists and could bias the estimates of their effect on voting. In order to account for this, I replicate the models presented in Table 3.10 but include a lagged version of partisan identification.⁵¹ The lagged version of partisanship is thus partisan identification as recorded in the previous survey wave. Because of this, it is only possible to re-estimate the models for the 1992 and 2004 presidential elections since there is no survey wave preceding the 2000 survey.

Note that the lagged partisan variable is coded such that higher scores indicate Republican identification and lower values indicate identification with the Democratic party. This explains the positive association between lagged partisanship and vote switching for the Democratic respondents and a negative association for Republican respondents. The main thing to observe in Table 3.11 is that the association between cross-pressure and party-switch is robust to the inclusion of previous partisan identification. In fact, it is now also significant for the Democratic respondents in 2004.

⁵⁰See Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013)

⁵¹See for this approach Lewis-Beck et al. (2008)

Table 3.11: Robustness Check 1: Party-Switch Model Including Lagged Partisanship

Vote Switch	1988-1992 Elections		2000-2004 Elections	
	Democrats	Republicans	Democrats	Republicans
Cross-pressure	2.08*	2.17*	2.08**	2.41**
	(1.00)	(0.88)	(0.72)	(0.80)
Partisanship lagged [1990, 2000]	0.56	-0.11	0.73*	-0.70*
	(0.29)	(0.28)	(0.29)	(0.32)
Age	-0.03	-0.03	-0.01	0.00
	(0.02)	(0.03)	(0.02)	(0.03)
Female	-0.19	-0.38	0.21	0.38
	(0.46)	(0.79)	(0.59)	(0.75)
College degree	-1.26*	0.02	-0.35	-0.02
	(0.54)	(0.89)	(0.60)	(0.78)
Income above median	0.22	1.62	-0.66	0.80
	(0.48)	(1.01)	(0.62)	(0.82)
Political knowledge	-0.23	-0.08	0.09	0.02
	(0.14)	(0.29)	(0.12)	(0.12)
Political alienation	-0.47*	0.79	0.18	-0.54
	(0.23)	(0.41)	(0.24)	(0.40)
Weak partisan	-0.40	-0.39	-0.61	-0.68
	(0.58)	(0.82)	(0.78)	(0.76)
Strong partisan	-0.95	-1.24	0.12	-1.19
	(0.62)	(0.97)	(0.75)	(0.85)
N	152	64	131	137
LR Chi-Squared	35.35***	16.10	23.69**	25.60**
State Level Fixed Effects Included	yes	yes	yes	yes

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

*Robust standard errors in parentheses**Party identification of voters as reported in 1992, 2000, 2004 surveys*

As a second robustness check I create *a new version of the cross-pressure variable* in which I use lagged partisan identification instead of contemporaneous partisan identification. Thus, for the 1992 presidential elections, cross-pressure reflects a person's partisan identification as stated in the 1990 survey and their economic evaluations as recorded in the 1992 survey. The results of these analyses are displayed below in Table 3.12 and again confirm Hypothesis 5: there is a positive effect of cross-pressure on the likelihood to switch between parties across the presidential elections. One note of caution with the models presented in Table 3.12 is the fact that the combination of including state level fixed effects and using partisan identification of a prior survey wave reduces the number of observations.

Table 3.12: Robustness Check 2: Party-Switch Model With Lagged Cross-Pressure

Vote Switch	1988-1992 Elections		2000-2004 Elections	
	Democrats	Republicans	Democrats	Republicans
Cross-pressure lagged [1990, 2000]	2.03* (0.97)	2.13* (0.90)	2.32** (0.82)	2.91* (1.15)
Age	-0.04* (0.02)	-0.02 (0.03)	0.00 (0.02)	0.00 (0.03)
Female	-0.30 (0.50)	-0.31 (0.79)	0.31 (0.61)	0.44 (0.87)
College degree	-1.41* (0.55)	-0.85 (1.09)	-0.07 (0.69)	0.84 (1.04)
Income above median	0.08 (0.52)	0.99 (1.03)	-0.04 (0.68)	0.97 (1.00)
Political knowledge	-0.15 (0.24)	0.03 (0.33)	0.05 (0.12)	0.06 (0.14)
Political alienation	-0.51* (0.24)	0.32 (0.45)	0.32 (0.29)	-0.61 (0.41)
Weak partisan	-0.42 (0.59)	-0.31 (0.90)	-0.97 (0.83)	-1.52 (1.20)
Strong partisan	-1.17* (0.56)	-1.61 (1.09)	-0.43 (0.70)	-1.53 (0.96)
N	140	56	112	100
LR Chi-Squared	29.23***	14.55	13.51	16.90**
State Level Fixed Effects Included	yes	yes	yes	yes

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

Robust standard errors in parentheses

Party identification of voters as reported in 1992, 2000, 2004 surveys

As a final robustness check I re-run the models in Table 3.10 on the entire panel using an individual fixed effects model. As discussed in the section on abstention, models with individual level fixed effects provide the most rigorous specification to control for potential omitted variable bias because they measure within subject variability across time. One difficulty with the models however is that they require change over time on both dependent and independent variables. I therefore run these analyses on the entire panel rather than split Democrats and Republicans in order to maximize the number of observations. To test for the different effects of cross-pressure for Republican versus Democratic voters I now include a categorical variable of cross-pressure.

All variables that do not change over time are excluded from the model (gender, partisan strength, education). The other independent variables, political knowledge, alienation and income levels, only show minimal variation over time. Including these in the models thus severely reduces the number of respondents.⁵² I therefore also exclude these variables and estimate only the effect of cross-pressure on a change in vote choice. The dependent variable denotes a vote for the ‘in-party’, and thus captures whether or not voters switch over time (from in-party to out-party), or whether they consistently vote for the in-party. The coefficients of cross-pressure in Table 3.13 reflect whether a change toward cross-pressure over the period under study affects the likelihood of a consistent in-party vote. The results suggest that this is the case and that the experience of cross-pressure significantly *reduces* the likelihood of an in-party vote. Table 3.13 hereby confirms the main results displayed in Table 3.10.

⁵²In the case of the 2000-2004 models it reduces it to $N = 16$.

Table 3.13: Robustness Check 3: Party Switch Model with Individual Level Fixed Effects

<i>DV = in-party vote</i>	1988-1992 Elections	2000-2004 Elections
<i>base category: democrats non cp</i>		
Democrats cp	-2.00*** (0.51)	-3.03* (1.30)
Republicans non cp	1.87** (0.69)	17.31*** (0.47)
Republicans cp	0.40 (0.61)	-1.04 (0.83)
Year dummy	-0.93*** (0.22)	-0.29 (0.36)
N	314	120
Pseudo-R Squared	0.27	0.46
Wald Chi-Square Statistic	49.53***	30.81***

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

Standard errors in parentheses, clustered at the individual level

The baseline category in the analyses are Democratic partisans who are not cross-pressured. The analyses in Table 3.13 suggest that cross-pressured Democrats are significantly *less* likely to vote for the in-party in both the 1988 and 1992 elections as well as across the 2000 and 2004 elections compared to non cross-pressured Democrats (coefficients -2.00 and -3.03). Table 3.13 shows that non cross-pressured Republicans are significantly more likely to stay loyal to the Republican party in this period compared to non cross-pressured Democrats. Importantly, when I change the baseline in Table 3.13 to “non cross-pressured Republicans”, the coefficient for cross-pressured Republicans is -1.46 (p < 0.001) in the period 1988-1992 and -20.31 (p < 0.000) in the period 2000-2004. Thus, the difference between non cross-pressured and cross-pressured Republicans is also significant, where the latter again are *less* likely to consistently vote for the Republican party across presidential elections. In light of these robustness checks, I thus accept Hypothesis 5 and conclude that cross-pressured partisans are *less* likely to vote for the in-party compared to non cross-pressured partisans.

3.6 Discussion and Next Steps

In this chapter I have focused on partisans who were either negative about the economic performance of their in-party president or positive about the economic performance of the out-party president in three US presidential elections, 1992, 2000 and 2004. First, I have shown that the number of partisans who are cross-pressured in this manner is substantive. This is especially true when the economy is either performing very poorly or very well. This suggests that: ‘partisan rationalization and its consequences are not fixed.’⁵³ This is good news from the perspective of democratic accountability because it suggests that even partisans are not ‘fools.’⁵⁴ Partisans are *receptive* to the economic signal, especially when the economic signal is unambiguous. As expected, this receptiveness is moderated by the strength of a voters’ partisan affiliation to their party (H1). Yet, this is true mostly for *negative* information about the in-party. Partisans of various levels of affiliation are equally likely (or unlikely) to accept that the out-party president has improved the state of the economy. I did not find any evidence to suggest that partisans with moderate levels of political knowledge were more likely to receive and accept economic information (H2).

The second main finding of the chapter is that once partisans are cross-pressured, they are *less* likely to vote for their in-party and more likely to switch to the other party (H5), whereas there is no effect on their likelihood to abstain from voting (H4). I find that this pattern is consistent across all three elections under study and is robust to alternative modeling strategies. Cross-pressure had a substantive effect (20-40%) on increasing the likelihood of a party-switch among cross-pressured partisans across all presidential elections under study. This supports existing literature on the existence of the economic vote. Specifically, it suggests that once partisans accept economic

⁵³Parker-Stephen (2013, p.1087)

⁵⁴As also argued by Key (1966).

information that contradicts their partisan loyalty, they act upon this information even if this means voting for the other party. This chapter has hereby presented a different perspective on partisanship and economic voting by exploring the conflict rather than the alignment between these two motivations of vote choice.

Throughout the chapter I have drawn attention to potential methodological concerns about the measurement of economic-partisan cross-pressure. I have presented various pieces of evidence that suggest that partisans *do* update their retrospective economic evaluations, whilst maintaining their partisan identity (Table 3.5). Throughout the regression analyses I have furthermore presented a range of robustness checks to validate the results presented here. Nevertheless, it is not possible to entirely eliminate these concerns with the data at hand. The subsequent chapters therefore use three different strategies that attempt to offer more exogenous measures of cross-pressure.

First, in the next chapter, Chapter 4, I again use survey data that captures voters' evaluations of incumbent competence, but I complement this with expert measures of incumbent competence. In Chapter 5, I use an external 'performance shock', the Watergate scandal, to explore cross-pressure. In the case of Watergate the negative performance signal is thus not in the eye of the beholder (the voter), but is externally administered. In the final empirical chapter, Chapter 6, I use a lab experiment in which participants receive a cross-pressure treatment. The alternative strategies used in the subsequent chapters thus aim to address the concerns about measurement bias that might still remain despite the robustness checks presented here.

Several other questions remain unanswered in this chapter. In order to gain a more complete understanding of when democratic accountability is exercised, it is important to understand why some partisans *did* respond to the economic signal in 1992 and

2000, but others didn't. The strength of partisan affiliation was shown to be the most important predictor of this. Yet, this then raises the question as to why some partisans develop strong attachments and others don't. Another question that remains is whether the political context of an election affects the behaviour of cross-pressured partisans. I explore this latter question in the next chapter where I will contrast the behaviour of cross-pressured partisans in highly polarised districts with those in non-polarised districts and also contrast highly competitive with non-competitive elections.

Table 3.14: Evidence for Hypotheses Tested in Chapter 3

Hypothesis	Evidence
1. Strong partisans are less likely to be cross-pressured	✓
2. Political knowledge moderates the likelihood of cross-pressure	X
3. Clear information signals affect the number of cross-pressured voters	✓
4. Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans	✓
5. Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans	✓

Chapter 4

Candidate Competence Cross-Pressure in the 2006 Midterm Elections

We begin with what is perhaps the most basic, yet underinvestigated, hypothesis about voting choice in House elections: voters prefer high-quality candidates over scoundrels; the well-qualified over the incompetent.

- Buttice and Stone (2012, p.871) *The Journal of Politics* -

4.1 Introduction

In this chapter I shift my focus from presidential to Congressional elections in order to explore the effect of cross-pressure on voting behaviour in different electoral contexts. A study of the behaviour of cross-pressured partisans across electoral districts allows me to vary the type of electoral context, specifically the level of ideological polarisation and competitiveness of an election, whilst holding national-level developments constant.¹ It is important to examine the effect of contextual factors on the behaviour of cross-pressured partisans because it offers a more complete understanding of the

¹Although of course national level trends might have a differential effect at the district level, i.e. an economic recession might hit harder in some districts as compared to others.

conditions under which cross-pressure matters most to electoral outcomes - and when these effects might be muted.

In this chapter I use the *2006 Congressional Election Study* by UC Davis for my analyses. I focus specifically on the elections for the House of Representatives.² This data has two important advantages over other data on local elections. First, the UC Davis data on the 2006 House elections provides information about *both* incumbent candidates *and* their challengers. Although in theory voters decide between incumbents and challengers, many studies in political science only use information about the incumbent in their models due to data limitations. The *2006 Congressional Election Study* is a positive exception to this. A second advantage of the data is that it includes candidate evaluations by individual voters as well as candidate evaluations provided by a panel of political experts. This is important because it allows me to create two different measures of cross-pressure: first, a measure that uses voters' subjective evaluations of incumbent and challenger competence. Second, a more exogenous measure that relies on candidate competence evaluations provided by political experts. Because voters' own evaluations of incumbents and challengers tend to be biased, I replicate the analyses in the chapter with the expert measure as a robustness check of the results.³

Throughout the chapter, cross-pressure is conceptualised as a conflict between a voter's partisan identification and the competence ascribed to the incumbent and challenger running for House Representatives. A voter is considered cross-pressured when their partisan affiliation is in *conflict* with the competence evaluation they (or

²Stone, Walter J. 2010. 2006 Congressional Election Study. University of California, Davis, CA. (<http://electionstudy.ucdavis.edu/>)

³See for a discussion on problems with using voters' perceptions of candidate ideological positions Jessee (2009) and voters' evaluations of candidate performance Evans and Andersen (2006); Evans and Pickup (2010); Bartels (2002); Gerber et al. (2010)

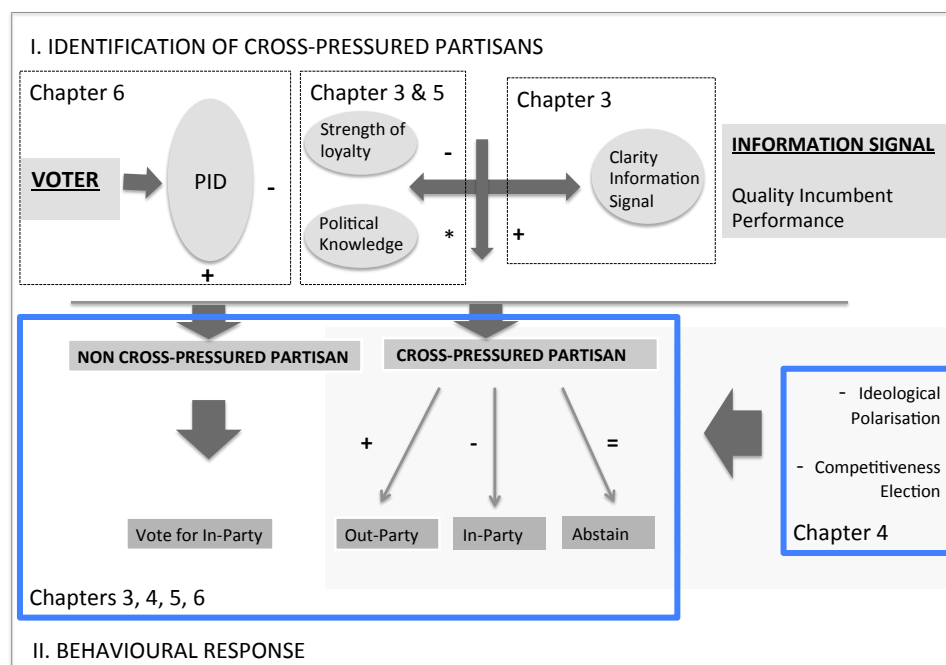
the experts) have provided of the incumbent and challenger. Thus, if a voter identifies with the party of the incumbent, but the challenger is deemed *more* competent, this voter is cross-pressured. The reverse is true for a voter who identifies with the party of the challenger candidate. In the chapter I first re-test Hypotheses 4 and 5 (abstention and party-choice) and then test the two contextual hypotheses (H6 and H7) this is shown below in Figure 4.1 and listed in Table 4.1.

Table 4.1: Hypotheses Tested in Chapter 4

Behavioural Response of Cross-Pressure Partisans
H4: Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans
H5: Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans
Contextual Effects on the Behaviour of Cross-Pressured Partisans
H6: High levels of ideological polarisation <i>reduce</i> the likelihood that a cross-pressured partisan votes for the out-party
H7: High levels of electoral competitiveness <i>reduce</i> the likelihood that a cross-pressured partisan votes for the out-party

The chapter proceeds as follows. First, in section 4.2 I briefly discuss the existing literature on voting behaviour in House elections and the interaction between partisan or ideological concerns and candidate quality in the context of Congressional elections. Second, in section 4.3, I discuss the data I use and the operationalisation of the main variables. In section 4.4, I estimate the number of cross-pressured partisans in the 2006 House Elections and test their behavioural response (H4 and H5). In this section I compare and contrast the results of *both* voter and expert evaluations of candidate competence. Then, in section 4.5, I explore whether the response of cross-pressured partisans varies across electoral context. Specifically I test whether the response of cross-pressured partisans is more moderate in districts that are highly polarised and districts that are considered competitive. The final section 4.6 presents reflections on the results and concludes.

Figure 4.1: Framework of Cross-Pressured Voting - Chapter 4



4.2 Voting in Congressional Elections

An important assumption about Congressional elections is that they are dominated by incumbents and money. Many studies have shown that experience, name recognition and the ability to attract donors and mount an effective campaign are crucial determinants of electoral success in Congressional elections.⁴ As a consequence, the barrier for new candidates to enter a local race and challenge incumbents is high.⁵ Perhaps partially because of the dominance of incumbents, the level of voter interest in Congressional elections tends to be low and voters' knowledge of candidates' issue positions is thought to be minimal.⁶

Yet, recent studies have disputed this view on Congressional elections.⁷ These studies contend that, similar to national level elections, both ideological and performance concerns matter to voters who vote in House elections.⁸ These studies find that voters elect representatives that share their ideological views even in a low-information context, mostly due to voters' reliance on party cues.⁹ Furthermore, voters *punish* candidates who move too far to either end of the ideological spectrum or candidates who vote for unpopular policy proposals.¹⁰

In addition to their concern for partisan or ideological representation, recent studies show that voters in House elections also care about the quality of incumbent performance and seek to elect candidates who are thought to be competent and skilled.¹¹ The authors of these studies suggest that many previous studies on House elections

⁴Jacobsen and Kernell (1983); Cox and Katz (1996); Gelman and King (1990); Jacobsen (2009)

⁵Stone et al. (2004)

⁶Jacobsen (2007); Mondak (1995); Buttice and Stone (2012)

⁷Also see Mondak (1995) for an earlier critique of the literature and empirical evidence that candidate quality matters to midterm elections in the period 1969-1981.

⁸Buttice and Stone (2012); Stone and Simas (2010); Simas (2013); Joesten and Stone (2014)

⁹Joesten and Stone (2014)

¹⁰Simas (2013); Canes-Wrone et al. (2002)

¹¹Buttice and Stone (2012); Stone and Simas (2010)

conflate qualities that are related to incumbency status, such as levels of spending and name recognition, with more general political competence and ability. Once these two dimensions of skills (campaigning versus governing) are separated however, it becomes clear that voters *do* value candidate competence above and beyond incumbent visibility or experience. As such the authors conclude that:

Despite copious evidence that voters in House elections are not attentive or well informed, we find that local candidate differences related to voters' fundamental interests help explain voting choice and vote shares.¹²

Thus, just as is the case with presidential elections, when voting for the House of Representatives, voters prefer to elect candidates who share their ideological viewpoints *and* have a track record of good performance. Voters thus weigh both representative as well as accountability concerns when voting in House elections. This dynamic is observed not just with voters, but is also reflected in the behaviour of candidates in midterm elections. Research shows that challenger candidates who hold a distinct quality advantage tend to *moderate* their policy positions in order to minimise ideological polarisation and maximise the weight voters will attribute to quality differences between candidates. This reflects the fact that candidates in House elections assume that voters care about *both* ideological representation and high quality government performance.¹³

In this chapter I thus assume that voters consider both the ideological and issue positions of the incumbent and challenger candidate, as well as their overall suitability or competence as local representatives in the 2006 House elections. In the next sections I proceed as follows: I first explore whether there are voters whose partisan loyalties

¹²Buttice and Stone (2012, p.871) See for a similar argument Stone and Simas (2010)

¹³See Buttice and Stone (2012); Adams et al. (2011); Groseclose (2001) In the case of Britain Green and Hobolt (2008) show that competition in terms of party competence matters more to voters' decision-making when the two major parties converge on a particular issue and are thus more ideologically similar.

conflict with their competence ratings of the incumbent and challenger candidates in the 2006 House elections. I then examine whether these cross-pressured partisans behaved differently at election time when compared to partisan votes whose evaluations *aligned* with their partisan identification. Finally, I explore whether this behaviour varied across districts as a result of varying levels of ideological polarisation and the competitiveness of the election.

4.3 Data and Methods

As mentioned, I use the 2006 *Congressional Election Study* by UC Davis.¹⁴ The data consists of a voter survey and an expert survey of House races in 2006. The expert survey consists of a sample of political experts in 155 House districts. 100 of the districts consist of a random sample, an additional 55 districts were added to include more competitive seats.¹⁵ All analyses presented here are weighted with provided sample weights. The political experts are sampled from 2004 delegates to the national party conventions. The benefit of using delegates is that they are well-informed about all candidates and the local campaigns. The downside of using party delegates as political experts is the fact that their opinions are likely coloured by their partisan biases. The authors of the data have therefore adjusted all expert measures to account for potential partisan bias and these ‘non-partisan’ measures are used.¹⁶ The expert ratings are aggregated at the district level. With regards to the voter survey, I use the the 2006 *Cooperative Congressional Election Study* which was run in conjunction with the expert survey of UC Davis. Thus, the expert survey can be

¹⁴See for other applications of this data Stone et al. (2004, 2010); Stone and Simas (2010); Adams et al. (2011); Buttice and Stone (2012)

¹⁵Also see Buttice and Stone (2012) who discuss the data at length on p.874-875.

¹⁶This is described in the appendix to Buttice and Stone (2012) footnote 10 and is done by regressing all expert scores on a dummy variable of experts’ partisan identification, these estimates are then subtracted from the expert responses.

mapped onto the voter survey. The total number of voter respondents is $N = 1,000$.¹⁷

In this chapter I measure cross-pressure as a conflict between a voter's partisan identification and their (or the experts') rating of the competence of the incumbent and challenger candidate. In line with the previous chapter, I exclude voters who identify as Independents which is true for 9.8% of voter respondents. In terms of the second element of cross-pressure, performance, I am limited by the available data as well as the fact that I now compare both incumbents and challengers. As argued by Downs (1957), when voters assess incumbent performance, they can only compare this with what they assume the challenger 'would have done' if they had been in office.¹⁸ No such measure is available in the survey however; voters were not asked to assess how they thought the challenger would have performed if they had been elected two years previously.

I thus have to rely on more general measures of candidate and incumbent competence to assess the assumed quality of government performance of both the incumbent and challenger candidate. Although the data contain a battery of competence and candidate quality related questions to both experts and voters, there is unfortunately very little overlap between the two surveys. I thus use the only question that was asked of both experts and voters which is: "how would you rate the competence of the candidate?" To be exact, voters were asked "how well does the following word - COMPETENT - describe the candidate?" and could answer on a scale of 1-4 where 1 = not well at all, 2 = not too well; 3 = quite well and 4 = extremely well. Political experts were asked to "please rate the following characteristic - COMPETENCE - of the Democratic/Republican candidate", whereby answer categories ranged between

¹⁷Stephen Ansolabehere, 2010, "CCES Common Content, 2006", available at <https://dataverse.harvard.edu/dataset.xhtml?persistentId=hdl:1902.1/14002>, data downloaded at March 4, 2013.

¹⁸Downs (1957)

1-7 ranging from “extremely weak” to “extremely strong.”

It is difficult to collapse these scales into a comparative measure and this is not necessary since I am interested in the *comparative* quality of incumbents and challengers. I thus keep the scales intact and, for both the expert and voter measure, create a variable that reflects the *quality differential* between incumbent and challengers. This variable is positive when the incumbent is more competent than the challenger and negative when the challenger is deemed to be more competent in comparison to the incumbent. Voters are then considered to be cross-pressured when their partisan identification and the quality differential *conflict*. This is depicted below in Table 4.2. Note that for a voter to be cross-pressured it is not just true that their preferred candidate is of *low* quality, rather the alternative candidate needs to also be considered to be of *higher* quality. Also, if the incumbent and challenger are considered *equally* competent, a voter’s partisan loyalties are thought to prevail and the voter is not considered to be cross-pressured. The estimated number of cross-pressured partisans using this approach thus reflects a conservative estimate.

Table 4.2: Operationalisation of Cross-Pressure 2006 Midterm Elections

Voter Identifies with	Most Competent Candidate	Cross-Pressured
Incumbent	Incumbent	No
Incumbent	Challenger	Yes
Incumbent	Equally Competent	No
Challenger	Challenger	No
Challenger	Incumbent	Yes
Challenger	Equally Competent	No

4.4 Results Part I: Cross-Pressured Partisans in the 2006 House Elections

As a first step, I explore whether there are many differences between the candidate evaluations provided by voters and experts. Figure 4.2 shows kernel density plots for the candidate ratings provided by voters and experts. For Figures 4.2a and 4.2b black lines depict incumbents and grey lines represent challengers. For Figures 4.2c and 4.2d blue lines represent ratings of Democratic candidates and red lines reflect competence ratings of Republican candidates.

A first observation is that voters rate incumbents and challengers approximately equally (Figure 4.2a). Experts on the other hand tend to rate incumbents (black lines) somewhat more positively compared to challengers (grey lines), this can be seen in Figure 4.2b. The average score of an incumbent using voters' evaluations is 2.55 (on a 4-point scale) compared to 2.65 for a challenger. The difference in means of voters' evaluations is not statistically significant. The difference in means is significant for expert evaluations however at $p < 0.001$. Experts give an average score of 4.9 (out of 7) to incumbents, compared to 4.6 to challengers. Assuming that the average incumbent is of higher quality than the average challenger, this suggests that the expert evaluations are somewhat more accurate when compared to those of voters.

If I split candidates into Democrats and Republicans, I find that both voters and experts rate Democratic candidates (blue lines) somewhat more competent than Republican candidates (red lines), this is visible in both Figures 4.2c and 4.2d. The difference in the average competence scores between Democratic and Republican candidates is statistically significant for both voter evaluations ($p < 0.001$) and expert evaluations ($p < 0.0001$). The average rating of Democratic candidates is 2.75 among voters (out of 4) versus 2.46 for Republican candidates. The average expert rating of Democratic

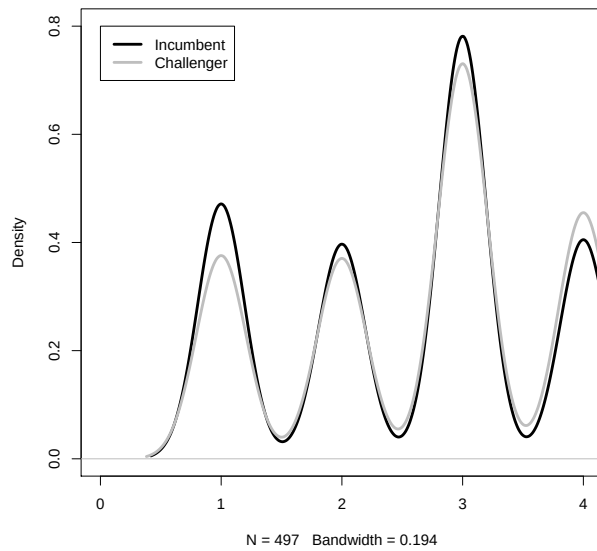
candidate is 5 (out of 7) compared to 4.6 for Republican candidates. The fact that *both* experts and voters rated Democratic candidates more competently compared to Republican candidates is in line with the fact that the 2006 midterm elections were held under a Republican presidency. Various factors such as the ongoing war in Iraq, President Bush's low approval ratings and a range of corruption scandals made 2006 a generally bad year for the Republican party.¹⁹ The lower competency ratings for Republican candidates may thus reflect the fact that the electorate, and experts, were generally more negative about the electoral prospects of the Republican party, or the fact that high-quality Republican candidates were dissuaded from running because they thought chances of success for Republican candidates were low in these elections.²⁰

¹⁹Mondak and Mitchell (2009); Jacobsen (2007)

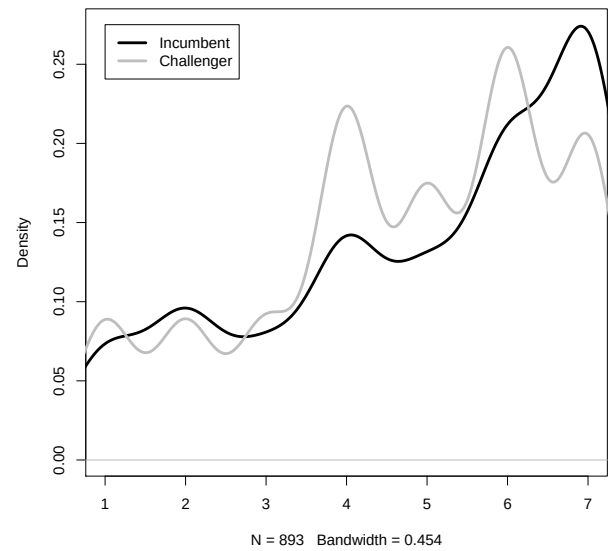
²⁰See Mondak and Mitchell (2009); Jacobsen (2007)

Figure 4.2: Voter and Expert Ratings of Candidate Competence 2006 Midterm Elections

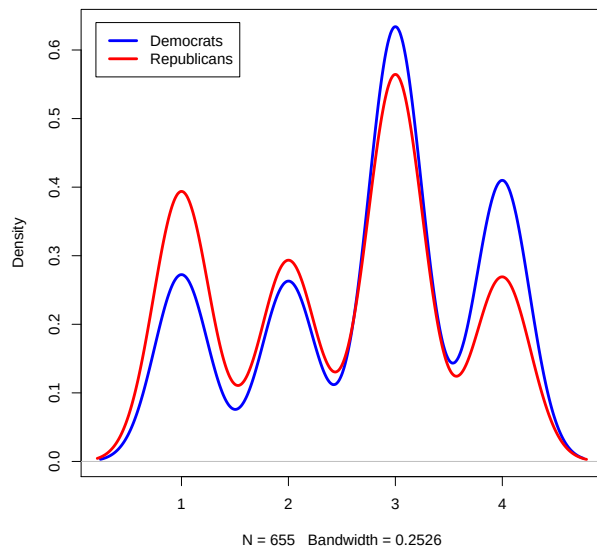
(a) Voter Evaluations: Incumbents and Challengers



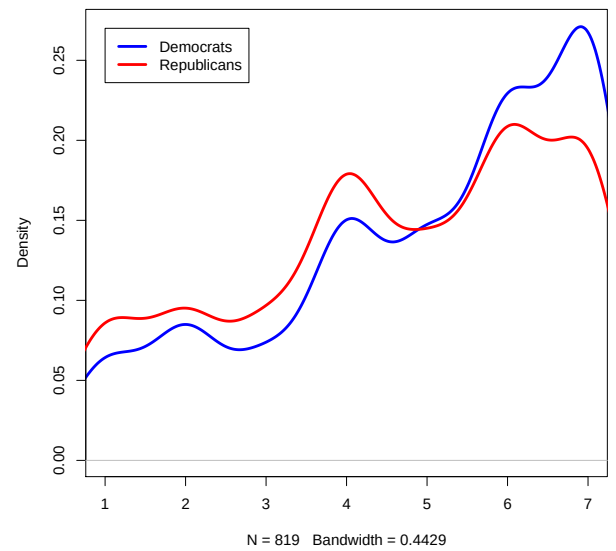
(b) Experts Evaluations: Incumbents and Challengers



(c) Voter Evaluations: Democratic and Republican Candidates



(d) Expert Evaluations: Democratic and Republican Candidates

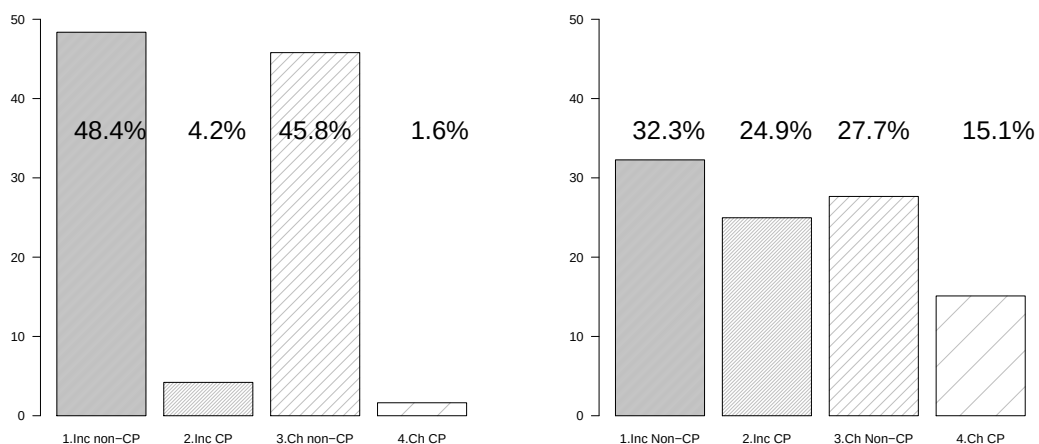


4.4.1 Estimating the Number of Cross-Pressured Partisans in 2006

I now estimate the number of cross-pressured partisans in the 2006 House elections in line with the operationalisation presented in Table 4.2. Thus, partisan identification is coded toward either the incumbent or the challenger and this is contrasted with the perceived competence of both the incumbent and challenger. Figure 4.3 displays the estimated number of voters who are cross-pressured using both voters' evaluations of candidates (Figure 4.3a) and experts' evaluations of candidates (Figure 4.3b). The barplots reflect voters who identify either with the incumbent party (bars 1 and 2) or the challenger party (bars 3 and 4) and splits these into cross-pressured partisans (bars 2 and 4) and non cross-pressured partisans (bars 1 and 3). Figure 4.3 suggests that there is a large discrepancy between the estimated number of cross-pressured partisans when using voter ratings of candidates compared to candidate ratings provided by experts.

Figure 4.3: Estimated number of Cross-Pressured Partisans in 2006 US Midterm Elections

(a) Using Voters' Evaluations of Candidate Competence (b) Using Experts' Evaluations of Candidate Competence



As may be expected, the number of cross-pressured voters is much higher when using experts' evaluations of candidate competence compared to using voter evaluations which are more subjective and biased. When I rely on candidate competence ratings provided by experts, I find that approximately 40% of partisans are cross-pressured. When I use the voter measure of candidate competence, the number of cross-pressured partisans is only 5%. The discrepancy between voter and expert measures is in line with the extensive literature on the effect of partisan bias on performance (or in this case competence) evaluations: partisans are unlikely to provide positive ratings of the out-party candidate and are disproportionately likely to provide positive ratings of in-party candidates, regardless of the actual level of competence of the competing candidates.²¹ Interestingly, in both cases the proportion of cross-pressured partisans who identify with the *incumbent* is higher compared to the number of voters who identify with the challenger and are cross-pressured. To be precise 44% of incumbent partisans are cross-pressured when using the expert measure, compared to 35% of challenger partisans. For the voter measure this is 8% and 3.5% respectively. In general these estimates are in line with the fact that the 2006 midterms involved many Republican incumbents who were facing a 'bad year'.²² This could suggest that general negative sentiments toward the Republican party (the incumbents) translated to low competence ratings.

4.4.2 The Behaviour of Cross-Pressured Partisans

In this section I test Hypotheses 4 and 5 and explore whether cross-pressured partisans were more likely to abstain in the 2006 House elections (H4) and whether they were more likely to vote for the out-party (H5). Table 4.3 below presents the results

²¹See for several studies on the effects of partisan bias of performance evaluations Bartels (2002); Marsh and Tilley (2009); Tilley and Hobolt (2011); Evans and Andersen (2006); Pickup and Evans (2013); Gerber et al. (2010).

²²Mondak and Mitchell (2009); Jacobsen (2007)

of binary logistic regression models. In models 1 and 2, the dependent variable takes the value of 1 when a voter abstained in 2006 and the value of 0 otherwise. In total 11% of the respondents who answered this question ($N = 843$) stated that they did not vote in the House elections. In models 3 and 4 the dependent variable takes the value of 1 if a voter voted for the in-party - the party they identify with and the value of 0 if they did not vote for this party. In Models 3 and 4 non-voters are excluded.

I include a range of standard demographic controls (age, gender, ethnicity, education and income). In addition to these demographic controls, I also control for a range of variables that are known to be important to Congressional elections.²³ I thus control for levels of campaign spending of both candidates (in USD 100,000) and include a variable that reflects economic satisfaction of voters. Ideally I would also control for levels of presidential approval to account for a potential backlash against President Bush or the Iraq war. Unfortunately however, only half of the respondents answered the approval rating question. In order to capture anti-Republican party sentiments I therefore include a dummy variable that reflects whether or not the incumbent in the district is Republican. Standard errors in the models are clustered at the district level. Note that the models in which voters' evaluations of candidate competence levels are used show a reduced number of observations. This is because only a small number of voters ($N = 428$ out of $N = 1,000$) rated *both* the incumbent and challenger candidates on their competence.

²³The control variables I include follow the approach taken in the following studies: Jacobsen (2007); Simas (2013); Buttice and Stone (2012)

Table 4.3: Binary Logistic Regression Model Effect Cross-Pressure on Abstention and Vote Choice in 2006 House Elections

	Abstention		In-Party Vote	
	Model 1	Model 2	Model 3	Model 4
Cross-Pressure - Voter Evaluations	0.65 (0.95)	— —	−3.68*** (0.86)	— —
Cross-Pressure - Expert Evaluations	— —	−0.17 (0.36)	— —	−0.85* (0.39)
Age	−0.04 (0.02)	−0.06*** (0.02)	0.02 (0.02)	−0.01 (0.01)
Female	0.19 (0.56)	0.70 (0.44)	0.89 (0.74)	0.56 (0.32)
Non-White Ethnicity	0.83 (0.65)	0.70 (0.56)	0.47 (1.03)	0.96 (0.63)
College degree	−0.72 (0.63)	−0.83* (0.40)	0.73 (0.72)	−0.36 (0.48)
<i>base: leaning partisan</i>				
Moderate Partisan	−0.33 (0.78)	−0.16 (0.42)	−1.06 (0.80)	−1.24* (0.49)
Strong Partisan	−0.21 (0.52)	−0.86 (0.45)	−0.20 (0.73)	0.23 (0.57)
<i>base: below 25,000</i>				
Income 25,000-60,000	1.89 (1.26)	0.95 (0.64)	−0.05 (1.16)	−0.53 (0.67)
Income 60,000 - 100,000	1.74 (1.28)	0.74 (0.61)	0.08 (1.19)	0.02 (0.77)
Income 100,000 and above	1.44 (1.35)	1.13 (0.71)	−0.52 (1.46)	−0.25 (0.88)
<i>base: economy stayed the same</i>				
Economy got worse	−0.84 (0.74)	−0.12 (0.45)	0.06 (0.95)	0.36 (0.49)
Economy got better	−0.43 (0.60)	−0.36 (0.53)	0.12 (0.85)	0.71 (0.43)
Republican Incumbent	0.18 (0.62)	1.15* (0.48)	0.44 (0.85)	0.16 (0.54)
Spending Democratic Candidate	−0.01 (0.03)	0.03 (0.03)	0.05 (0.04)	−0.01 (0.03)
Spending Republican Candidate	−0.02 (0.03)	−0.04 (0.02)	−0.02 (0.03)	0.01 (0.02)
Constant	−1.25 (2.31)	−1.39 (1.33)	−0.18 (2.23)	2.88* (1.29)
N	306	544	283	496
Pseudo R-Squared	0.09	0.13	0.23	0.12
Wald Chi Square Statistic	10.91*	32.25***	32.06***	48.24***

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

Standard errors clustered at the district level

Table 4.3 displays the results for these analyses. In line with the findings of the previous chapter (Chapter 3 on economic cross-pressure) Models 1 and 2 suggest that there is no significant effect of cross-pressure on the likelihood of abstention in the 2006 House elections. This is true both when using voters' evaluations of candidate competence (Model 1) as well as when using expert ratings' of candidate competence to create a measure of cross-pressure (Model 2). I thus accept Hypothesis 4 and conclude that cross-pressured partisans are no more (or less) likely to abstain from voting compared to non cross-pressured partisans. Note that further exploration shows that this is true *both* for cross-pressured partisans who identify with the incumbent as well as those who identify with the challenger. This becomes clear when the models are run with cross-pressure as a categorical rather than a binary variable, this model specification is provided in the Appendix, Table A.6 and Table A.7.

Models 3 and 4 in Table 4.3 explore Hypothesis 5: are cross-pressured partisans *less* likely to vote for the in-party? Model 3 uses voters' evaluations of candidates and Model 4 uses expert ratings of candidate competence. Note that in the previous chapter I explored this hypothesis by testing whether cross-pressured partisans were more likely to alter their voting behaviour in-between presidential elections and thus assessed the effect of cross-pressure on a vote *switch*. In this chapter I rely on cross-sectional data and I am thus unable to explore vote-switching across time. Here I therefore explore whether cross-pressure affects the likelihood that a voter voted for the opposing party in the 2006 House elections. In total 8.6% of the sample voted for a party that was not the party they identified with ($N = 660$).

Models 3 and 4 in Table 4.3 confirm that cross-pressure has a significant and negative effect on the likelihood of voting for the in-party. This thus confirms Hypothesis 5: cross-pressured partisans are significantly *less* likely to vote for their in-party, the

party they identify with. Again this is true both when using voters' evaluations of candidate competence (Model 3) and when using expert evaluations (Model 4). When I re-estimate Models 3 and 4 with a categorical version of cross-pressure (rather than a binary version) I find that incumbent partisans who are cross-pressured are significantly less likely to vote for the in-party compared to non cross-pressured incumbent partisans regardless of the type of evaluations used.²⁴ Yet, cross-pressured partisans who identify with the challenger candidate are significantly less likely to vote for the in-party compared to non cross-pressured challenger partisans only when using voters' evaluations of candidates. These model specifications are provided in the Appendix, Tables A.6 and Table A.7.²⁵ This suggests that although the number of cross-pressured partisans is lower when using voters' evaluations of candidate competence, the effect of cross-pressure is more pronounced when using this measure as opposed to experts' evaluations of candidate competence.

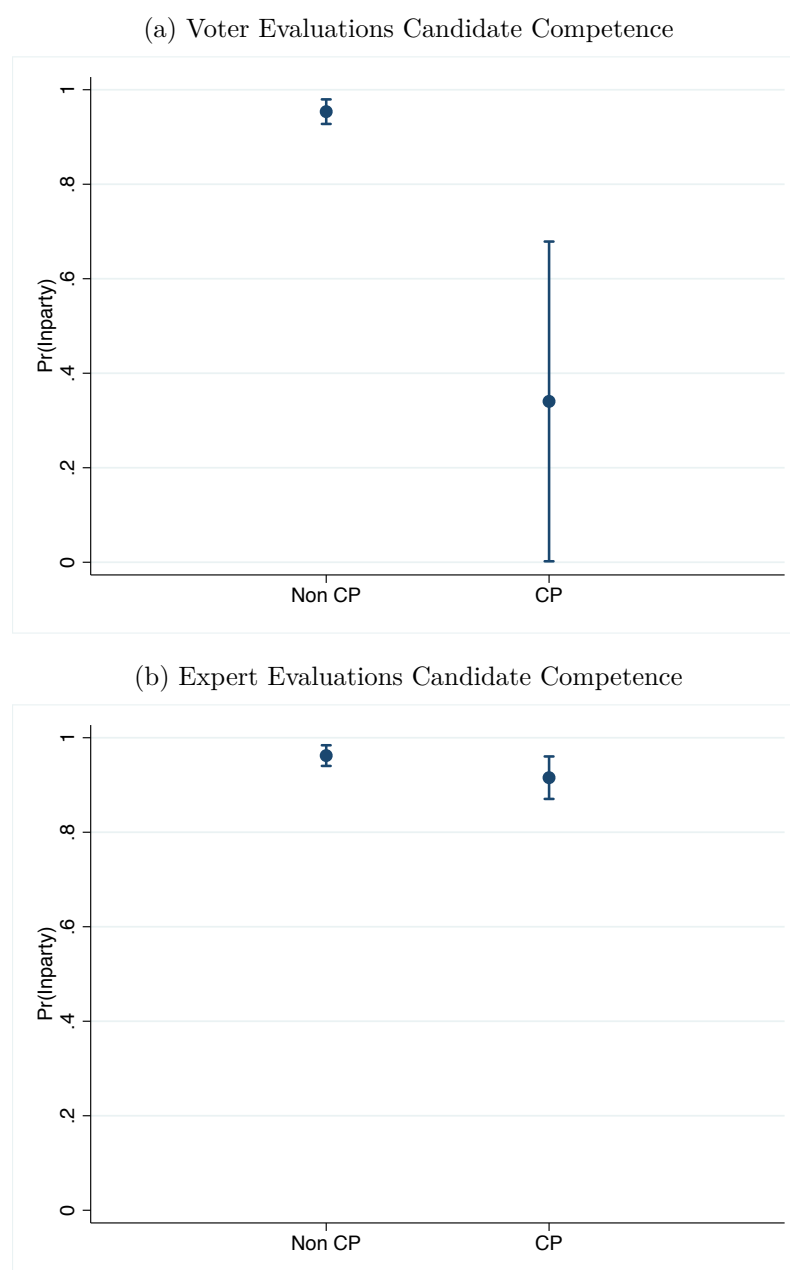
Because logistic regression coefficients are not intuitive to interpret, I calculate the predicted likelihood that a cross-pressured partisan votes for the party they identify with in the 2006 House elections and compare this with the predicted likelihood of an in-party vote of a partisan voter whose partisan identity and competence evaluations align. I use the estimates from Models 3 (voter evaluations) and 4 (expert evaluations). The results are shown in Figure 4.4, all other variables are set to the mean. The top panel suggests that when I rely on voters' evaluations of candidates, a partisan voter whose party identification aligns with their competence rating of their preferred candidate has a likelihood of voting for the in-party candidate of 95%. This is high as expected because this voter has little reason to consider voting for the opposing candidate. Importantly, the likelihood of an in-party vote drops to 34% if this voter rates the opposing candidate as more competent. The bottom panel suggests

²⁴With $p < 0.000$ (voter evaluations candidates) and $p < 0.06$ (expert evaluations candidates).

²⁵With $p < 0.012$ (voter evaluations candidates), but $p < 0.97$ (expert evaluations candidates).

a much more moderate effect when using experts' ratings of candidates. Here, the likelihood of an in-party vote is 96% for a non cross-pressured partisan and drops to 91.5% for a cross-pressured partisan, holding all other variables in the model at their mean.

Figure 4.4: Predicted Likelihood of Voting for the In-Party for Cross-Pressured and Non Cross-Pressured Partisans in the 2006 House Elections



The discrepancy between the two substantive effects suggests that voters' own evaluations are both more biased, but are also more influential to vote choice. The number of cross-pressured partisans is much lower when I use voters' competence scores of candidates, but for the voters who 'admit' to a conflict between their partisan loyalty and their impression of the competence of the two candidates, the substantive effects are very large (i.e a drop in the likelihood of an in-party vote of 60%). The number of cross-pressured partisans is much larger when I use objective competency ratings provided by political experts, but the consequent effect of these expert ratings on vote choice is minor (4.5%).

In line with the previous chapter, the findings in Table 4.3 suggest that cross-pressured partisans are no more likely to abstain from voting compared to non cross-pressured partisans. What is more, when they vote, cross-pressured partisans are significantly *less* likely to vote for the party they identify with. This again suggests that there are conditions under which partisans vote for the opposition party in light of performance information. There is an important caveat to these analyses however, they do not account for the fact that the electoral context might have an important effect on the behaviour of partisans. I therefore now explore the behaviour of cross-pressured partisans across different electoral contexts.

4.5 Results Part II: The Conditional Effects of Ideological Polarisation and Electoral Competitiveness

Existing research suggests that partisans attach *more* weight to their partisan loyalties in situations where the partisan stakes are high.²⁶ I therefore now explore whether voters are responsive to the electoral environment in which they vote.

4.5.1 Ideological Polarisation

The American party system has become increasingly polarised across all levels of government.²⁷ To the extent that polarisation increases the perceived differences between the Democratic and Republican party, increased ideological polarisation is thought to have a *negative* effect on the functioning of democratic accountability because it raises the ideological costs of voting for the opposing party. If the (perceived) differences between the two major parties are very large, then it becomes less likely that voters accept negative information about their own party. Moreover, it becomes more difficult for cross-pressured partisans to actually clear the hurdle of voting for the opposition under these conditions. To test this mechanism, I explore whether cross-pressured partisans who live in highly polarised districts are in fact *less* likely to abandon their own party compared to cross-pressured partisans who vote in districts with low levels of polarisation.

As a measure of polarisation I use the fact that political experts were asked to place both incumbents and challengers on an ideological scale ranging from 1-7, where 1 =

²⁶See for instance Eggers (2014) who shows that partisans in the UK were more willing to excuse MPs who were implicated in the 2010 UK Expenses Scandal when the MP faced strong opposition from either the Labour or Conservative Party. Voters were significantly more likely to punish MPs when they faced a Lib-Dem candidate.

²⁷For studies that discuss the increased polarisation of American elections see Ansolabehere et al. (2001); Abramowitz and Saunders (2008); Hetherington (2001)

very liberal and 7 = very conservative.²⁸ To measure polarisation, I take the absolute distance between incumbents and challengers. The resulting polarisation variable ranges between 1-7 where low values reflect a low level of ideological polarisation and high values indicate high levels of ideological polarisation. The mean of the polarisation variable is 2.2 with a standard deviation of 1.19. In order to test the effect of this polarisation variable on the behaviour of cross-pressured partisans, I include this variable as an interaction term in the models that were presented in Table 4.3. The results of this analysis are displayed below in Table 4.4. The dependent variable in these analyses is again in-party vote. The coefficients represent binary logistic regression coefficients. Due to space considerations, only the main variables are displayed, the full table is provided in the Appendix, Table A.8.

Table 4.4: Binary Logistic Regression Model: Effect of Ideological Polarisation on Vote Choice Cross-Pressured Partisans in 2006 House Elections

	Model 1	Model 2
Cross-Pressure - Voter Evaluations	-4.18* (1.96)	— —
Cross-Pressure Expert Measure	— —	-1.46 (0.87)
Level of Ideological Polarisation	0.62* (0.32)	0.00 (0.22)
Interaction CP * Ideological Polarisation	-0.37 (0.76)	0.27 (0.34)
Constant	-2.46 (3.44)	2.99 (1.48)
N	264	492
Pseudo R-Squared	0.34	0.13
Wald Chi Square Statistic	44.42***	52.17***

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

Standard errors clustered at the district level

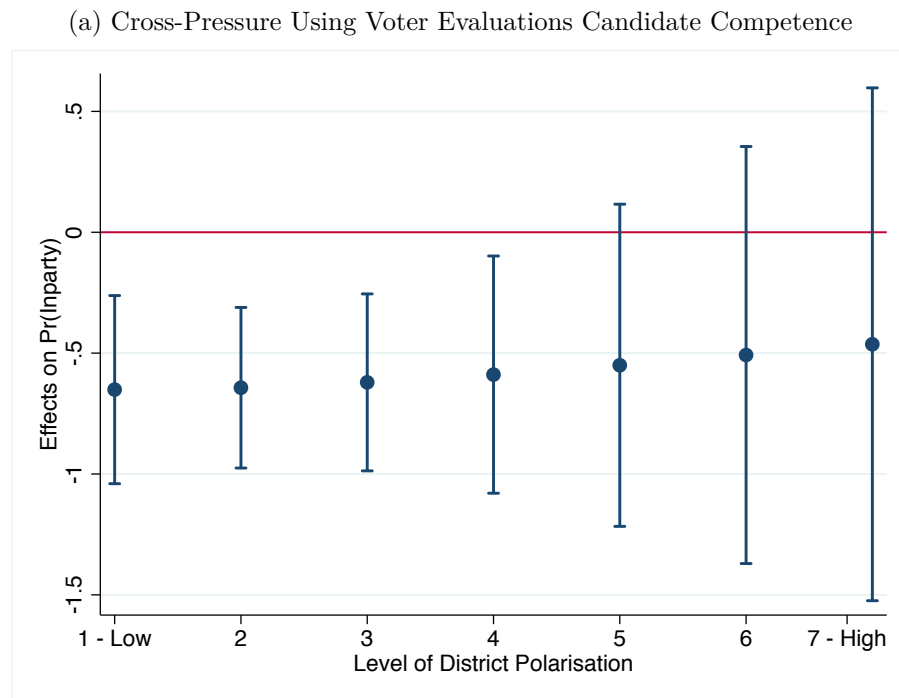
Model includes control variables, see the Appendix Table A.8 for a full display of the table

²⁸Note that here I use expert placement of candidates' ideological positions rather than voter perceptions of candidate placements to avoid introducing any more bias into the model related to voter perceptions. A discussion of the problems with using voter perceptions of candidate positions can be found in Jessee (2010)

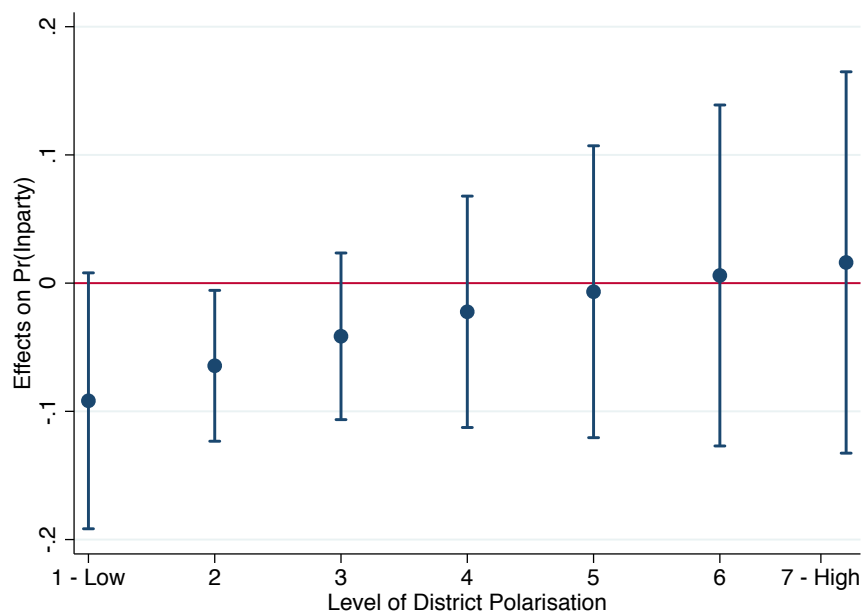
Table 4.4, Model 1 presents the results of the conditional effect of the level of ideological polarisation on the cross-pressure measure that relies on voters' own evaluations of candidate competence. Model 2 displays the effects on the cross-pressure measure that relies on expert ratings of candidates. The interaction term between ideological polarisation and the voter and expert measures of cross-pressure are not significant. Because this is a logistic regression model however, it could be that the interaction is significant at some levels of the interaction variable but not others. I therefore explore the interaction term graphically and plot the marginal effects of cross-pressure across the scale of ideological polarisation. The visual representation of the marginal effects of cross-pressure on the likelihood of an in-party vote across various levels of ideological polarisation are shown in Figure 4.5. In keeping with the main regression results presented above, the effects of cross-pressure are much stronger when using voters' evaluations of candidate competence. Note therefore that the scale of the y-axis are different for Figure 4.5a and Figure 4.5b.

For the voter measure of cross-pressure we clearly observe that the effect of cross-pressure on in-party voting is significant *only* at low levels of ideological polarisation (Figure 4.5a). This is true for a lesser extent when using the expert measure (Figure 4.5b). In general, both measures display the same pattern: once the incumbent and challenger candidate are further removed from each other on the ideological spectrum, cross-pressure does not reduce the likelihood of an in-party vote in highly polarised districts, but only has a significant effect in elections with low levels polarisation. This confirms Hypothesis 6 and suggests that the effect of cross-pressure is dependent on the electoral context. In line with expectation, higher levels of ideological polarisation thus *reduce* the functioning of the accountability mechanism.

Figure 4.5: Marginal Effects of Cross-Pressure on In-Party Vote Choice at Various Levels of District Polarisation



(b) Cross-Pressure Using Expert Evaluations of Candidate Competence



4.5.2 Electoral Competitiveness

As discussed in Chapter 2, section 2.6.6, a second indicator of high partisan stakes is the competitiveness of an election. Intuitively, if a district is very competitive, then the chances that the in-party candidate loses are higher, potentially dissuading a partisan voter from voting for the challenger, regardless of the cross-pressured experienced. The fact that an individual vote carries more weight in competitive elections is thought to further decrease the likelihood that a partisan voter is willing to vote against the in-party. As such, cross-pressured partisans are thought to be less likely to vote against their own party when the local election is considered to be very close.

In the survey, the political experts were asked to predict the competitiveness of the House seat. Experts could choose from three answer categories: “very close”, “somewhat close” and “not close at all.”²⁹ Because the answers provided by the experts are aggregated to the district level, this categorical survey question is transformed into a continuous variable at the district level. Answers range between 1-3 whereby low values indicate that, on average, experts thought the race would not be very competitive and high values indicate that, on average, the district experts thought the race would be competitive. I again run a binary logistic regression model with dependent variable in-party vote choice. The main independent variables of interest are cross-pressure and its interaction with district competitiveness. The results are displayed in Table 4.5. Appendix Table A.9 presents the full model including individual level control variables.

²⁹Note that here no equivalent question was asked of voters. Voters were asked to predict the percentage of votes the winner of the election would attain, but it is difficult to construct a measure of competitiveness based on this survey question. First, the answer categories are very broad, such that the most interesting range of votes is captured in one broad category: 51-60%. Furthermore, there is little variation with 50% of voters stating they expect the winner to receive between 51-60%.

Table 4.5: Binary Logistic Regression Model: Effect of Electoral Competitiveness on Vote Choice Cross-Pressured Partisans in 2006 House Elections

	Model 1	Model 2
Cross-Pressure - Voter Evaluations	−3.37 (5.56)	— —
Cross-Pressure Expert Measure	— —	−0.54 (1.14)
District Competitiveness	1.21 (0.90)	0.35 (0.59)
Interaction CP * District Competitiveness	−0.32 (2.46)	−0.18 (0.60)
Constant	−2.47 (2.43)	2.51 (1.51)
N	277	496
Pseudo R-Squared	0.27	0.12
Wald Chi Square Statistic	35.84**	53.83***

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

Standard errors clustered at the district level

Model includes control variables, see the Appendix Table A.9 for a full display of the table

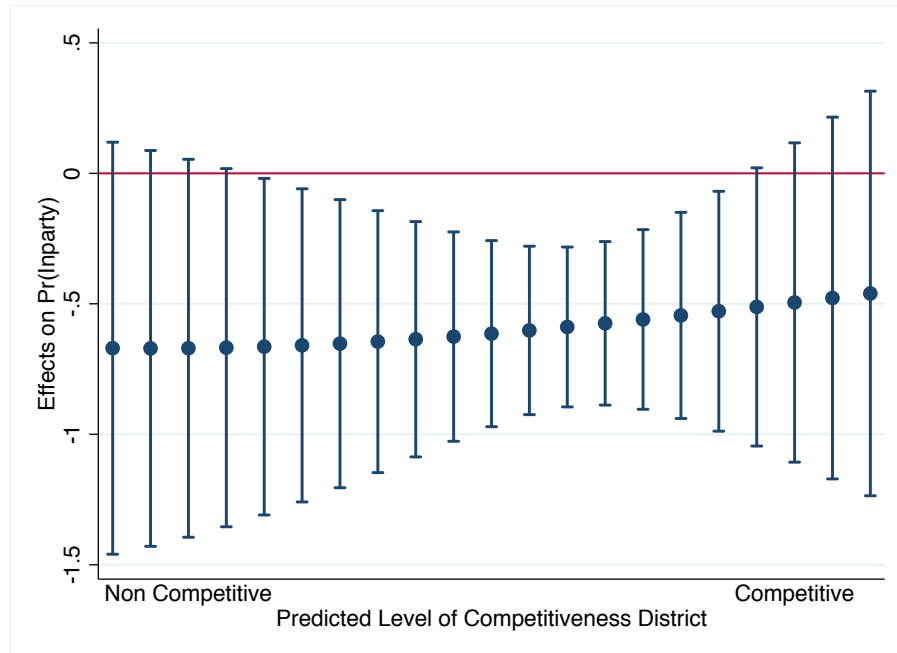
The interaction term between cross-pressure and the competitiveness of a district is not significant. I again present a visual representation of the interaction term to interpret the marginal effects of cross-pressure across various levels of predicted competitiveness of the district. This is displayed in Figure 4.6. In line with the findings presented above, I find a much stronger effect when I use the cross-pressure measure that relies on voters' evaluations of the competence of candidates when compared to the expert ratings of competence. Again therefore the scales of the y-axes are different for Figure 4.6a and Figure 4.6b.

Interestingly, when using voters' evaluations of candidate competence, it becomes clear that the effect of cross-pressure on voting for the in-party is not significant in districts that are considered either not competitive at all, or districts that experts consider to be very competitive. Rather, the effects of cross-pressure on voting are strongest in the middle section of the graph (Figure 4.6a). Using the expert measure, the interaction only borders on significance in the middle section of the graph, (Fig-

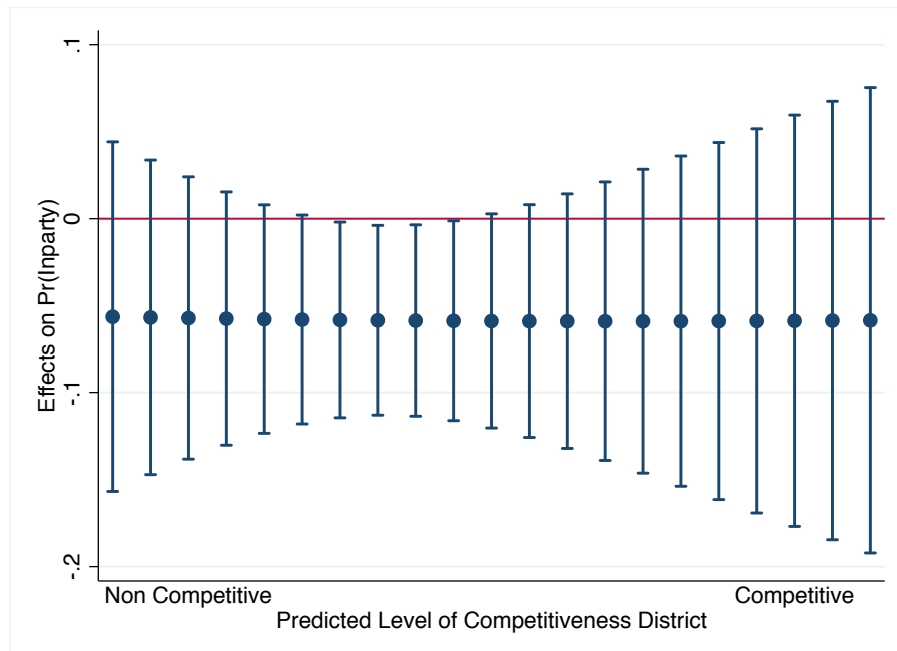
ure 4.6b), and the substantive effects are minimal when using the expert measure. The voter measure however suggests that negative perceptions of the competence of the in-party candidate matter most when voters perceive a sense of real competition. Furthermore, it seems that when the partisan stakes are too high, and an election is very competitive, partisan loyalties prevail. This confirms the fact that cross-pressure does not affect the likelihood of in-party voting in districts that are at the right end of the scale and are considered very competitive. I therefore accept Hypothesis 7.

Figure 4.6: Marginal Effects of Cross-Pressure at Various Levels of Predicted Competitiveness of House Election

(a) Cross-Pressure Using Voter Evaluations Candidate Competence



(b) Cross-Pressure Using Expert Evaluations of Candidate Competence



4.6 Discussion and Next Steps

In this chapter I have applied the concept of cross-pressure to local races for the House of Representatives. I have shown that partisans may face cross-pressure in local races between their loyalty to a party and the perceived competence of both the incumbent and challenger candidate. Through the use of both voter and expert evaluations, it becomes clear that the partisan filter has a strong effect on reducing potential cross-pressure. The estimated number of cross-pressured partisans in the 2006 House elections is much higher when using expert measures of candidate competence (40%) when compared to using voters' own evaluations of candidate competence (6%). The analyses on voting behaviour conducted in this chapter again confirm that cross-pressured partisans are *not* more likely to abstain from voting. Furthermore, again in line with the previous chapter, I find that cross-pressured partisans are *less* likely to vote for the party they identify with compared to partisans whose candidate evaluations align with their partisan identification. This effect is especially pronounced when using voters' evaluations of candidate competence. This suggests that although partisans are very resistant to negative interpretations of the competence of their in-party candidate, once they are convinced of their low level of competence, this information has a strong effect on their voting behaviour.

In addition to these individual level effects, this chapter has also tested whether the effects of cross-pressure vary across districts. In line with expectations (H6 and H7), I find that the effects of cross-pressure on vote choice are *insignificant* in races with high partisan stakes. Specifically, if districts are either strongly polarised or highly competitive, the effect of cross-pressure on party choice disappears. Under these circumstances, competence differences between candidates do *not* influence party choice. It is interesting to note that with regards to the competitiveness of an election, cross-pressure also has no effect in races that are considered not competitive at all. Thus,

voters need to have a perception of at least some real competition before they will consider voting against their in-party candidate.

These findings may be interpreted in a variety of ways. First, the fact that cross-pressure does not affect vote choice in districts with high partisan stakes may suggest that voters are aware of the electoral environment they vote in. This is also confirmed by the fact that cross-pressure has no effect in districts that are non-competitive. Voters realise that the race is basically uncontested and thus that there is little reason to vote against the in-party. This is positive news because it contradicts the idea that voters are entirely uninformed or unaware of the ins and outs of politics. It may furthermore suggest that cross-pressured partisans respond strategically in these contexts when they prioritise their partisan loyalty over competence concerns when the partisan stakes are high and voters are concerned about ideological representation. A second, less optimistic, interpretation is that competitive and highly polarised elections are likely characterised by highly visible political campaigns. As such, the reduced effect of cross-pressure in these districts may not reflect a strategic response of voters, but rather a context in which partisanship is primed more strongly and hence matters more to voters.

Regardless of the interpretation of individual level agency of voters, it is clear that the democratic accountability mechanism is hampered in contexts where partisan stakes are high. This is problematic when taking into account that the level of ideological polarisation in American politics has increased over the past decades.³⁰ Furthermore, the findings presented here confirm research that suggests that candidates can utilise partisan loyalty in a strategic manner.³¹ If a candidate is *less* competent compared to the opposition, the candidate can downplay the importance of the quality difference

³⁰Abramowitz and Saunders (2008)

³¹See Adams et al. (2011); Buttice and Stone (2012); Stone and Simas (2010); Simas (2013)

by playing up partisan concerns and raising the partisan stakes of the election.

Although recent studies on Congressional elections find that voters care about candidate ideology and competence,³² it must be acknowledged that Congressional elections are less salient to most voters than presidential elections. In the next chapter I thus shift my attention to a case of government performance that was highly salient to voters. In the next chapter I explore the response of the American electorate to the Watergate scandal.

Table 4.6: Evidence for Hypotheses Tested in Chapter 4

Hypothesis	Evidence
4. Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans	✓
5. Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans	✓
6. High levels of ideological polarisation <i>reduce</i> the likelihood that a cross-pressured partisan votes for the out-party	✓
7. High levels of electoral competitiveness <i>reduce</i> the likelihood that a cross-pressured partisan votes for the out-party	✓

³²See Buttice and Stone (2012); Stone et al. (2010); Simas (2013)

Chapter 5

Cross-Pressure in Response to the Watergate Scandal

It was more a ritual cleansing than an election: a way for voters who were taken in by a con-artist to express their shame and their resentment. They washed their hands of Richard Nixon by voting against Charles Sandman, Joseph Maraziti, David Dennis, Wiley Maine, Earl Landgrebe and Watergate. Nixon's last election: that is what it was.

- Anthony Lewis *New York Times*, 7 November 1974 -

5.1 Introduction

In the previous two chapters I have focused on economic cross-pressure and cross-pressure related to candidate competence. In both cases it might be quite difficult for voters to accurately attribute responsibility and hence exercise accountability toward the incumbent. I therefore now focus on a case study that sent a very clear and negative signal to voters about the performance of the incumbent government. In this chapter I focus on the response of American voters to the Watergate scandal. The Watergate scandal is one of the most high-profile events in American politics of the 20th century. The scandal revolved around the corrupt and illegal behaviour of various

high-level Republican party officials, including President Nixon. In this chapter I use Watergate as a case study to explore whether Republican partisans held their party to account for its involvement in the illegal activities that constituted the Watergate scandal. The Watergate scandal conveyed negative information about the Republican party to the electorate. Throughout the chapter I therefore assume that Republican partisans were cross-pressured in light of the events of Watergate whereas Independents and Democratic partisans are not assumed to have experienced cross-pressure as a result of the scandal. Rather, for Democratic partisans and Independents the scandal likely confirmed their negative opinion of the Republican party and President Nixon. In line with the analyses presented in Chapter 3, I again focus on both the *identification* of cross-pressured partisans as well as the *behavioural* response of cross-pressured partisans at the time of the 1974 midterm elections, the first elections after the scandal took place. Table 5.1 presents an overview of the elements of the framework of cross-pressured voting that will be addressed in this chapter.

Table 5.1: Hypotheses Tested in Chapter 5

Identification of Cross-Pressured Partisans
H1: As strength of partisan identification <i>increases</i> , the likelihood of cross-pressure <i>decreases</i>
H2: Partisans with <i>low</i> or <i>high</i> levels of knowledge are less likely to be cross-pressured compared to those with <i>moderate</i> levels of knowledge
Behavioural Response of Cross-Pressured Partisans
H4: Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans
H5: Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans

In this chapter I use two types of data to explore the response of the American electorate to Watergate. First, I focus my attention on the House Judiciary Committee on Impeachment. This committee was asked by the House of Representatives to conduct an inquiry into the possible impeachment of President Nixon for his involvement in the Watergate scandal. The committee consisted of 20 Democratic House representatives and 17 Republican House representatives. Of the 17 Republican representatives, 10 were loyal to the president and voted against impeachment, 7 voted in favour of his impeachment. A few of the House representatives who were loyal to Nixon were mentioned in the opening citation of this chapter. Many newspapers at the time of the 1974 election suggested that the members of the House Judiciary Committee who voted against the impeachment of President Nixon were punished for doing so in the 1974 midterm elections. As a first test of the electoral response to Watergate I therefore assess whether this was indeed the case. I use district level vote outcomes to explore whether Republican House representatives who were loyal to Nixon performed worse in the 1974 midterm elections compared to the Republican House representatives who were skeptical of the president and voted in favour of his impeachment. If this is the case then voters used the positions taken by their House representative vis-à-vis impeachment as a signal for either punishment or reward.

The main benefit of the district-level analysis presented in this chapter is that it utilises a difference in difference design to assess whether or not voters punished the Republican party in the 1974 midterm elections. A downside of the method however is that it does not offer any insights as to the decision-making process of individual voters. Furthermore, the district level analysis assumes a very targeted response of the electorate in response to Watergate. I therefore complement this district level analysis with a study of individual level survey data from the *American National Election Study* (ANES) that covers the period 1972-1974. In the individual level analysis I

explore whether the electorate turned away from the Republican party between 1972 and 1974 and whether Republican identifiers were less likely to stay loyal to their party in the period 1972-1974 compared to their Democratic counterparts. I also explore whether there are differences within the group of Republican partisan identifiers.

The chapter proceeds as follows. First, I briefly introduce the main facts of the Watergate scandal in section 5.2. In section 5.3, I review how the American electorate has responded to political scandals in general and to Watergate in specific. I conclude that there is very little empirical evidence for a so-called ‘Watergate effect’ on the 1974 midterm elections. I then introduce the first empirical analysis of the House Judiciary Committee in section 5.4 and discuss the difference in difference design. This is followed by the presentation of the difference in difference results in section 5.5. Section 5.6 presents the individual level analysis using the ANES panel data. The final section, 5.7, offers a reflection on the main findings of this chapter and discusses how the findings inform the central research question of the thesis.

5.2 The Watergate Scandal

In June 1972, five men were arrested for an intended burglary at the Democratic party headquarters at the Watergate complex in Washington. The burglars attempted to install wire-taps and copy documents to monitor the communications of the Democratic party. Almost immediately after the break-in, the FBI established that the burglars were in contact with government officials inside the White House. Nevertheless, the White House denied any involvement in the matter for up to almost a year after the burglary. Despite the increased media attention to Watergate, particularly by the investigative journalism from the *Washington Post*, President Nixon managed to secure an unprecedented landslide victory in the 1972 presidential elections. Yet,

ongoing court proceedings and persistent journalistic pursuit unravelled a complex plot involving high-ranking White House aides and Nixon confidantes in 1972-1973. Consequently, in early 1973, the US Senate decided to install a committee to further investigate the Watergate affair. The Senate hearings which started later that year received large-scale media coverage and were broadcast live on television, making Watergate an integral part of American political life from 1973 onwards. In February of 1974, the House of Representatives asked the House Judiciary Committee to start impeachment proceedings against the president.

In the following months, President Nixon fired the special prosecutor in light of his subpoena of the president requesting access to the White House tape-recordings. When Nixon fired the special prosecutor, both the attorney general and the deputy attorney general resigned out of protest. The Supreme Court ruled in July of the same year, in *the United States vs. Nixon*, that the president was obliged to hand over the White House tapes to the Senate Committee. In the final days of July, the bipartisan House Committee adopted three articles of impeachment against the president. Out of the 17 Republican representatives on the committee, seven voted in favour of impeachment of their president, ten voted against the impeachment of their president. The vote on impeachment never reached the full House however, because on the night of August 8th, President Nixon resigned. Nixon is succeeded by Vice-President Gerald Ford whom Nixon himself appointed only a few months earlier. In September of 1974, Ford granted Nixon a full presidential pardon regarding any Watergate-related actions. An act that is deemed highly controversial by many Americans at the time.¹

¹The summary of events presented above is based on accounts of the events as described in Genovese and Morgan (2012); Kutler (1990, 2010); Olson (2003)

Watergate has been referred to as: ‘a political scandal and constitutional crisis’² and ‘America’s most traumatic political experience of this century.’³ Many biographies of Nixon and detailed histories of the events of Watergate suggest that the scandal was reflective of a larger culture of secrecy, paranoia and mistrust that characterised both Richard Nixon and his White House.⁴ The insecurity and high levels of mistrust that defined Nixon were fueled further by his marginal victory in 1968 and his experience of continued resistance within his own Republican party.⁵ Furthermore, the ‘mindset of the Cold War’ is suggested to have facilitated the extreme attitudes of the Republican White House toward potential opponents. The Democratic party was not merely seen as the opposition, but as a potential traitor or enemy to the American nation.⁶ It was against this backdrop that activities such as phone-tapping, espionage and installing a recording system in the White House became normalized practices.

5.3 The Electoral Response to Political Scandals in the US

Political scandals provide voters with valuable information about the character and competence of an incumbent president or government. Based on this information, voters might decide to punish the incumbent for betraying their trust or for damaging the wellbeing of the country as a whole.⁷ As such, political scandals are helpful case studies to assess whether voters hold politicians to account for their performance

²Genovese and Morgan (2012, p.xi)

³Genovese and Morgan (2012, p.1) the authors quote "Times" magazine. Where this century refers to the 20th century

⁴See for instance Genovese and Morgan (2012); Kutler (1990, 2010); Olson (2003)

⁵Olson (2003)

⁶Genovese and Morgan (2012, p.11)

⁷See Stoker (1993) for a discussion of the various narratives that a political scandal might communicate.

in office, especially when the performance is below par.⁸ A review of studies into the electoral consequences of political scandals in American elections suggests that there is mixed evidence as to voters' responsiveness to political scandals and corruption cases. Although in general voters respond negatively to corruption allegations or political scandals, these negative attitudes do not necessarily translate into electoral repercussions.⁹

Studies on the second most prominent scandal in American political history, the Clinton-Lewinsky scandal, find that incumbent President Clinton barely lost any popular support in light of the scandal.¹⁰ One study suggests that Clinton's strong performance in the economic domain insulated him from blame attribution.¹¹ Others have argued that, in this particular case, Clinton's indiscretions were seen to be a personal matter, not of any relevance to his performance as president.¹² In general, Clinton's popularity remained high among his Democratic supporters and low among Republican voters. The scandal did little to change these polarised partisan attitudes.¹³

Examples of electoral punishment in the wake of scandal do exist however. In the 1990's the US House of Representatives experienced the 'banking scandal' which revealed that many House representatives (of both parties) had overclaimed expenses.

⁸See for instance Mondak and Mitchell (2009); Peters and Welch (1980); Stoker (1993); Jacobsen and Dimock (1994); Welch and Hibbing (1997); Zaller (1998); Fischle (2000); Owen (2000); Lawrence and Bennett (2001)

⁹For this review I looked at studies on the Clinton-Lewinsky scandal Fischle (2000); Zaller (1998); Owen (2000), a study on the scandal involving Democratic primary candidate Gary Heart by Stoker (1993) and various studies on corruption scandals involving members of the House of Representatives by Peters and Welch (1980); Jacobsen and Dimock (1994); Alford et al. (1994); Groseclose and Krehbiel (1994); Welch and Hibbing (1997); Mondak and Mitchell (2009)

¹⁰See for studies on the scandal Fischle (2000); Zaller (1998); Owen (2000); Lawrence and Bennett (2001)

¹¹See Zaller (1998) who also adds the fact that under Clinton the US was not involved in any major overseas war efforts which strongly increased his favourability numbers.

¹²Fischle (2000)

¹³Fischle (2000)

Several studies find that voters punished representatives who committed the largest financial offenses.¹⁴ Similarly, two studies on the electoral response to other acts of corruption in the US House also find that corrupt incumbents lose between 6-11% of their vote share.¹⁵ Finally, a study on the 2006 midterm elections suggests that voters punished candidates who were affiliated with political lobbyist Jack Abramoff, who was found guilty of several corruption charges.¹⁶ There are thus positive examples in which the American electorate punished politicians who were involved in corruption or political scandal.

Surprisingly, there are only very few studies that directly explore the electoral consequences of Watergate. Many studies that focus on the topic explore the effects of Watergate on public opinion, as opposed to the electoral repercussions of the scandal. This may be explained by the fact that Watergate emerged at a time that public opinion polling was gaining ground.¹⁷ Almost all of the existing studies on Watergate find that the public as a whole reacted negatively to Watergate. Nixon's approval ratings dropped and trust in the Republican party declined.¹⁸

Newspapers at the time of Watergate were quite convinced that voters would be voting with Watergate on their minds in the 1974 midterm elections. On the eve of the election, *The New York Times* wrote for instance: 'the Republican Party is braced for another harsh dose of Watergate medicine in the elections.'¹⁹ And *The Washington Post* spoke of the: 'purification of the political system.'²⁰ Many opinion polls too suggested that voters intended to punish the Republican party

¹⁴Jacobsen and Dimock (1994); Alford et al. (1994)

¹⁵Peters and Welch (1980); Welch and Hibbing (1997)

¹⁶Mondak and Mitchell (2009)

¹⁷Lang and Lang (1980)

¹⁸Robinson (1974); Dunham and Mauss (1976); Sniderman et al. (1975) For a more recent example of the response of the public to Watergate see Dinas (2013); Achen and Bartels (2016)

¹⁹Lydon (1974)

²⁰Kraft (1974)

and Republican candidates experienced the negative side-effects of Watergate on the campaign trail. Many of them tried to actively distance themselves from the party and its leadership.²¹ True to prediction, the 1974 elections proved disastrous for the Republican party. The party lost 5 seats in the Senate, 49 seats in the House of Representatives and 5 governorships. The newly elected Democratic House members became known as the *Watergate Babies*.²² The post-election newspaper coverage again emphasized the importance of Watergate to explain the election outcome. As *the New York Times* wrote:

while voters across the country are deeply concerned about the state of the economy, the Watergate scandal appeared to be a greater factor than the economy in the switch of votes from Republican to Democrat candidates.²³

The Washington Post added to this:

while rising prices and a sinking economy aided the Democrats yesterday, their victories seemed chiefly the legacy of the Watergate scandal that forced President Nixon from office.²⁴

There is only one academic study which clearly supports the ‘conventional wisdom’ interpretation of the effect of Watergate in 1974, this is a study by Uslander and Conway (1985). Unfortunately, the evidence the authors present is rather thin. First, the most important independent variable used in the study, retrospective voting, is a *combination of* retrospective economic evaluations *and* Nixon approval. As such, it is impossible to tease out whether there was an effect of Watergate irrespective of voters’ response to the economic recession. Rather, the fact that the authors use a

²¹Apple (1974); Broder (1974b); Wooten (1974)

²²Simultaneously the term has also been used to refer to new entries into journalism in the post-Watergate era, inspired by the investigative journalism conducted by the two Washington Post journalists who were responsible for the unraveling of the scandal.

²³Rosenbaum (1974b)

²⁴Cannon (1974); Chapmen (1974)

combined retrospective variable seems to suggest that perhaps there was no independent effect of either.²⁵

A second study that also explores the electoral effects of Watergate is much more skeptical of the claim that Watergate caused the Republican defeat in 1974.²⁶ Instead, Jacobsen (1985) argues that the defeat of the Republican party was caused by a lack of competent Republican candidates and a strong decline in campaign funding on the part of Republican donors. This has been referred to as the ‘strategic politicians thesis.’²⁷ The Republican party did not lose the 1974 midterm elections due to the behaviour of *voters*, but rather due to the behaviour of *politicians and other strategic actors*. Yet, as the authors concede, this campaign dynamic was strongly driven by the events of Watergate and could thus be interpreted as an *indirect* effect of Watergate.²⁸ Both campaign donors as well as potential candidates expected the electorate to punish the Republican party and hence withdrew their support and candidacies.

A third study on the topic by Conway and Wyckoff (1980) only finds weak effects of Watergate on the 1974 vote. Using a path analysis, the authors find that partisan identification, candidate incumbency and name recognition exercise the strongest pull on vote choice. Presidential performance ratings, economic evaluations and scores on a Nixon thermometer only have: ‘negligible effects’, the study finds.²⁹ With regard to this latter study, it must be pointed out that the findings are highly endogenous. Partisan preferences, the strongest predictor in the study, are measured in 1974 and

²⁵This criticism is suggested by Jacobsen et al. (1986)

²⁶Jacobsen (1985) and Jacobsen et al. (1986)

²⁷See also Rosenbaum (1974a); Sullivan (1974); Jensen (1974) all of whom report on the lack of Republican campaign funding.

²⁸Jacobsen et al. (1986)

²⁹Conway and Wyckoff (1980, p.6)

are thus most likely affected by a voter's opinion on Watergate.³⁰ A similar problem exists with the measure of presidential approval, which measures approval of President Ford. Since Ford had only been in office for a few months at the time of the survey, and his first act was to pardon Nixon, this variable at least partially captures a Watergate sentiment of voters.³¹

In sum, there is mixed evidence as to voters' responsiveness to political scandals involving corruption allegations in the US context. It appears that voters often respond along partisan lines, but positive examples of electoral punishment do exist.³² With respect to Watergate, it appears that the American public as a whole reacted negatively to information about the scandal. According to the leading newspapers at the time, Watergate was therefore the main cause of the Republican party's losses in the 1974 midterm elections. Yet, this explanation only finds minimal support in the existing academic literature. In the next sections of this chapter I present two sets of analyses that assess the extent to which (if any) Republican partisans punished the Republican party in the 1974 elections and which Republican partisans were most likely to experience cross-pressure in light of the events of Watergate.

5.4 District Level Analysis: Introducing the House Judiciary Committee

In January 1974, the US House of Representatives asked the House Judiciary Committee to explore a case of impeachment against President Nixon. Three articles for impeachment were on the table: the obstruction of justice, the abuse of power and

³⁰This was shown for instance by Dunlap and Wisniewski (1978) who find that Republican voters *updated* their partisan identification leading up to the 1974 Congressional elections.

³¹This is also noted by the authors themselves on p.9-10.

³²Most notably with regard to the House of Representatives in the wake of the banking scandal and the Abramoff scandal.

the defiance of subpoenas. The Judiciary Committee consisted of 21 Democrats and 17 Republican members of the House of Representatives. Among the Republican members of the committee, seven voted in favour of impeachment of President Nixon on at least one article. In the analyses presented below, these representatives will be referred to as ‘Nixon Skeptics.’ Ten Republican members voted *against* the impeachment of President Nixon on all counts. These members will be referred to as the ‘Nixon Loyalists.’ Only two Democrats voted against impeachment on one count; all others voted in favour on all three articles. In the next sections I focus only on the Republican members of the House Judiciary Committee and use the variation in the positions taken by these members to assess the size of the ‘Watergate Effect’ on the 1974 midterm elections. Specifically, I explore whether Nixon loyalists suffered greater losses in vote share or saw a greater decline in their victory margins compared to Nixon skeptics. I also explore whether there were differences in the decline in turnout and the number of third party votes across the two sets of districts. If this is true, then we may attribute these differences between these two sets of districts (loyalists versus skeptics) to the Watergate scandal.

Table 5.2 presents an overview of the Republican members of the committee and indicates whether or not they managed to retain their seat in the 1974 midterm elections. Only a small number (two) of the members resigned. Table 5.2 shows that out of the 17, a total of ten members were re-elected, five lost their seat. Importantly, four out of the five members who lost their seat had supported President Nixon against the charges of impeachment, suggesting that perhaps the electorate targeted their response in line with the position on impeachment taken by their House representative.

Table 5.2: Republican Members of the House Judiciary Committee 1972-1974

State and District	House Member	Nixon Loyalist/Skeptic	1974 Election
CALIFORNIA 22	Moorhead	Loyalist	WON
CALIFORNIA 39	Wiggins	Loyalist	WON
ILLINOIS 13	McClory	Skeptic	WON
ILLINOIS 19	Railsback	Skeptic	WON
INDIANA 10	Dennis	Loyalist	LOST
IOWA 6	Maine	Loyalist	LOST
MAINE 2	Cohen	Skeptic	WON
MARYLAND 5	Hogan	Skeptic	RETIRED
MICHIGAN 4	Hutchinson	Loyalist	WON
MISSISSIPPI 5	Lott	Loyalist	WON
NEW JERSEY 2	Sandman	Loyalist	LOST
NEW JERSEY 13	Maraziti	Loyalist	LOST
NEW YORK 25	Fish Jr.	Skeptic	WON
NEW YORK 36	Smith III	Loyalist	RETIRED
OHIO 5	Latta	Loyalist	WON
VIRGINIA 6	Butler	Skeptic	WON
WISCONSIN 8	Froehlich	Skeptic	LOST

It may seem a tall order to suggest that the American electorate responded to the events of Watergate by directly attributing blame and reward to their House representative for their voting behaviour on the Judiciary Committee. Yet, it is important to realize that the events surrounding Watergate were heavily publicized on radio, television and in the newspapers. The idea that voters directly punished those members of the House of Representatives who stood by President Nixon is discussed in several academic studies and was written about by both the *New York Times* and the *Washington Post* at the time of the election.³³

³³Academic studies see Uslaner and Conway (1985); Conway and Wyckoff (1980); Wright (1977) although only Wright (1977) tests this empirically.

The Washington Post for instance reported on the day after the elections that: ‘among the Republican losers were four of the ten members of the House Judiciary Committee who opposed the impeachment of the president’³⁴ whilst: ‘all committee Democrats who voted for impeachment and sought re-election won.’³⁵ As stated at the start of this chapter, one reporter from the *New York Times* concluded:

It was more a ritual cleansing than an election: a way for voters who were taken in by a con-artist to express their shame and their resentment. They washed their hands of Richard Nixon by voting against Charles Sandman, Joseph Maraziti, David Dennis, Wiley Maine, Earl Landgrebe and Watergate. Nixon’s last election: that is what it was³⁶

A range of other studies have furthermore shown that voters hold local representatives accountable for votes they cast in Congress.³⁷ In the 2006 midterm elections, Republican representatives who voted in favour of the Iraq war fared worse than those who voted against it and in 2010 voters who did not agree with health care reforms punished their Democratic representatives who supported these measures.³⁸ Similarly, studies on the House banking scandal of 1992 also show that voters punished House representatives in line with the severity of their overdrafts.³⁹ It thus does not seem unfeasible to suggest that voters would hold their House representative *directly* accountable for the position they took vis-à-vis the impeachment of President Nixon.

This suggestion is supported by a study by Wright (1977), which similarly aims to assess the electoral repercussions of the Watergate Scandal.⁴⁰ In the article, the focus is also on the Republican members of the House Judiciary Committee, but, contrary

³⁴Broder (1974a)

³⁵Lyons (1974)

³⁶Lewis (1974)

³⁷Ansolabehere and Jones (2010); Canes-Wrone et al. (2002)

³⁸Grose and Oppenheimer (2007); Sides et al. (2012)

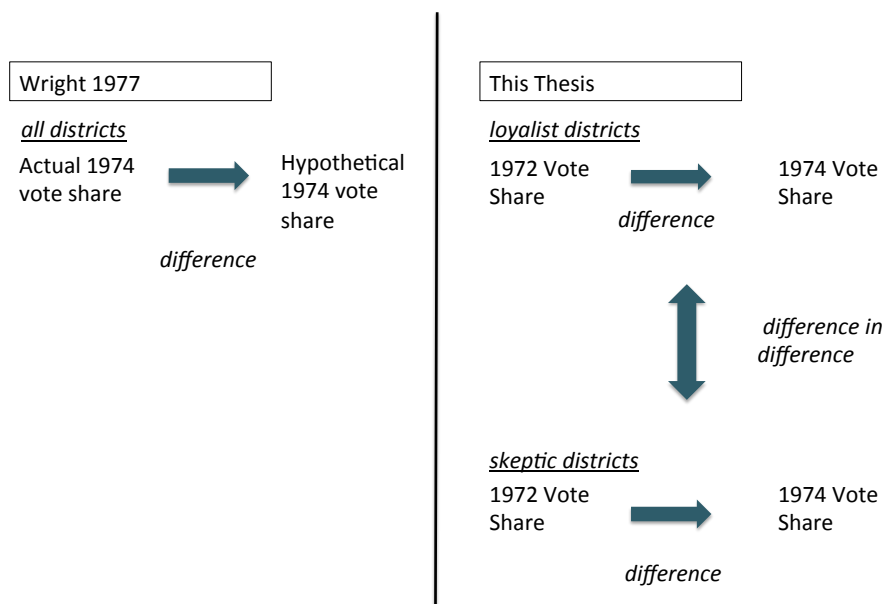
³⁹Alford et al. (1994); Jacobsen and Dimock (1994)

⁴⁰Wright (1977)

to the approach taken here, the vote share received by the Republican members of the Judiciary Committee is compared to the vote share these members *would have received had they not been on the committee*. The hypothetical vote share that the Republican members of the committee would have received in the absence of Watergate is calculated on the basis of previous vote share, turnout in 1972 and incumbency status. The study finds that Republican committee members who supported the impeachment of President Nixon fared approximately 5% better than predicted. Nixon loyalists on the other hand fared *worse* than predicted and lost an average of 7% more than anticipated.

Wright's study thus suggests that Watergate had an important effect on the individual success of House representatives. A potential downside of the study conducted by Wright however is the fact that it compares vote shares in 1974 with *hypothetical* vote shares in 1974, which are based on 1972 turnout and vote share. It does not directly compare the changes in vote share between the two sets of districts however. The approach I take on the other hand compares *actual* vote shares in 1972 and 1974 between the two sets of districts. The difference between Wright (1977) and this study are displayed in Figure 5.1. The main benefit of the difference in difference approach used here is that it relies on actual vote shares as opposed to hypothetical, predicted, vote shares. In using the difference in difference design, the loyalist and skeptic districts are assumed to differ only in terms of the position taken by the House representative with regard to the impeachment of President Nixon. Other trends, such as the economic recession, are assumed to be consistent across districts. In the next section I investigate this assumption of similarity of districts.

Figure 5.1: Methodological Approach Wright (1977) versus this Thesis



5.5 Results Part I - the House Judiciary Committee

In the district-level analysis I focus on the districts that were represented by the Republican members of the House Judiciary Committee on Impeachment. I compare districts that were represented by a Republican House member who voted *in favour* of impeachment with districts that were represented by a member who voted *against* impeachment. The sample of the analysis consists of the 17 district represented by Republican members of the House Judiciary Committee. The districts are divided such that:

- Treatment group : Nixon loyalists; districts represented by a House member who voted *against* the impeachment of President Nixon. $N = 10$
- Control group : Nixon skeptics; districts represented by a House member who voted *in favour* of the impeachment of President Nixon. $N = 7$

The appointment of members of the House Judiciary Committee may be seen as unbiased. Members of the House were appointed to the Judiciary Committee before the occurrence of Watergate and it seems unfeasible that any of the members of the Judiciary Committee could have foreseen that they would become part of the largest impeachment inquiry in American history. As such, we may assume that House Representatives did not self-select into the Judiciary Committee with the foresight of gaining an electoral advantage (or disadvantage) from their membership.⁴¹ Yet, we cannot assume that the positions taken by the members of the committee as to the impeachment of President Nixon are randomly distributed. Rather, I assume that the positions taken by the members of the House Committee reflect strategic considerations: committee members voted in line with what they assumed the majority of

⁴¹One factor that distinguishes the members of the judiciary committee from other house members is the fact that the majority of the members of the judiciary committee have a legal background.

their constituents approved off.⁴²

As such - an individual voter's exposure to either treatment (Nixon loyalty) or control (Nixon skepticism) is not randomly distributed and the research design should be classified as a 'quasi-natural' experiment. A positive feature of the quasi-experimental design is the fact that it controls for national-level developments that might have affected a decline in Republican vote share across *all* districts. Prime examples of these are the economic recession, decline in turn-out and a more general decline in partisan voting or anti-Republican sentiment in the 1970's. As such, the design tackles issues that remain unresolved in other studies that aim to examine the Watergate effect.⁴³ Note however that this does not preclude the possibility that national level trends might have had a diverse effect across districts.⁴⁴ An important downside of the design presented here is the small sample size of 17 districts. The main complication resulting from this is the lack of statistical power in determining whether differences between the two sets of districts are significant. In the analyses presented below I therefore use information on the vote outcomes in *all* electoral districts ($N = 435$) between 1972 and 1974 to empirically generate p-values for the estimates presented. This process is further explained in section 5.5.7.

In the analyses presented below I explore four different outcome variables: (1) the vote shares received by the Republican members of the Judiciary Committee on Im-

⁴²See Lang and Lang (1980) who suggests that members of the House committee even had their own polling done to assess the preferences in their district with regards to the impeachment of the president.

⁴³Such as Conway and Wyckoff (1980); Uslander and Conway (1985); Wright (1977)

⁴⁴I attempted to investigate this possibility by trying to find district level data on economic developments across districts. Unfortunately I was unable to find such data for the period 1972-1974. The best information I could find was census data for the period 1970 and then 1977 which lists economic indicators. The economic indicators were average household income and the number of families living in poverty. These data are seven years apart however and are furthermore not identified at the district level but rather at the city level. Not all districts were represented in the census data which made the comparison largely uninformative.

peachment; (2) their victory margins; (3) the total number of votes cast per district; and (4) the number of votes cast for third parties. If voters responded to Watergate in a targeted manner, then I expect that the vote share and victory margins of the loyalist representatives show a *stronger* decline compared to those of the skeptic members of the Judiciary Committee. It is also possible however that Republican voters did not express their discontent by voting for the opposition party, but rather by not voting at all. I therefore also explore whether there is a difference in changes in turnout across the two sets of districts. A final option I explore is whether Republican voters expressed their discontent with the Republican party by voting in increasing numbers for a third party option in loyalist districts because it is feasible that Republican identifiers would feel uncomfortable voting for a Democratic candidate and would therefore express their discontent with their own party by voting for a third party.

5.5.1 The Difference in Difference Model

In order to assess whether or not Republican representatives of the House Judiciary Committee who supported Nixon against impeachment received a lower vote share compared to the Republican representatives on the Committee who voted in favour of his impeachment, I use a difference in difference research design.⁴⁵ Put simply, I compare the districts that were represented by Nixon loyalists with those that were represented by Nixon skeptics and assess whether the former saw a significantly larger decrease in their vote share between 1972 and 1974 compared to the former.

The unit of analysis are thus House electoral districts. The main time-period of interest are the midterm elections immediately prior and post- Watergate: 1972 and 1974.

⁴⁵I rely on the notation of the difference in difference model as set out in Angrist and Pischke (2009) and as applied in Bechtel and Hainmueller (2011)

For the analyses below I use the following notation, i districts = $\{1, \dots, N\}$ electoral districts with $N = 17$, and $t = \{1972, 1974\}$. I create a binary treatment variable $D_{it} = \text{Nixon Support}$. This takes the value of 1 for districts with representatives who were loyal to Nixon, and the value 0 for districts represented by Nixon skeptics, at time-point 1974.⁴⁶ The outcome variable Y_{dit} reflects potential outcomes, where Y_{1it} and Y_{0it} reflect the vote share received by the Republican House members in the control and treatment conditions. The vote share reflects the number of votes for the Republican House candidate as a percentage of the total number of votes cast in the House race in the particular district. The total vote includes Republican, Democrat and potential Third Party votes.

The causal effect of Nixon support on 1974 electoral outcomes (or: the average treatment effect on the treated), can be defined as the difference in vote share in Nixon-loyalist districts with and without the treatment - conditional on the treatment taking place [$D_i = 1$]:

$$\alpha = E[Y_{1i,74} - Y_{0i,74} | D_i = 1]$$

The problem with this specification is that the condition: $E[Y_{0i,74} | D_i = 1]$ (vote share in loyal districts in absence of treatment, conditional on treatment) is unobserved. Therefore, in line with common practice in difference in difference designs, I identify the missing observation by using the parallel trends assumption. This assumption suggests that, in the absence of Watergate, Republican vote shares in loyalist districts would have followed a trend *parallel* to the trend in Nixon skeptic districts:

$$E[Y_{i0,74} - Y_{i0,72} | D_i = 1] = E[Y_{i0,74} - Y_{i0,72} | D_i = 0]$$

If the parallel trends assumption holds, then any deviation in trends in the period of interest (1972-1974) may be attributed to the treatment (Nixon loyalty). The

⁴⁶Note this is thus an interaction between time-period (1972 or 1974) and Nixon-loyalty or Nixon-skepticism.

average treatment effect on the treated can then be re-defined as a difference in difference estimation as follows, where I calculate the difference in the change in vote share between treated and non treated districts:

$$\alpha = \{E[Y_{i,74}|D_i = 1] - E[Y_{i,72}|D_i = 1]\} - \{E[Y_{i,74}|D_i = 0] - E[Y_{i,72}|D_i = 0]\}$$

I then estimate a fixed effects regression model as follows:⁴⁷

$$Y_{it} = \gamma_i + \delta t + \alpha D_{it}$$

Here, γ is a district level fixed effect which controls for time-invariant unobserved factors at the district level, δ is a time-fixed effect which either takes the value 1972 or 1974 and controls for potential time-trends, α is the treatment effect which is an interaction between district (loyal versus skeptic) and the time-dummy. Because I only use two time-periods (1972 and 1974) this model is equivalent to a first-difference model.

5.5.2 Data

The data I use are taken from the Office of the Clerk of the House of Representatives.⁴⁸ The Office of the Clerk of the House publishes all official vote counts from 1920-2010 at the district level in pdf-format. These have been transcribed for the analyses conducted in this chapter. The first variable of interest I explore is the vote share received by the Republican House candidate in 1972 and 1974. The Republican vote share is calculated as a proportion of the total reported votes for the House race in the district. This includes the vote totals for the Republican, Democrat and

⁴⁷The regression model is in line with the difference in difference model as explained by Angrist and Pischke (2009) and also used by Bechtel and Hainmueller (2011)

⁴⁸[http : //clerk.house.gov/member_info/electionInfo/](http://clerk.house.gov/member_info/electionInfo/) Accessed October through December 2015 - and October 2016

reported other parties.⁴⁹

Boundary changes occurred in the electoral districts for the House between 1972 and 1974. The majority of the candidates that I focus on ran in the same district in both years. Yet, the boundary changes led to a change in electoral district for the representatives from California.⁵⁰ In the analyses provided below, I follow the candidate, rather than the district. Thus, for the representatives from California, I use their vote outcomes for districts 20 and 25 in 1972, but districts 25 and 39 in 1974. Boundary changes also occurred prior to 1972, this is relevant for the estimation of the placebo regression. The House representatives from New York ran in different districts in 1970 compared to 1972. Again, I follow the candidates as opposed to the districts.⁵¹ The Appendix, Table A.10 offers an overview of the changes in district boundaries. Removing the districts that underwent boundary changes from the analyses does not alter the substantive results presented below.

5.5.3 Establishing the Parallel Trends Assumption

The first task is to establish the parallel trends assumption as set out above in order to assess the validity of the difference in difference estimation. In Figure 5.2 I plot the average Republican vote share in the loyalist and skeptic districts in the period 1964-1976. The red vertical line reflects the year 1973 and symbolizes the occurrence of Watergate.

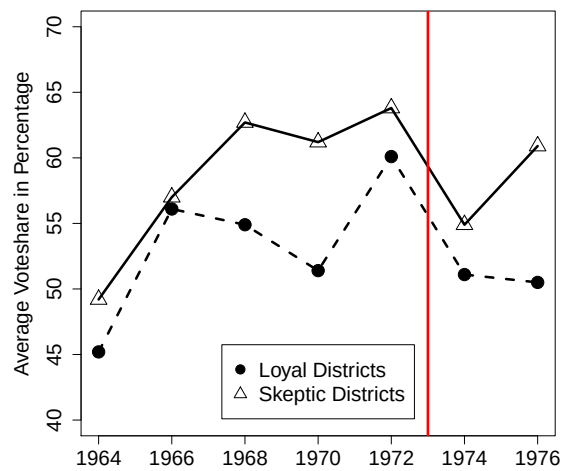
⁴⁹Liberals and Conservatives are counted as "other" (i.e. third party) unless the Office of the Clerk explicitly adds these votes to the votes received by the Democrat and Republican candidates and lists these as one unit. Write-ins and scatter votes are also classified as "other".

⁵⁰In 1972 House Representative Moorhead ran in district 20, in 1974 he ran in district 22. In 1972 House Representative Wiggins ran in district 25 and in 1974 he ran in district 39.

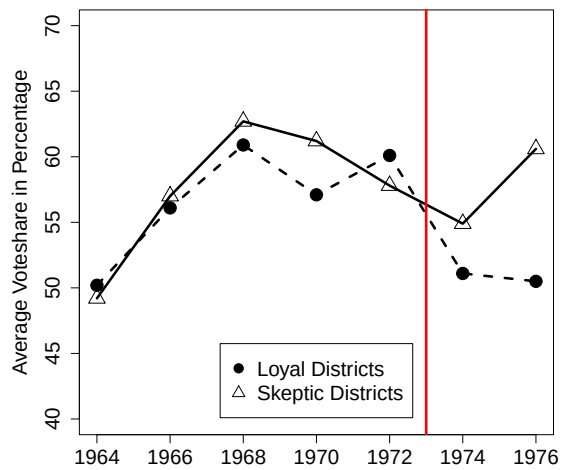
⁵¹Vote totals prior to 1970 are thus taken from the district in which House representatives Fish Jr. and Smith III ran at that point in time, as opposed to the district which they came to represent in 1972.

Figure 5.2: District Level Vote Share Republican Representatives 1964-1976 House Elections

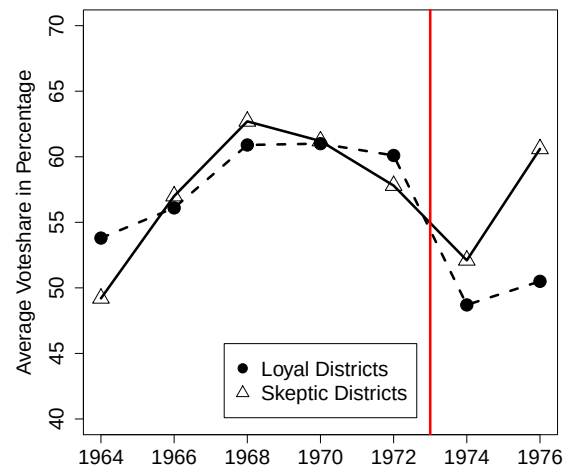
(a) All Districts Included (N=17)



(b) Excluding Unopposed Races (N = 16)



(c) Excluding Unopposed Races and Statistical Outliers (N = 14)



In Figure 5.2a I include all 17 districts. Here, the trend in Republican vote share across the two sets of districts appears relatively parallel up until the 1970 elections. A divergence of the parallel trend is observed in the period 1970-1972 when the vote share received by Republican candidates increased more strongly in the Nixon loyalist districts, suggesting that perhaps the loyalist districts were more strongly Republican in the period prior to Watergate. In the period 1974-1976 we see that Republican candidates in the Nixon skeptic districts receive a larger increase in the number of votes, suggesting that perhaps indeed they were rewarded by the electorate for their tough stance on impeachment. A problem with the trends in vote share plotted in Figure 5.2a is that there is a (minor) *deviation* of the parallel trends in the period prior to Watergate, specifically in the period 1970-1972 where the increase in Republican vote share appears somewhat steeper in loyalist districts compared to skeptic districts. A further look at the data reveals that Figure 5.2a includes data-points of races in which candidates ran unopposed, thus biasing the results.⁵²

In Figure 5.2b I thus plot the average vote share received by Republican House Representatives in the loyal and skeptic districts but exclude any race in which a candidate ran unopposed. This actually leads to an overlap in Republican vote share in the period 1964-1968, but suggests a stronger divergence in the parallel trends between the two groups in the period 1970-1972 (Figure 5.2b). This is problematic because it suggests the two sets of districts may not be comparable. In Figure 5.2c I further exclude districts that may be considered statistical outliers.⁵³ After having excluded

⁵²Unopposed races are as follows:

Districts Represented by Nixon Loyalists:

1964 = 1 race ; Mississippi, no Republican candidate running

1968 = 1 race; Mississippi, no Republican candidate running

1970 = 1 race; Mississippi, no Republican candidate running

1976 = 1 race; Virginia, no Democratic candidate running

Districts Represented by Nixon Skeptics:

1972 = 1 race; Illinois no Democrat candidate running

⁵³The Appendix, Table A.4 offers a more detailed discussion of the calculation of outliers. An

both unopposed races and statistical outliers, Figure 5.2c suggests that the two sets of districts follow each other reasonably closely in the period 1966-1970, but show divergences especially in the post-Watergate elections (Figure 5.2c). In line with expectations, the decline in vote share between 1972 and 1974 is steeper for loyalist districts in all plots depicted in Figure 5.2.

The evidence in favour of the parallel trends assumption is not overtly persuasive. Most importantly, even when unopposed races and statistical outliers are excluded, there is still a divergence in the trend between the two sets of districts in the period 1970-1974. It is therefore necessary to further probe the parallel trend assumption. As a first step I explore the trend in presidential vote share for the two sets of districts to see whether Republican presidential candidates fared much better in Nixon loyalist districts. There is an important caveat to this analysis: the electoral data on presidential vote share is available only at the *state* level and not at the district level. This is thus a much more indirect test of the parallel trends compared to the one presented above.

observation is considered an outlier when the vote share is either 1.5 times higher or lower the inter-quartile range. This is true for the following observations:

- 1964: New Jersey District 13 - Republican vote share 21.5%
- 1970: New Jersey District 13 - Republican vote share 25.5%
- 1974: Maine District 13 - Republican vote share 71.3%
- 1974: Mississippi District 14 - Republican vote share 73%

Figure 5.3: Republican Party Vote Share in Presidential Elections 1956 - 1976 at State Level

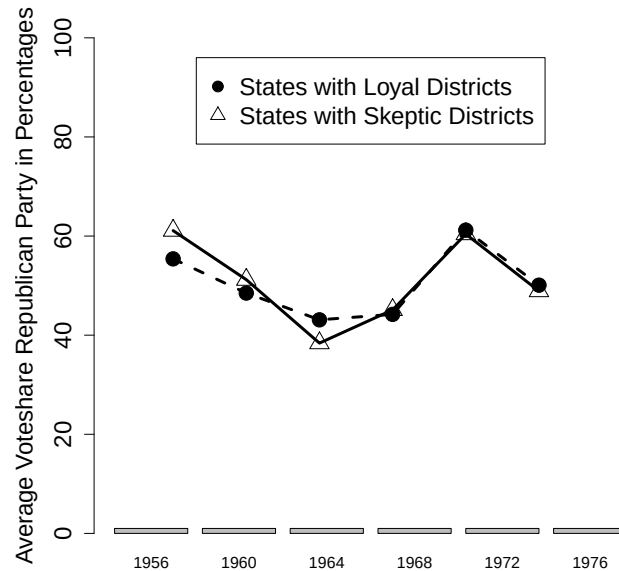


Figure 5.3 depicts the presidential vote share at the state level. States are classified as either ‘loyal’ or ‘skeptical’ based on the choices made by the Representatives of the districts that fall within this state. This approach is unproblematic for all districts apart from those located in New York which contains two districts, one of which is loyal and one of which is skeptical.⁵⁴ I therefore exclude New York from my analysis for this plot. Figure 5.3 suggests that there were no large differences in presidential vote share at the state level in which the 17 districts were situated. There is a divergence in the parallel trend in the period between 1960-1964, but in 1968 and 1972 (the elections in which Nixon participated), the vote share received by President Nixon was very similar across these two sets of states: in 1968 Nixon received 44.2% of the vote share in loyal states versus 45.1% in states in which the skeptical districts were located. In 1972 this was 61.2% versus 60.4%.⁵⁵ It thus does not appear to be the

⁵⁴In the districts located in the states New Jersey, Illinois and California, both representatives voted in the same way.

⁵⁵These differences are not statistically significant at a conventional level.

case that President Nixon was disproportionately popular in the states which contain the loyalist districts compared to the states that contain the skeptic districts.

5.5.4 Further Tests of the Similarity of Districts

To further probe the similarity of the two sets of districts, I use data from the 1972 US Census to compare the districts on a range of demographic features in the period prior to Watergate.⁵⁶ The data are measured in either 1970 or in 1971-1972 and thus reflect the pre-treatment values. Again, a challenge emerges: the data provided in the Census are not identified at the district level, but at the city-level. I thus use the US City Data books to map cities onto districts using *The Historical Atlas of United States Congressional Districts: 1789-1983*.⁵⁷ This allows me to map cities onto electoral districts. A full overview of the mapping procedure is provided in the Appendix, Table A.11.

The estimates provided in Table 5.3 are averaged across the district groups. There are some challenges when interpreting this data. First, not all districts were represented in the city data books. Specifically, no cities were included for the Maine, Maryland (both skeptic) and Michigan (loyalist) districts. This means that the two groups (loyalists and skeptics) are not balanced in terms of the number of observations. The loyalist sample reflects data on nine districts, compared to five skeptic districts. Furthermore, some district estimates reflect many more observations at the city level than others.⁵⁸ Keeping these limitations in mind, Table 5.3 provides positive encouragement as to the comparability of the two sets of districts. I conducted mean

⁵⁶See ICPSR Study ICPSR2896, available at <http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/2896/terms> accessed January and February 2015

⁵⁷See Martis (1982) Please refer to the Appendix, Table A.11 for a complete list of cities for which data were available and a discussion of limitations of this mapping procedure.

⁵⁸Specifically, California district 25 [loyalist] is over-represented, it reflects 14 observations, whereas for most electoral districts only one city could be mapped.

difference tests for all the mean values reported in Table 5.3 and found no statistically significant differences across the two groups.

Table 5.3: Similarity Districts: Demographics Aggregated at District Level

	Loyalists (N = 9)	Skeptics (N = 5)
Percentage Females	52.9 (1.42)	52.5 (1.14)
Median Age	30.8 (5.5)	30.4 (4.2)
Percentage 18+	67.4 (3.4)	66.3 (6.2)
Percentage 65+	12.1 (5.1)	11.1 (3.9)
Median years Schooling	11.7 (0.9)	12.2 (0.4)
Median Family Income	\$9.577 (1.601)	\$11.407 (2.959)
<i>Standard deviations in parentheses</i>		

A further set of balance tests between the two sets of districts are provided in the Appendix, Table A.12 where I compare the attitudes and partisan identification of individuals from the individual level data (ANES 1972-1976 panel study). These results are not presented in the main text due to the low number of observations (N = 78 individuals, representing only 4 districts). These tests suggest that sample respondents in the ANES study who lived in loyalist districts did not differ significantly from respondents who lived in skeptic districts.

5.5.5 Placebo Test of the Watergate Effect

The final way in which I test the comparability of districts is by conducting a placebo test for the difference in difference regression. Here I test the treatment effect of Watergate on an election occurring *prior to* the treatment. Thus, using the fixed effects regression model, I regress the effect of treatment on the *change in Republican vote share* between 1968 and 1970 and 1970 and 1972 respectively. I estimate a fixed effects regression model that only includes the treatment effect and district and time fixed effects. Since these elections occurred *prior to* Watergate, I expect a substantially

small and statistically insignificant treatment effect. Such a finding would increase the confidence in the parallel trends assumption.

Table 5.4 shows the results of this regression model using two different models for each of the time-periods and confirms this expectation. Models 1 and 3 are run on all districts [$N = 17$], Models 2 and 4 are run on a sub-set of districts in which uncontested races and statistical outliers are excluded.⁵⁹ In line with expectations, I find substantively small estimates of the treatment effect, with the exception of Model 3. The large estimate in this model may be attributed to the fact that uncontested races are included in this analysis, inflating the vote share in loyalist districts. None of the tests reach a conventional level of statistical significance ($p < 0.53$ is the lowest result for model 3). This increases the confidence that there was no significant treatment effect prior to the occurrence of the Watergate scandal. Yet, it must be noted that the results in table 5.4 also confirm the fact that there is no perfect parallel trend in the two sets of districts prior to Watergate. If the districts would have experienced an identical trend in Republican vote share, then the treatment effect of the placebo regression would have been 0.

⁵⁹For the years 1968-1970 this results in a sample of 15 districts. Excluded are Mississippi district 5 [uncontested] and New Jersey district 13 [outlier]. For 1970-1970 again Mississippi district 5 and New Jersey district 13 are excluded, as well as Illinois district 19 which was uncontested in 1972, this leads to $N = 14$

Table 5.4: Placebo Test Difference in Difference

	1968-1970	1968-1970	1970-1972	1970-1972
	Model 1	Model 2	Model 3	Model 4
Treatment Effect	-1.92 (3.68)	-1.63 (3.68)	6.13 (6.29)	2.50 (4.09)
Intercept	-1.56 (3.61)	-1.56 (3.62)	2.61 (4.42)	-2.26 (3.47)
District Fixed Effects:	yes	yes	yes	yes
N	17	15	17	14
R-squared	0.09	0.07	0.13	0.02
Sample:	All Districts	Subset	All Districts	Subset

*** $p < .001$; ** $p < .01$; * $p < .05$

Standard Errors clustered at District Level

5.5.6 Difference in Difference Estimation

I now turn to the estimation of the Watergate effect on the 1974 midterm elections for the House of Representatives using a difference in difference estimation. Similar to the placebo tests conducted above, I estimate a fixed effects regression model that includes both the Watergate treatment effect, as well as time and district level fixed effects. The treatment effect represents the interaction of ‘Nixon loyalist’ * the 1974 time dummy. The outcome variable is the difference in vote share received by the Republican House candidate between 1972 and 1974. The results of this analysis are displayed in Table 5.5. In Model 1, I include all districts (N=17), In Model 2, I exclude unopposed races (N = 16) ; in Model 3, I exclude unopposed races and the races in which the House Representatives retired (N = 14) ⁶⁰ because we might assume that voters do not hold their replacements accountable for their predecessors impeachment vote. Finally, in Model 4, I exclude unopposed races, races with retired

⁶⁰These are NY36 and CA22 respectively.

representatives and statistical outliers (N = 12).⁶¹

Table 5.5: Difference in Difference Estimates 1972-1974 Vote share Republican Representatives

	Model 1	Model 2	Model 3	Model 4
Treatment Effect	−0.06 (6.48)	−4.35 (5.50)	−5.78 (5.90)	−4.15 (2.42)
Intercept	−8.93 (5.51)	−4.64 (4.30)	−2.47 (4.57)	−7.33* (0.88)
District Fixed Effects:	yes	yes	yes	yes
N	17	16	14	12
R-squared	0.35	0.35	0.29	0.55
Sample:	All Districts	Subset	Subset	Subset

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

Standard Errors clustered at District Level

Depending on the sample used, the treatment effect ranges from a negative 0.06% up to a negative 5.78%. All estimates in Table 5.5 suggest that the loyalist districts fared *worse* at the 1974 elections compared to the skeptic districts.⁶² The treatment effects in Table 5.5 do not reach a level of conventional statistical significance however.⁶³ This suggests that we cannot exclude the possibility that the difference in reduction in vote share between loyalist and skeptic districts occurred by chance. I further explore this in the next section where I empirically calculate a p-value for the difference in difference estimation, using the available data on all House races between 1972 and 1974.

⁶¹Please see the Appendix for a discussion of the calculation of statistical outliers, there were no outliers in 1972, but two outliers in 1974: Maine district 2 and Mississippi district 14.

⁶²Note that the effects in models 3 and 4 are quite similar to the estimates provided by Wright (1977) when he uses his comparison with the ‘hypothetical vote.’

⁶³Model 1 p < 0.9; Model 2 p < 0.4; Model 3 p < 0.3; Model 4 p < 0.12

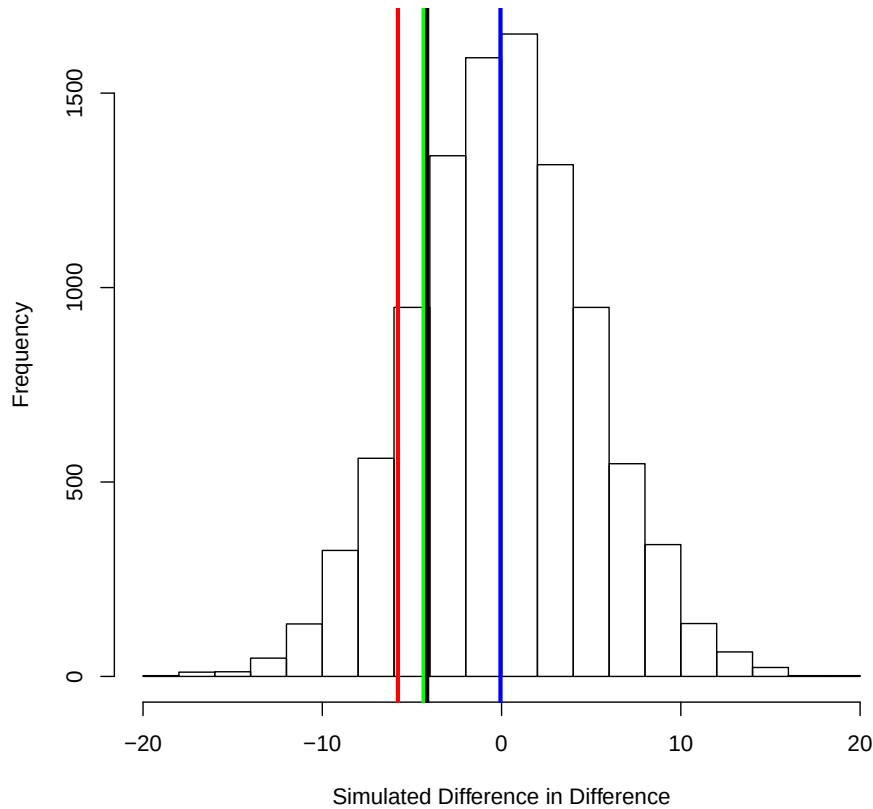
5.5.7 Data Simulations: Generating a Level of Statistical Significance

The lack of statistical significance of the Watergate effect in Table 5.5 suggests that we cannot conclude that the stronger reduction in vote share in loyalist districts is due to Watergate. An important potential reason for the lack of statistical significance lies with the small sample sizes ($N=17$ or even $N = 14$) and the consequent lack of statistical power. To tackle this problem, I conduct data simulations using the electoral outcomes of all districts in 1972 and 1974. The idea behind this simulation is to create a distribution of the average difference in difference in vote share between 1972 and 1974, using all 435 districts. This allows me to empirically estimate a p-value for the treatment effects found in Table 5.5.

The data simulation is conducted as follows. I collected the Republican vote share for the House elections for all electoral districts ($N = 435$) in 1972 and 1974. I exclude districts in which the House race was unopposed in either 1972 or 1974.⁶⁴ I then calculate the difference in the Republican vote share for each district between 1972 and 1974. I then randomly draw a selection of 17 districts, ten of these are assigned to the treatment group (loyalist) and seven are assigned to the control group (skeptics). Similar to the actual difference in difference estimation presented above, I then calculate the average difference between the 1972 and 1974 elections for both the treatment and control group. I then calculate the difference in difference in vote shares between the assigned loyalists and skeptic districts. I repeat this process 10,000 times which results in the distribution of the difference in difference between the simulated created treatment and control groups. This distribution is shown in Figure 5.4. The estimates from models 1 through 4 are depicted with four differently coloured lines

⁶⁴I also exclude districts with missing data, this leaves a sample of $N = 347$. Districts with missing data are districts which were unchallenged and where the law of the state did not require to record the outcomes of unchallenged races.

Figure 5.4: Distribution Simulation Difference in Difference Vote Share Republican House Candidates 1972-1974



in Figure 5.4, where Model 1 is the blue line, Model 2 the green line, Model 3 the red line and Model 4 the black line.

The mean of the distribution created is close to zero (-0.05). This is reassuring because it suggests that, on average, a random division of 17 districts into control and treatment groups does not render a treatment effect. The standard deviation of the distribution is 4.9. Visually, the distribution appears to constitute a normal distribution (Figure 5.4), this is confirmed by the fact that both the kurtosis and skewness are close to zero and by two tests of normality, the Kurtosis and Skewness test (Table 5.6), both of which reject the assumption that the data is not normally distributed.

Table 5.6: Descriptive Statistics Simulated Difference in Difference Distribution Vote Shares

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
-0.05	-0.05	4.9	0.08 p < 0.10	0.0 p < 0.73

Based on the simulated data, I calculate the statistical significance of the model estimates of Table 5.5. Both the one-tailed and two-tailed test results are displayed in Table 5.7. It becomes clear that none of the model estimates are statistically significant at a conventional level when using the data simulations to empirically calculate a p-value. This is true for both the one-tailed and two-tailed tests.⁶⁵ As such, the difference in the reduction in vote share between the loyalist and skeptic Republican candidates cannot be considered significant from a statistical point of view. Only the one-tailed tests for Models 2 and 3 approach the 10% significance level.

Table 5.7: Significance Difference in Difference Model Estimates

Model Estimate	Two-Tailed Test	One-Tailed Test
Model 1: -0.06	p < 0.99	p < 0.49
Model 2: -4.35	p < 0.36	p < 0.18
Model 3: -5.78	p < 0.24	p < 0.12
Model 4: -4.15	p < 0.39	p < 0.19

The Appendix (Figure A.5) provides a robustness check in which the state of California is excluded from the analysis because this state underwent re-districting between 1972 and 1974. This analysis again suggests that none of the model estimates are statistically significant.

⁶⁵P-values are calculated for a significance level of 0.05

5.5.8 The Effect of Watergate on Victory Margins

As a second exploration of the effect of Watergate on the 1974 midterm elections I explore the effects of Watergate on the margins of victory held by Republican House representatives. The main reason to explore this is the fact that other research on the effect of corruption allegations on House races suggests that the main substantive effect of these allegations is the reduction of victory margins.⁶⁶ Table 5.8 presents the victory margins of the Republican members of the Judiciary Committee in 1972 and 1974. The final column provides the difference between 1972 and 1974 and the average reductions in victory margins.⁶⁷

In 1972, the average margin of victory is 21.4% for loyalist districts, compared to 30.1% in skeptic districts, or 18.5% when I exclude the district that was unchallenged. This suggests that it is not the case that skeptic members represented districts that were necessarily more easy to retain prior to Watergate. It was also not the case that representatives who voted in favour of impeachment somehow felt more secure in their opposition to Nixon on the basis of larger victory margins. Rather, in 1972, all Republican members of the House Judiciary Committee represented districts with generous victory margins.

⁶⁶Peters and Welch (1980); Welch and Hibbing (1997)

⁶⁷Note that vote shares are calculated based on the *total* number of votes, including third parties. Victory margins represent the absolute difference between Republican and Democratic party vote shares

Table 5.8: Vote-Margins Republican Members of the House Judiciary Committee to Impeach Nixon

House Member	Victory Margin 1972	Victory Margin 1974	Difference 1974 vs 1972
NIXON LOYALISTS:			
Sandman (NJ 2)	31.5	-16	-47.5
Maraziti (NJ 13)	12.8	-14.5	-27.3
Dennis (ID10)	14.5	-8.7	-23.2
Hutchinson (MICH 4)	34.5	7.6	-26.9
Latta (OH 5)	45.5	25.1	-20.4
Mayne (IOW 6)	4.9	-9.2	-14.1
Smith III (NY 36)*	11.6	-20.6	-32.2
Lott (MISS 5)	11.2	58.6	+47.4
Moorhead (CA 22)	14.8	11.4	-3.4
Wiggins (CA 39)	33	14.8	-18.2
AVERAGE	+21.4	+ 4.8	-16.6
<i>excluding *</i>	+22.5	+ 7.7	-14.9
NIXON SKEPTICS			
Cohen (MA 2)	8.8	42.7	+33.9
Fish Jr. (NY 25)	35.9	21.2	-14.7
McClory (ILL 13)	22.9	9.01	-13.9
Railsback** (ILL 19)	100**	30.6	- 69.4
Froehlich (WISC 8)	1.90	-8.9	-10.8
Butler (VIRG 6)	15.4	18.2	+ 2.7
Hogan (MARY 5)*	25.8	-5.1	-30.92
AVERAGE	+ 30.1	+15.4	-14.7
<i>excluding **</i>	+ 18.5	+12.9	-5.6
<i>excluding * and **</i>	+17	+16.5	-0.53
Difference in Difference			-14.37%

* candidate retired in 1974

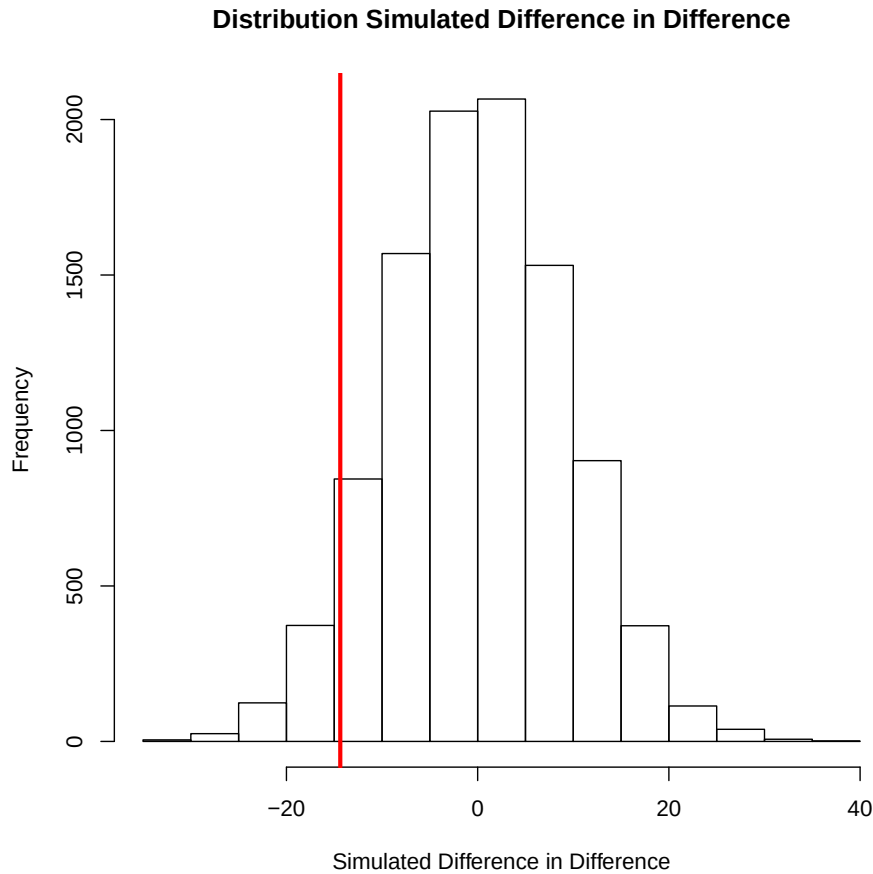
** race was unopposed

This picture changes in 1974 (post-Watergate). In loyalist districts, the average decline in the Republican victory margin between 1972 and 1974 was -16.6% compared to -14.7% in skeptic districts. This does not suggest a strong difference in the reduction of the victory margin across the two groups. However, if I exclude the races that were represented by members who retired, as well as the district that was unopposed in 1972, then I find that the average reduction in victory margin was -14.9% in loyalist districts versus -0.53% in skeptic districts, suggesting a Watergate treatment effect of -14.37% for loyalist districts.

Again, in order to gauge the statistical significance of this finding, I create a distribution of the difference in difference in victory margins across all 435 districts in 1972 and 1974.⁶⁸ I again randomly draw 17 districts, allocate ten to treatment and seven to control and calculate the average difference in victory margin for control and treatment and then calculate the difference in difference. The distribution is shown in Figure 5.5.

⁶⁸I exclude districts that were unopposed or have missing data which results in a sample of $N = 347$. Districts with missing data are districts which were unchallenged and where the law of the state did not require to record the outcomes of unchallenged races.

Figure 5.5: Distribution Simulation Difference in Difference Victory Margins Republican House Candidates 1972-1974



The average difference in victory margin of the simulated distribution between 1974 and 1972 is very close to 0, again suggesting that a random draw of 17 districts does not render a difference in victory margins. This distribution deviates slightly more from a normal distribution, specifically, it is skewed somewhat to the right, as also suggested by the skewness test (Table 5.9). The red vertical line now represents the treatment effect identified above, -14.37%. The calculation of a p-value based on the simulation suggests that this estimate borders on statistical significance at $p < 0.12$, with a two-tailed test and $p < 0.06$ with a one-tailed test. The Appendix, Figure A.6, again provides the same analysis excluding the state of California which was re-districted between 1972 and 1974. This gives very similar results ($p < 0.13$ and p

<0.06), but renders a distribution that is less skewed.

Table 5.9: Descriptive Statistics Simulated Difference in Difference Distribution Victory Margins

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
0.09	0.04	9.34	0.08 p <0.02	-0.03 p <0.46

5.5.9 The Effect of Watergate on Turnout and Third Party Votes

As a final test of the Watergate effect on the 1974 elections I estimate the difference in the decline in turnout and the difference in the increase in third party votes between the two sets of districts. First, with respect to turnout, there is a clear decline in voter turnout in both sets of districts. Compared to the total number of votes cast in 1972, the average turnout in 1974 in loyalist districts is 77.5% and 71.6% in districts represented by a Judiciary Committee member who was skeptical of Nixon. Importantly, the decline in turnout was *larger* in the skeptic districts compared to the loyalist districts. This is true whether I look at all 17 districts, but also when I exclude unopposed races and districts in which the House representative retired.

The estimates of the difference-in-difference regression model are presented below in Table 5.10. The dependent variable is the decline in turnout. The positive estimates suggest indeed that the decline turnout was lower in loyalist districts compared to skeptic districts. This contradicts the expectations of a Watergate effect. Model 1 includes all districts apart from the district that was unopposed in 1972, Model 2 excludes the unopposed district and the districts represented by members who resigned.⁶⁹ In line with the procedure followed above, I again empirically generate

⁶⁹I do not run a model for the entire sample because this would include a district that is unopposed

p-values for these estimates by simulating a difference in difference distribution of turnout. I provide this simulation in the Appendix, Figure A.7. Following this procedure I find that neither of the estimates of Table 5.10 are statistically significant. The two-tailed test results in p-values of $p < 0.26$ for Model 1 and $p < 0.52$ for Model 2. The one-tailed test generates p-values $p < 0.13$ for Model 1 and $p < 0.25$ for Model 2.

Table 5.10: Difference in Difference Estimates 1972-1974 Voter Turnout

	Model 1	Model 2
Treatment Effect	5.92 (5.71)	3.46 (5.73)
Intercept	-28.45*** (3.93)	-26.15*** (3.96)
District Fixed Effects:	yes	yes
N	16	14
R-squared	0.66	0.62
Sample:	Subset	Subset

*** $p < .001$; ** $p < .01$; * $p < .05$

Standard Errors clustered at District Level

As a final step I explore whether there is a significant difference in the increase in third party votes between the loyalist and skeptic districts. I find that this is not the case. The third party vote share *increases* in both loyalist and skeptic districts between 1972 and 1974. Yet contrary to expectation the increase is stronger in skeptic districts. Specifically, the third party vote share increases from 1.02% to 2.60% in loyal districts compared to an increase from 2.50% to 5.65% in skeptic districts. This difference in difference is not significant from a statistical point of view. I present the full regression model for the third party vote share and the simulation of the in 1972 and contested in 1974, the turnout increases with 90% between these two years which clearly biases the model estimates if this district is included.

difference in difference distribution in the Appendix (Table A.19, Figure A.8).

I thus find that there was a *decline* in turnout and an *increase* in third party votes in both loyal and skeptic districts. Yet, in neither case was there a statistically significant difference in these trends between loyalist and skeptic districts. Rather it appears that the electorate punished the Republican party across the board.

5.5.10 Discussion of District Level Analysis

The main purpose of the district level analyses has been to establish whether the electorate punished the Republican party for the events of Watergate in the 1974 midterm elections and to do so in a way that approximates a causal identification of the ‘Watergate Effect.’ The district-level comparison between loyalist and skeptic districts precluded other potential explanations for the decline in Republican vote share such as the economic recession or a general anti-incumbent midterm swing which were considered to be constant across districts. I found no statistically significant differences between loyalist and skeptic districts in the vote share received by the House representatives. Nor did I find a significant difference in the decline in victory margins, voter turnout or third party votes across the two sets of districts. On the basis of this I thus reject the assumption that the (Republican) electorate punished the Republican House representatives who voted against Nixon’s impeachment and rewarded those who voted to impeach him.

This does not imply that the electorate did not respond to Watergate at all however. In fact, the district level analyses have made clear that the Republican vote share declined in both loyal and skeptic districts, as did turnout. Furthermore, the number of third party votes increased in both sets of districts. As such, perhaps the most

likely explanation for the findings from the district level analysis is that the electorate, specifically Republican partisans, punished the Republican party as a whole, but did not target their response to the members of the Judiciary committee. I therefore now turn to the analysis of individual level voter data to test: (1) whether Republican identifiers were less likely to vote in the 1974 elections compared to Democrats and Independents; and (2) whether Republican identifiers were less likely to vote for their in-party compared to Democratic identifiers. The individual level analysis also aims to explore whether there are significant differences within the group of Republican identifiers and whether there were other important factors, such as the economy, that drove individual level decision-making in the 1974 midterm elections.

5.6 Results Part II - Voter Level Analysis

5.6.1 Data

In this section, I use the ANES panel study that covers the period 1972-1976. The panel study consists of five waves: a 1972 pre- and post-electoral survey, a 1974 post-electoral survey and a 1976 pre- and post-survey. I rely mostly on the 1972 and 1974 waves. In line with other studies, I consider the 1972 pre- and post-survey to record the public's pre-Watergate attitudes, and the 1974 wave to record the public's post-Watergate attitudes.⁷⁰ Because I am interested in tracking opinion and behavioural change over time, I only use panel respondents and exclude cross-sectional additions to the panel. This leaves a total of 1320 respondents for the period 1972-1976 and 1624 for the period 1972-1974.

As a first step I explore whether Republican panel respondents were more likely to

⁷⁰For instance Dinas (2013); Achen and Bartels (2016)

leave the panel study between 1972 and 1974. This does not appear to be the case. In the 1972 pre-election survey, 1313 respondents answered the question on partisan identification, 26.3% identified as Republican. In the 1974 post-electoral survey, 1286 respondents answered the question and 24.3% identified as Republican. In 1976, 1300 respondents answered the question and 25% identified as Republican. The drop in respondents answering the question between 1972 and 1974 did not reflect a drop-out of Republican identifiers in particular. Of the 27 respondents who did not answer the question in 1974, 8 were Republican identifiers in 1972, 8 identified as Independent or indicated no preference and 11 identified as Democrat in 1972.

Table 5.11 cross-tabulates partisan identification in the two panel waves. Here it becomes clear that those who identified as Independent are most likely to alter their partisan identification across the two panel waves, only 47% of the respondents who answer the question in the 1972 survey identifies as independent again in 1974. As expected, a larger number of Independents moves to identify with the Democratic party as opposed to the Republican party in 1974. Democratic partisans are most consistent, 89% consistently identifies as Democrat in both 1972 and 1974. This is true for 79% of Republicans. Table 5.11 suggests that approximately 20% of Republican partisans altered their party identification between 1972 and 1974, potentially in response to the events of Watergate. In the analyses presented below I use panel respondents' partisan identification as stated in the 1972, pre-Watergate, survey wave and explore their attitudinal and behavioural changes in the period 1972-1974.

Table 5.11: Partisan Identification in the 1972-1974 Panel

1974 Identification:	Democrat	Republican	Independent
1972 Identification:			
Democrat	721 (89%)	32 (4%)	56 (11%)
Republican	67 (11%)	459 (79%)	63 (10%)
Independent	63 (35%)	31 (18%)	84 (47%)

5.6.2 Identifying Cross-Pressured Republicans: Descriptive Analysis

In line with the model on cross-pressured voting central to this thesis, I first explore whether the strength of partisan affiliation (Hypothesis 1) and knowledge of politics (Hypothesis 2) moderate the extent to which a voter updates their opinion of Nixon in light of Watergate. Unfortunately the panel survey does not include any facts-based questions about politics. Thus, for a measure of political sophistication/interest I have to rely on self-reported interest in politics even though this is known to be a more biased measure when compared to factual knowledge of politics.⁷¹ The survey question I use asks respondents whether they: “follow what goes on in politics?” Answer categories are: (1) most of the time, (2) some of the time; (3) only now and then and (4) hardly at all. I collapse this in three categories where those who answered (1) are considered ‘highly interested’, those who answered (2) constitute the medium category of interest and those who answered ‘only now and then’, or ‘hardly at all’ are considered to have ‘low’ levels of interest in politics.

As a first indication of the response of the panel respondents to the events of Watergate I explore the decline in Nixon’s popularity among all panel respondents. This confirms the fact that the electorate as a whole responded negatively to the events of Watergate.⁷² Nixon’s thermometer ratings dropped from 67% to 40% between 1972

⁷¹Price and Zaller (1993)

⁷²See Dinas (2013) for a discussion of differences in sensitivity between younger and older voters

and 1976 (Figure 5.6a). The popularity of the Democratic party leader increased in the same period of time.⁷³ Figure 5.6b displays Nixon's popularity ratings for three groups of respondents: Republicans, Democrats and Independents.⁷⁴ Group identification *moderates* the response to Watergate in the sense that Nixon's popularity remains highest among Republican identifiers. Yet, it is interesting to note that the decline in Nixon's popularity appears approximately equal across these three groups.

Figure 5.6c now plots Nixon's popularity across three categories of Republican partisan identification. Interestingly, Figure 5.6c suggests that the decline in Nixon's popularity was somewhat stronger among moderate partisans compared to weak and strong Republican partisans.⁷⁵ This thus suggests a variation to Hypothesis 1 which assumed that resistance to cross-pressure increases as the strength of partisan affiliation increases. Instead, there appears to be a very minor difference between partisans with *moderate* levels of identification and those with *strong* or *weak* levels of identification. Specifically, partisans with moderate levels of party affiliation display a stronger decline in Nixon affinity. I explore in the next section whether this is a significant difference.⁷⁶

which shows that although both became more negative about Nixon, the event of Watergate mattered more to young voters in the sense that, compared to older voters, it reduced young people's reliance on prior attitudes such as partisan identification and Nixon affect.

⁷³In 1972 the score for the Democratic Party Leader is the thermometer rating of McGovern. In 1976 it reflects respondents' ratings of Jimmy Carter. In 1974 respondents were asked for their opinion on a range of Democratic candidates, but not Jimmy Carter. Thus, the estimate for 1974 reflects the average rating for the following candidates: Henry 'Scoop' Jackson, Edward (Ted) Kennedy, Hubert Humphrey and Walter Mondale.

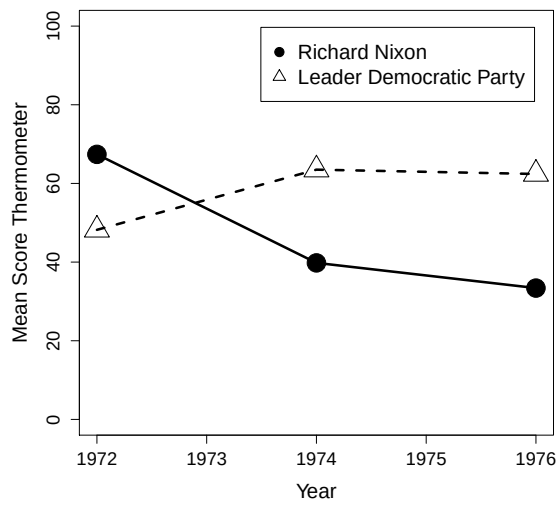
⁷⁴Identification is determined based on the partisan affiliation that the respondents reported in the particular survey year.

⁷⁵Respondents are identified as weak/moderate/strong partisans on the basis of the strength of their affiliation in the 1972, pre-Watergate survey

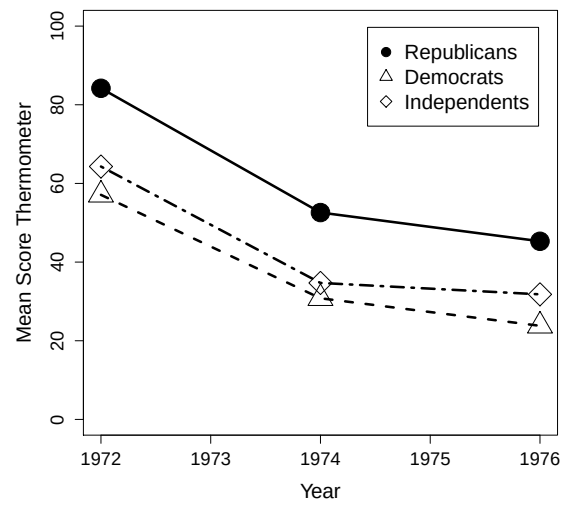
⁷⁶See Zaller (1992); Stoker (1993) who present similar findings in terms of political knowledge.

Figure 5.6: Nixon Thermometer Scores ANES Panel 1972-1976

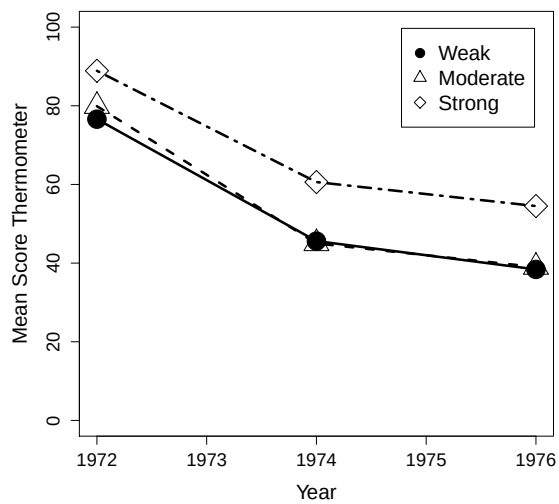
(a) Entire Panel



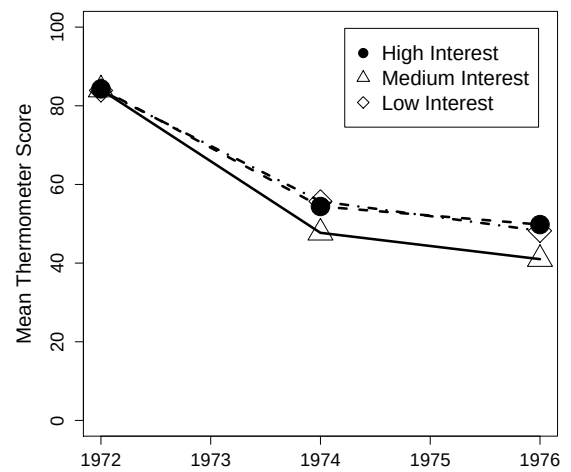
(b) Partisan Groups



(c) Levels of Republican Partisan Identification



(d) Political Interest - Republicans Only



Finally, Figure 5.6d plots Nixon’s popularity among Republican partisans with various levels of interest in politics. This plot appears to confirm Hypothesis 2: there is a steeper decline of Nixon’s popularity among Republican identifiers with *moderate* levels of interest in politics compared to Republican identifiers with high and low levels of interest.⁷⁷

5.6.3 Identifying Cross-Pressured Republicans: Regression Analysis

I now run an ordinary least squares regression analysis (OLS) where the dependent variable is the *change* in Nixon’s popularity between 1972 and 1974 (post-Watergate). Since the thermometer score ranges between 0-100, this variable ranges between negative 99 and positive 99. The independent variables of interest are the strength of a voters’ partisan identification in 1972 and their level of interest in politics in 1972. I further control for demographic variables measured in 1972 such as age, gender, employment and levels of education. I also include a measure of economic satisfaction in 1974 to control for the fact that some of the negativity projected onto Nixon might be driven by the economic downturn of the early ’70s.⁷⁸ The models presented in Table 5.12 account for the fact that respondents are nested geographically and includes state-level fixed effects. Note that in this analysis I focus only on Republican partisans because these partisans are considered to be cross-pressured by the events of Watergate. Partisanship is recorded *prior to* Watergate, in the 1972 survey.

⁷⁷Survey question used to determine level of interest in politics is: ‘Some people seem to follow what’s going on in government and public affairs most of the time, whether there’s an election going or not. Others aren’t that interested. Would you say you follow what’s going on in government and public affairs most of the time, some of the time, only now and then or hardly at all?’ Responses are collapsed in three categories denoting: most of the time (high interest); some of the time (moderate interest) and only now and then or hardly at all (low interest). I identify Republicans on the basis of their stated partisan identification in 1972 and level of interest stated in 1972.

⁷⁸The race variable is excluded from the analysis because there are too few observations of non-white respondents. The Appendix presents a robustness check including the race variable. This does not alter the results presented in the main text.

Table 5.12: Ordinary Least Squares Model: Change in Nixon Thermometer Scores - Republican Partisans

	Model 1	Model 2
Age	-0.08 (0.12)	-0.10 (0.12)
Female	5.12 (3.00)	4.36 (3.17)
In Employment	0.65 (2.63)	0.68 (2.56)
High School	-3.84 (3.75)	-3.53 (3.73)
High School +	-8.08* (3.26)	-7.13* (3.46)
Religious	1.89 (2.98)	1.71 (2.97)
Economy got better	-1.59 (3.96)	-1.80 (4.04)
Economy got worse	-4.23 3.51	-4.19 (3.62)
<i>baseline: weak partisan</i>		
Moderate partisan	-1.13 (2.61)	-1.36 (2.68)
Strong partisan	3.31 (3.14)	3.47 (3.10)
<i>baseline: low interest</i>		
Medium interest	-	-8.20** (2.42)
High interest	-	-4.33 (2.73)
Constant	-33.10* (12.68)	-26.91* (13.04)
State Fixed Effects	Yes	Yes
N	566	566
Adj. R-squared	0.03	0.02

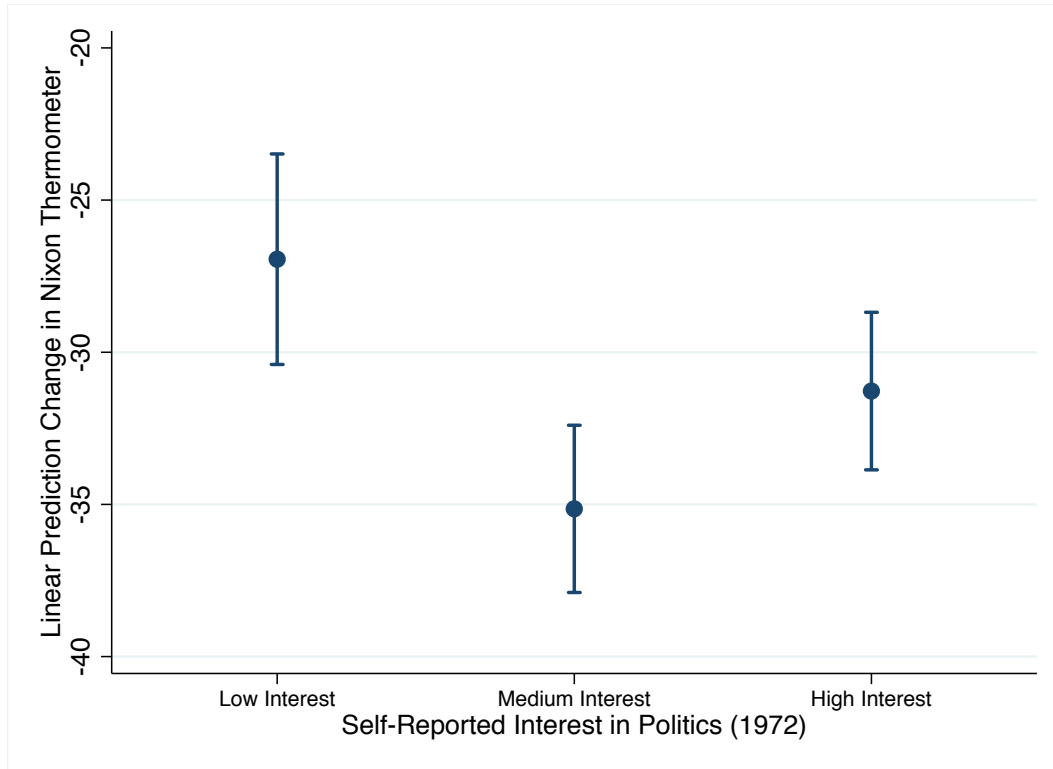
***p < .001; **p < .01; *p < .05

Robust Standard Errors in Parentheses

Models include Republican partisan identifiers only

Table 5.12 presents two models. Model 1 tests for Hypothesis 1, the effect of strength of partisan affiliation. I do not find confirmatory evidence of Hypothesis 1. There are no significant differences between weak, moderate and strong Republican partisans as to their decline in Nixon approval. Rather it appears that all Republican partisans, regardless of the strength of their affiliation with the party, were equally likely (or unlikely) to adjust their approval of Nixon in the wake of the Watergate scandal. Model 2 however offers confirmatory evidence of an effect of political interest. Specifically, Republican partisans with *moderate* levels of interest in politics were more likely to negatively adjust their opinion of President Nixon compared to those with low levels of interest. It must be noted that here partisans with *moderate* levels of interest in politics can only be distinguished from those with *low* levels of interest, not from those with *high* levels of interests. This thus partially confirms Hypothesis 2: the strongest decline in Nixon's popularity is found among respondents who report a *moderate* level of interest in politics compared to those with *low* levels of interest in politics. Figure 5.7 displays the substantive implications of this finding and depicts the predicted decline of Nixon approval for Republican partisans with different levels of interest in politics.

Figure 5.7: Predicted Decline Nixon Thermometer Score Among Republican Partisans with Different Levels of Interest in Politics



5.6.4 The Behaviour of Cross-Pressured Partisans in the 1974 Midterm Elections

I now explore the behaviour of Republican partisans in the 1974 midterm elections. The district level analysis suggests that there was a general decline in turnout in 1974 compared to 1972 and in this section I explore whether those who identified with the Republican party prior to Watergate were less likely to vote in 1974. This is a test of Hypothesis 4 - abstention. A second finding of the district level analysis is that the third party vote increased between 1972 and 1974. Unfortunately however, I am not able to test whether this is mainly due to defection from Republican partisans because in the ANES data only 9 respondents report a third party vote in 1974. I therefore explore a more general question and, in line with Hypothesis 5, test whether Republican partisans were less likely to vote for their in-party (the Republican party) in 1974 compared to the likelihood that a Democratic voter voted for the Democratic

party. In the following section I thus assume that in the wake of Watergate, Republican partisans experienced cross-pressure and Democratic partisans and Independent identifiers did not.

Abstention in the 1974 Midterm Elections

I first explore whether panel respondents who identified with the Republican party in the 1972 wave (pre-Watergate) were more likely to abstain from voting in 1974 compared to voters who identified with the Democratic party or who identified as Independent (Hypothesis 4). To test this, I run a binary logistic regression model with dependent variable ‘voting in 1974’. This variable takes the value of 1 if a respondents reports to have voted in 1974 and takes the value of 0 otherwise. Two important challenges of the data must be mentioned. First, the total number of panel respondents for the period 1972-1974 is $N = 1,614$, yet only $N = 967$ respondents, or 60%, have answered this survey question. Second, of the 967, only 46 respondents report not to have voted. I therefore present two models in Table 5.13. Model 1 is a regular binary regression model, Model 2 is a ‘rare events’ binary regression model which uses a method of penalized likelihood to account for the small number of cases of abstention.⁷⁹

In addition to Watergate, there are two other important potential explanations for a decline in turnout in 1974 and an anti-Republican vote. The first is the state of the national economy, which stagnated in the period 1972-1974 and was classified as a recession in 1973. I therefore include a variable in the regression models that captures whether a respondent thinks the economy has remained the same, improved or gotten worse. This variable is measured in the 1974 panel wave. In addition to Watergate and the national economy, a third potential driver of voting behavior in 1974 might

⁷⁹For a discussion on this type of model see King and Zeng (2001)

be a respondent's opinion on the Vietnam war. A question on the Vietnam war is included in the 1972 survey wave and asks respondents whether they think the US should withdraw from Vietnam. Unfortunately however, only 733 respondents (45%) have answered this question and therefore including this in the regression analysis (especially the abstention model) severely reduces the number of observations. I therefore do not include this variable in the main results presented here, but present a replication of the models presented here with the inclusion of the Vietnam variable in the Appendix, Table A.23. Table A.23 also includes two additional demographic controls: race and religion which contain a large number of missing observations. The results presented here are robust to the inclusion of these three additional variables.

The main independent variable of interest in Table 5.13 is the variable which reflects partisan identification. Republican partisans are split into three groups according to the strength of their affiliation with the party: weak, moderate and strong identifiers. Table 5.13, Model 2, suggests that Republican identifiers with moderate levels of affiliation to the party are significantly less likely to report to have voted in the 1974 elections compared to Democratic identifiers and Independents. Differences between various categories of Republican identifiers are not significant. Taking into account the fact that only 60% of panel respondents answered this question, these findings suggest that moderate Republican identifiers were less likely to vote in the 1974 elections.⁸⁰ To the extent that these models control for respondents' opinion on the state of the economy and prior turnout (1972 elections), this response can be attributed to the events of Watergate.

⁸⁰A similar dynamic has been reported for voters' response to other corruption cases. See for examples Chong et al. (2012); Davis et al. (2004); McCann and Dominguez (1998); Slomczynski and Shabad (2011); Stockemer et al. (2012)

Table 5.13: Binary Logistic Regression Model: Voter Turnout in the 1974 Midterm Elections

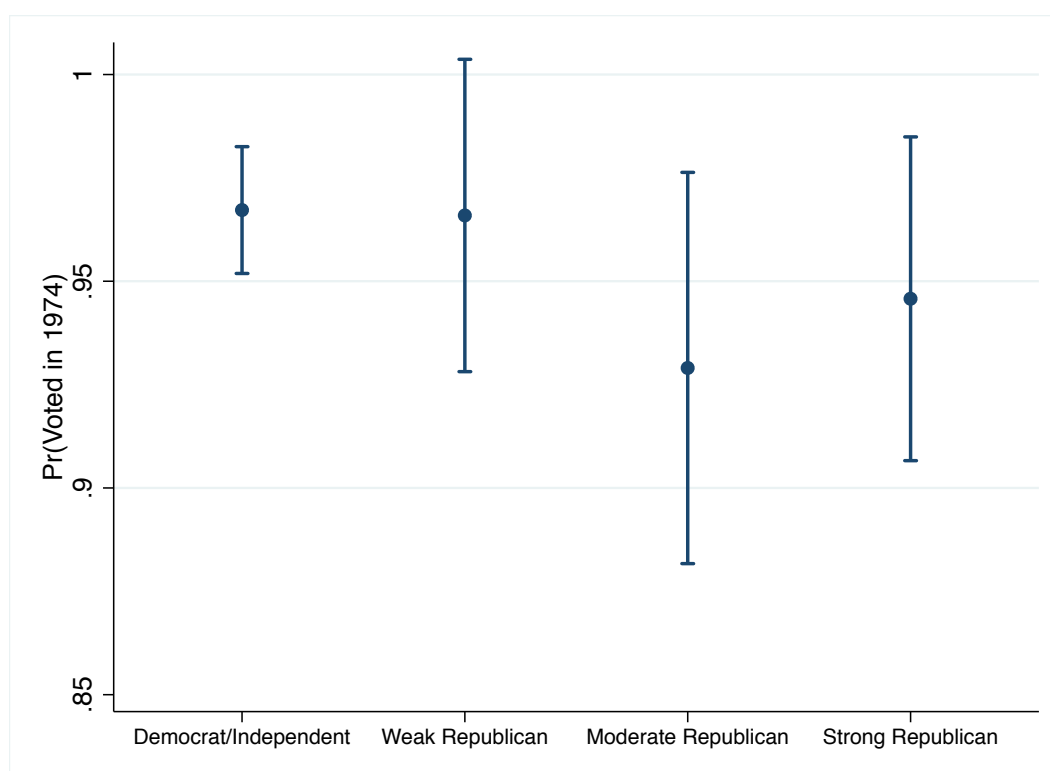
	Model 1	Model 2
Voted in 1972	-0.23 (1.13)	- 0.51 (0.86)
<i>base category: Democrats and Independents</i>		
Weak Republican	0.23 (0.68)	- 0.16 (0.60)
Moderate Republican	-0.60 (0.22)	-0.84 * (0.44)
Strong Republican	-0.19 (0.51)	-0.57 (0.45)
Age	0.00 (0.01)	0.01 (0.01)
Female	-0.46 (0.41)	-0.49 (0.38)
Employed	-0.22 (0.43)	-0.26 (0.39)
<i>base category: no high school</i>		
High School	1.35* (0.58)	1.07* (0.53)
High School +	1.19* (0.48)	1.13** (0.42)
<i>base category: economy stayed same</i>		
Economy got better	-0.02 (0.49)	0.04 (0.44)
Economy got worse	0.85 (0.46)	0.56 (0.42)
<i>base category: not interested in politics</i>		
Medium Interest	-0.26 (0.58)	-0.37 (0.52)
High Interest	-0.00 (0.58)	-0.16 (0.53)
Constant	- (2.15)	4.23* (2.15)
N	454	847
LR Chi-Squared	14.71	-
Wald Chi Square Statistic	-	14.12
State Fixed Effects	Yes	No

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

Robust Standard Errors in Parentheses

Substantively, the predicted likelihood of a moderate Republican respondent reporting to have voted is approximately 93% compared to 97% of Democratic identifiers and Independents. This is displayed in Figure 5.8. Note however that because post-estimation is not possible after the rare events model I use the estimates from Table 5.13 Model 1. Yet because the coefficient for “moderate Republican” is not significant in Table 5.13 Model 1, the confidence intervals in Figure 5.8 overlap. The main reason to present the predicted likelihoods in Figure 5.8 is to give an indication of the substantive difference in the likelihood of voting for all partisan identifiers.

Figure 5.8: Predicted Likelihood of Voting in the 1974 Midterm Elections based on 1972 pre-Watergate Partisan Identification



In-Party Vote in the 1974 Midterm Elections

Taking into account the low response rate to the abstention item, the analysis suggests that moderate Republican identifiers were less likely to vote in the 1974 midterm elections compared to panel respondents who identified with the Democratic party or as Independent in 1972. This thus partially explains the decline of the Republican vote share in the 1974 midterm elections. The next question I explore is whether, when they voted, Republican partisans were actually less likely to vote for the party they identified with and whether this differs across levels of partisan identification. I exclude Independent identifiers from this analysis because it is not clear what the in-party of an Independent is.

I therefore create a dependent variable ‘in-party vote’ which takes the value of 1 if a panel respondent identified as Republican in 1972 (prior to Watergate) and voted for the Republican party in 1974 (post Watergate). The dependent variable takes the value of 0 if a respondent identified as Republican in 1972 but voted for the Democratic party or a third party in 1974. For Democratic identifiers the variable takes the value of 1 if they identified with the Democratic party in the 1972 wave (pre-Watergate) and voted for the Democratic party in 1974 (post-Watergate) and the value of 0 if they voted for a third party or the Republican party. Note that the majority of out-party votes are not for a third party, but rather for either the Republican or Democratic party.

The resulting variable, ‘in-party vote’, has 798 responses, 20% of whom report a mis-match between pre-Watergate identification and 1974 vote choice. I estimate a binary logistic regression model with dependent variable in-party vote to test whether Republicans were significantly less likely to vote for their in-party compared to Democratic partisans and whether there are any differences between Republican partisans.

I again include a variable on economic dissatisfaction to control for the fact that the economic recession might have been an important reason for Republican voters to vote against the Republican party. Appendix Table A.24 presents a model that also includes a variable reflecting a voter's opinion on the war in Vietnam and additional controls for a respondents' race and religion. The substantive findings presented here are robust to the addition of the Vietnam variable, which does not exert a significant effect on in-party voting in 1974. Yet, the significance of the estimates reported below in Table 5.14, Model 1 disappears when race and religion are included (Table A.24), yet the model-fit (LR Chi-Squared) of the model with race and religion is worse than the model reported here in the main text and includes only half the sample.

Model 1 in Table 5.14 offers confirmatory evidence of Hypothesis 5: Republican partisans with weak and moderate levels of partisan affiliation are significantly less likely to vote for they identified with in 1974 compared to Democratic identifiers. Respondents who reported a strong partisan affiliation with the Republican party however are no less likely to vote for the Republican party in 1974 compared to Democratic identifiers. Note that weak and moderate Republican partisans do not differ significantly from each other, neither do moderate and strong partisans. Weak and strong Republican partisans do differ significantly, with the latter more likely to vote for the Republican party in 1974. This suggests, in line with expectations, that Republican identifiers with strong party affiliations did not alter their behaviour in the 1974 elections in response to Watergate.

Table 5.14: Binary Logistic Regression Model: In-Party Vote in the 1974 Midterm Elections

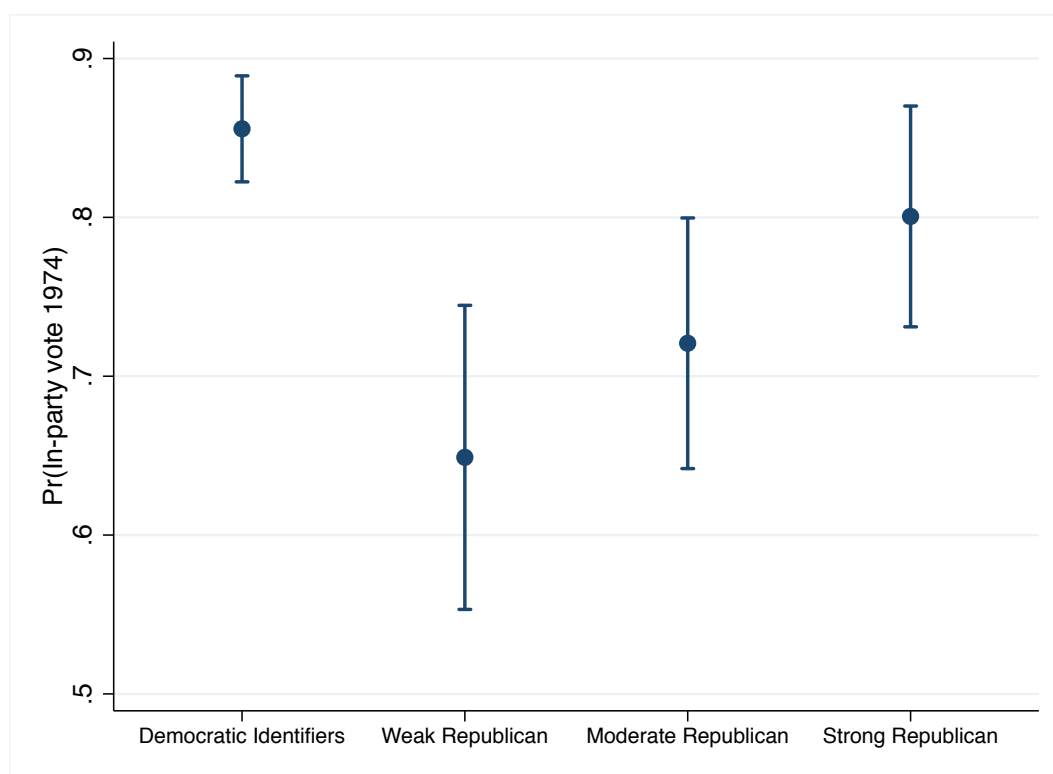
	Model 1
<i>base category: Democratic partisans</i>	
Weak Republican	−1.25*** (0.28)
Moderate Republican	−0.95 *** (0.27)
Strong Republican	−0.52 (0.28)
Age	0.00 (0.01)
Female	−0.24 (0.21)
Employed	−0.44 * (0.22)
<i>base category: no high school</i>	
High School	−0.03 (0.31)
High School +	−0.29 (0.26)
<i>base category: economy stayed same</i>	
Economy got better	0.16 (0.25)
Economy got worse	0.43 (0.23)
<i>base category: not interested in politics</i>	
Medium Interest	−0.28 (0.31)
High Interest	−0.41 (0.31)
N	774
LR Chi-Squared	39.97***
State Fixed Effects	Yes

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

Robust Standard Errors in Parentheses

Substantively, panel respondents who identified with the Democratic party in 1972 had a likelihood of voting for the Democratic party in 1974 of 86%. For weak Republicans this is 65%, for moderate Republicans the likelihood is 72%. Strong Republicans on the other hand have a predicted likelihood of voting for the Republican party in 1974 of 80%. This is displayed with 95% Confidence Intervals in Figure 5.9. Figure 5.9 uses the estimates of Model 1, setting all variables to their mean value and excludes the state fixed effect.

Figure 5.9: Predicted Likelihood of Voting for the In-Party in the 1974 Midterm Elections based on 1972 pre-Watergate Partisan Identification



5.7 Discussion and Next Steps

In this chapter I have explored the response of Republican partisans to one of the largest scandals in the history of American politics: the Watergate scandal. I have presented two types of analysis to assess whether or not Republican partisans punished the Republican party in response to the Watergate scandal in the 1974 midterm elections. The individual analysis shows that the electorate as a whole updated its opinion of President Nixon in response to the events of Watergate. This is true even for Republican voters with strong levels of affiliation to the party. This suggests that once a scandal the size of Watergate erupts, even partisans find it difficult to ignore this.

There are important differences between various Republican partisans as to their electoral behaviour however. I do not find any evidence that the attitudinal changes of strong Republicans result in behavioural changes. Yet, I find that Republican partisans with *moderate* levels of affiliation to the party were both less likely to vote in 1974 and less likely to vote for the Republican party. This group, approximately 40% of the Republican partisan electorate according to the ANES panel, thus punished their party for its involvement in the Watergate scandal. Republicans with weak levels of party affiliation were likewise less likely to vote for the party in 1974. This is in line with the previous two chapters where I found that under certain conditions partisan voters *do* exercise accountability toward their in-party. The fact that moderate partisans were most responsive to the Watergate scandal may highlight a mechanism of partisan affiliation that is similar to the mechanism of ‘acceptance and resistance’ as described by Zaller (1992) with respect to political interest.⁸¹ Moderate partisans might have a higher awareness of political developments compared to weak partisans and a lower level of resistance compared to partisans with strong attachments to the

⁸¹This is also discussed in Chapter 2, section 2.6.2

party. This combination could make moderate partisans most likely to exercise accountability toward the in-party in the wake of a political scandal.

It is interesting to note that in contrast to the previous two chapters I now find that cross-pressured partisans, in this case Republican partisans, were less likely to vote at election time compared to non cross-pressured partisans, in this case Democratic partisans and Independents. This finding supports several other studies that find that knowledge of corruption *depresses* voter turnout.⁸² It is often thought that information about corruption facilitates its electoral punishment. Yet, in the case of Watergate this does not appear to be the case. Rather, both the individual and district level analysis suggest that turnout decreased in the wake of the Watergate scandal. Knowledge of corruption may thus dissuade people from voting.

I did not find this dissuasion effect in the chapter on economic cross-pressure (Chapter 3) or cross-pressure in light of candidate competence (Chapter 4). A potential explanation for this discrepancy might lie with the difference in the type of performance failure that voters are confronted with across these chapters and the consequent different types of partisan motivation that are activated. Existing research in the field of psychology suggests that ingroup bias, of which partisanship is an example, manifests itself in a variety of ways depending on the *type* of group identity that is activated: instrumental or expressive. Importantly, these different types of group identification are often linked to different types of behaviour. Instrumental group identification is focused on the attainment of (material) goals and leads to competitive behaviour between in-groups and out-groups. Expressive group identification on the other hand is focused on the creation of a positive group identity and often leads to non-competitive, often symbolic types of behaviour that are focused on maintaining a positive image of

⁸²Chong et al. (2012); Davis et al. (2004); McCann and Dominguez (1998); Slomczynski and Shabad (2011); Stockemer et al. (2012)

the self and the group.⁸³ A connection might thus exist between the type of incumbent performance that a voter evaluates and the type of partisan response (vote-switching versus abstention) that occurs.

In addition to the analysis of individual level survey data, I have also presented an analysis of the 1974 midterm elections at the district level. This analysis suggests that the electoral response in 1974 was not targeted toward the members of the House Judiciary Committee on Impeachment. Rather, it seems that weak and moderate Republican partisans turned away from the Republican party as a whole. Although Republican House representatives who attempted to protect President Nixon against impeachment fared worse in the 1974 elections compared to House representatives who voted to impeach him, this difference in vote share, victory margins, turn out or third party votes cannot be attributed to Watergate. The question thus remains what might explain the fact that four of the five Republican members of the committee who lost their seat were Nixon loyalists. One explanation may be that that President Nixon's resignation successfully averted blame away from the individual House Representatives who supported him. I cannot find any evidence to suggest that Nixon loyalists faced particularly strong challengers when compared to Nixon skeptics. Rather it appears that the withdrawal of campaign funding on the Republican side and the simultaneous increase in funding on the Democratic side harmed all Republican candidates in 1974.

It must be noted that although I find evidence in favour of a Watergate response when using the individual level survey data, concerns may exist about this analysis with respect to its ability to causally identify the Watergate effect. For the analysis of the ANES data I use the 1972 panel as the pre-Watergate context and the 1974

⁸³See Scheepers et al. (2006) for an overview of the literature on expressive and instrumental in-group bias and their different manifestations and motivational processes.

panel as the post-Watergate outcome. The ‘treatment’ that is administered between the two panel waves is the Watergate scandal. In the analysis I further control for other potential developments in the same time-period that might have affected behavioural change, the most important of which are the economic recession and the war in Vietnam. Yet, the potential for unobserved variable bias always exists when using survey data. Thus, there might be other issues or events that occurred in the period 1972-1974 that caused Republican voters to turn away from the Republican party that I have not accounted for. One example that has already been mentioned is the existence of an ‘indirect’ Watergate effect, or the ‘strategic politicians hypothesis.’ Jacobsen et al. (1986) show that Republican challengers were the least funded group of challengers since 1960, whereas Democratic challengers in 1974 were the best funded groups of challenger candidates. As such, the anti-Republican vote in 1974 was likely not solely caused by behavioural and attitudinal changes among voters in response to Watergate, but should at least partially be attributed to the strategic behaviour of parties and campaign donors.

A second problem that may exist with the individual level analysis is the fact that survey items might be biased by voters’ opinions on Watergate. This is true for survey items measured in the 1974 (post-Watergate) wave such as the survey item that reflects a voter’s opinion on the national economy. It is thus difficult to capture the causal effect of Watergate when using individual level survey data. This was of course the main reason to use the district level difference in difference design in addition to the ANES analyses. Yet, in the difference in difference analyses I did *not* find a significant Watergate effect on the 1974 midterm elections.

In light of this, I move away from the use of survey data in the next empirical chapter. Instead I present a lab experiment that tests voters’ electoral response to candidate

corruption. There are several important benefits to the experimental approach. Most importantly, the lab experiment allows me to control the information environment in a way that is impossible with real-world case studies. Thus, although voters in 1974 might have ignored information about Watergate or updated their partisan identification in light of the events, in the next chapter participants in the experiment are unable to do so. Furthermore, in the lab experiment candidates are matched-up randomly such that I rule out strategic party behaviour. Candidates are furthermore unable to offer trade-offs to individual voters (either material or immaterial) in order to compensate for corrupt behaviour. Partisan identification in the experiment is thus explicitly expressive in nature. The experiment presented in the next chapter offers a causally identified test of the effect of expressive group identification on the electoral punishment of political corruption.

Table 5.15: Evidence for Hypotheses tested in Chapter 5

Hypothesis	Evidence
1. Strength of partisanship moderates likelihood of cross-pressure	X
2. Political knowledge moderates likelihood of cross-pressure	✓
4. Cross-pressured partisans are equally likely to abstain compared to non cross-pressured partisans	X
5. Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans	✓

Chapter 6

A Lab Experiment on Corruption and Cross-Pressure

Party identification may act as a drag on democratic accountability, or it may function as a highly efficient information shortcut.

- Eric Groenendyk (2013, p.5) *Competing Motives in the Partisan Mind* -

6.1 Introduction

The central aim of this thesis is to explore under what conditions partisans exercise accountability toward their preferred candidate or party. In the previous chapters I have therefore focused on situations in which partisans were confronted with cases of poor performance involving their in-party president or candidate. Yet, several difficulties exist when using observational data to study cross-pressure between partisan preferences and retrospective performance evaluations. First, incumbent politicians and political parties might attempt to *hide* instances of poor performance from the electorate to avoid punishment. This is true especially when illegal or corrupt acts are concerned. This limits voters' abilities to accurately exercise accountability. Second, partisans may avoid cross-pressure in light of poor performance in several ways. Partisans might avoid reading or listening to media which disseminate negative in-

formation about their preferred party or candidates.¹ Furthermore, if partisans *do* receive negative information about their party, they can choose to ignore this information, re-interpret it in more positive terms, or simply decide it is not important.² Finally, voters might update their partisanship in light of a performance failure in order to avoid the experience of cross-pressure.³ Corrupt candidates might decide to retire to avoid punishment for instance or strong challenger candidates might seek out corrupt candidates to run against in which case the punishment of corrupt candidates could be partially due to the behaviour of parties as opposed to attitudinal changes among voters.

This chapter avoids many of these challenges through the use of a lab experiment that simulates cross-pressure between group identification and performance failure.⁴ The lab experiment centers on repeated election rounds in which participants are allocated to either take the role of a voter or of a candidate. Candidates and voters participate in multiple election rounds. At the end of every election round, candidates are asked to disseminate valuable tokens to voters in order to win their votes. Candidates are provided with the opportunity to act in a corrupt fashion by holding back tokens. Voters are informed about the honesty level of the candidates via a random audit and are then asked to choose between the two competing candidates they are presented with. In the experiment, partisan identification is simulated via the creation of two distinct group identities. Group identities are based on participants' preferences for one of two painters. As such, there is a control group where neither voters nor

¹O'Keefe and Mendelsohn (1974) for instance find that voters who voted for Nixon in 1972 were less likely to follow the Watergate hearings compared to those who voted for McGovern.

²This bias might be conscious or subconscious. See for examples of this work on the response of Democratic voters to the Clinton-Lewinsky scandal Zaller (1998); Fischle (2000); Owen (2000) For examples of this in case of Watergate see O'Keefe and Mendelsohn (1974) and in the case of Gary Heart by Stoker (1993)

³See Dunlap and Wisniewski (1978) for an example of Republican partisans in light of Watergate.

⁴The lab experiment is also part of a co-authored paper with Hector Solaz and Catherine de Vries entitled *In-Group Loyalty and The Electoral Punishment of Corruption*

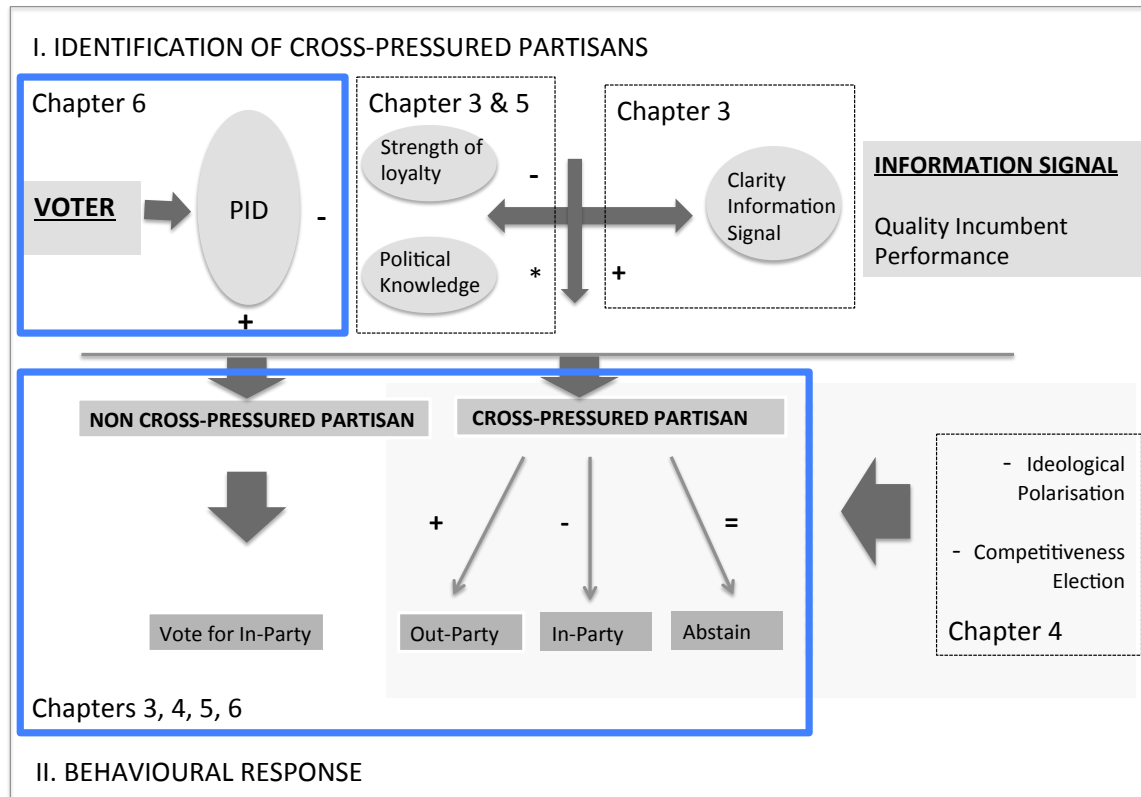
candidates hold a particular group identity and a treatment group where two group identities are created.

The question the experiment addresses is whether the introduction of group membership (such as partisanship in real-life) affects the punishment of candidates who embezzle funds. Contrary to the previous chapters, I am better able to control the information environment in which voters operate in this chapter. Moreover, I am able to rule out alternative explanations for the (absence) of electoral accountability. The chapter thus provides a direct - and causal - test of the central behavioural hypothesis of the framework of cross-pressured voting (H5) and explores whether cross-pressured partisans are *less* likely to vote for the in-party candidate when they receive information that this candidate is corrupt. The chapter proceeds as follows. First, in section 6.2, I briefly discuss the existing literature on the electoral response to political corruption. Then, in section 6.3, I discuss the main reasons to use an experimental method to study cross-pressure between partisan identification and corruption. I then discuss the experimental set-up in section 6.4, followed by the results in section 6.5. The final section, 6.6, reflects on the findings of this chapter in light of the framework of cross-pressured voting and the central research question.

Table 6.1: Hypotheses Tested in Chapter 6

Behavioural Response of Cross-Pressured Partisans
H5: Cross-pressured partisans are less likely to vote for the in-party compared to non cross-pressured partisans

Figure 6.1: Framework of Cross-Pressured Voting - Chapter 6



6.2 Why Focus on Political Corruption?

The central research question of the thesis seeks to explore the conditions under which the accountability mechanism of democratic government works and when it fails to do so. To study this, I have explored several case studies in which partisans are presented with information signals about the performance of their preferred party or candidate that contradict their partisan preferences. In this chapter I focus on corruption allegations because instances of corruption send a clear and negative signal to voters about the performance of the incumbent. As such, the study of the electoral response to corruption provides a helpful case study through which to assess whether or not partisans exercise accountability toward their preferred party.

What is more, corruption allegations are an important element of many American elections. In the most recent 2016 US presidential elections for instance, both presidential candidates accused their opponent of being corrupt. Hilary Clinton was referred to as ‘the Queen of Corruption’ by conservative media, and Donald Trump was accused of having avoided paying income taxes, thereby cheating the American public out of his contributions to public provisions. Trump was furthermore accused of paying off a District Attorney to avoid prosecution in the case lodged against Trump University.⁵ Another example are the 2006 US midterm elections which took place in a ‘culture of corruption’, especially on the side of the Republican party.⁶ Various House candidates were involved in a high-profile scandal involving the lobbyist Jack Abramoff who has since been convicted for fraud, bribery and tax evasion. As discussed in the previous chapter, historically, there have been many corruption scandals that involved members of the US House of Representatives. Most noteworthy was the 1992 Banking Scandal which revealed that representatives of both sides held significant overdrafts with the House Bank.⁷

Political corruption allegations are thus an important part of many American elections and election campaigns. Corruption may take many forms. The term corruption reflects a broad categorisation that is used to capture a range of different behaviours such as the abuse of power, bribery and exchanges of favours between political officials and other powerful figures. In this chapter, I focus on the direct embezzlement of funds because this type of corruption is easy to simulate in a lab setting and has direct effects on the wellbeing of citizens. Regardless of the exact type of corruption commit-

⁵A Conservative magazine referred to Clinton as ‘the Queen of corruption’, <http://spectator.org/hillary-queen-of-corruption/> - accessed October 26, 2016. For the allegations against Trump see <http://time.com/4484399/donald-trump-hillary-clinton-corruption/> , accessed on November 8, 2016.

⁶Mondak and Mitchell (2009)

⁷See for studies on corruption in the US House of Representatives Peters and Welch (1980); Welch and Hibbing (1997); Alford et al. (1994); Jacobsen and Dimock (1994)

ted, the consequences of corrupt behaviour are universally negative. Corruption has been shown to have a detrimental effect on tax revenues,⁸ investments and economic growth,⁹ equality,¹⁰ subjective wellbeing and life satisfaction¹¹ and has been shown to decrease citizens' trust in government.¹² Some studies have furthermore shown that corruption reduces electoral turnout and as such minimizes democratic representation and participation.¹³ I also found this in the previous chapter on Watergate. As such, it is important to study the electoral response to corruption because it is an important condition in which the electoral system should function as a mechanism of punishment and reward.

Evidence is mixed however as to the electoral responsiveness to political corruption. Although some research suggests that corruption is punished at election time,¹⁴ other studies find that even in countries with strong political institutions, corrupt politicians are re-elected.¹⁵ One of the main puzzles in the literature on political corruption therefore is: why do voters re-elect politicians that are corrupt? In seeking an explanation for this, many studies focus on differences between electoral institutions. Systems with proportional representation (PR) are thought to increase the occurrence of corruption and simultaneously hamper the punishment thereof.¹⁶ Others have argued that clear lines of accountability as well as the availability of feasible challengers

⁸Pani 2010

⁹Del Monte and Papagni (2001); Mauro (1995)

¹⁰Chong and Calderon (2000); Gupta et al. (2002); Uslaner (2008); You and Khagram (2005)

¹¹Tay et al. (2014)

¹²Anderson and Tverdova (2003)

¹³Chong et al. (2012); Davis et al. (2004); McCann and Dominguez (1998); Stockemer et al. (2012)

¹⁴Fackler and Lin (1995); Ferraz and Finan (2008); Krause and Méndez (2009); Winters and Weitz-Shapiro (2013); Klasnja et al. (2016)

¹⁵Rundquist et al. (1977); Reed (1996); Chang et al. (2010); Vivyan et al. (2012); Eggers (2014) For overviews of the literature on corruption see Golden (2010); de Vries and Solaz (2017)

¹⁶See for this argument Chang and Golden (2007); Kunicová and Rose-Ackerman (2005); Persson et al. (2000, 2003) but note that disagreement exists as to whether open or closed list systems facilitate corrupt behaviour and whether presidential systems incite more corruption. Where Kunicová and Rose-Ackerman (2005) argue that closed systems are more corrupt, whereas Chang and Golden (2007) find this to be true for open list systems. Furthermore Kunicová and Rose-Ackerman (2005) argue that presidential systems are more corrupt, yet Persson et al. (2003) find the opposite.

may facilitate electoral punishment of corrupt officials.¹⁷ Another key factor that might affect the (absence) of punishment of corrupt behaviour is the availability of information. Many studies, as well as international organizations such as the World Bank, IMF and Transparency International, believe that corruption goes un-punished because voters are not accurately informed about corrupt practices. Based on the fact that the majority of the public expresses negative sentiments about corruption in social surveys, the assumption is that if voters *know* about corruption, they will *punish* it.

Various academic studies have found the opposite however: increased levels of information can *decrease* electoral punishment because knowledge of corruption reduces turnout due to a general sense of disengagement in light of corruption allegations.¹⁸ Evidence exists furthermore that voters neglect to punish corrupt politicians *even when* they are fully informed about corrupt practices. An important reason as to why voters may neglect to punish politicians they know to be corrupt is the fact that voters receive side-benefits as a consequence of corruption. These benefits may be material (pork barrel), or they may be immaterial such as when politicians compensate for their illegal affairs by delivering strong performance in another policy domain. The most important example of this is the re-election of corrupt politicians who presided over periods of economic growth.¹⁹

¹⁷Tavits (2007); Schleiter and Voznaya (2012); Davis et al. (2004)

¹⁸See Ferraz and Finan (2008); Winters and Weitz-Shapiro (2013) who find that information increases punishment. Yet, Chong et al. (2012); Davis et al. (2004); McCann and Dominguez (1998); Slomczynski and Shabad (2011); Stockemer et al. (2012) all find that knowledge about corruption decreases participation and may thus hamper the working of the democratic system.

¹⁹See Fernández-Vázquez et al. (2016) who find in a Spanish case study that corruption may be ignored when the incumbent increases the performance of the local economy. Yet Winters and Weitz-Shapiro (2013) find the opposite in Brazil. Research by Zechmeister and Zizumbo-Colunga (2013) shows that presidential approval in light of corruption is conditional on the economic context such that corruption accusations are more damaging during bad economic times. This holds across 19 presidential systems in Latin America Klasnja et al. (2016) shows this dynamic to also be true in Slovakia.

These are all important explanations as to why voters may neglect to punish corrupt politicians. In the experiment, however, I focus on the fact that voters may neglect to punish corrupt politicians *even when* they are fully informed and when they receive *no material benefits* from doing so.²⁰ Rather, in line with the theory set out in Chapter 2, I suggest that the lack punishment may be explained by the trade-off that voters strike between partisan loyalty and performance evaluations.²¹ The experiment thus aims to offer a causal test of the effect of partisan identification on the punishment (or lack thereof) of corrupt candidates.

The idea that group membership in itself matters to the willingness to punish an in-group member is rooted in social identity theory.²² Social identity theory suggests that, from a young age, people perceive themselves and others through categories such as ethnicity, language, religion, gender and - in the case of politics - partisanship. In addition to the psychological need to belong to a group, there are several psychological processes that *follow* from group membership.²³ Most of these processes relate to the positive distinction of the in-group from the out-group and the consequent preferential treatment of the in-group to maintain a positive self-image.²⁴ Maintaining a positive image of the in-group is important in the domain of politics and mostly manifests itself through in-party bias. A clear example of this is the fact that partisans rate the state of the economy more favourably when their in-party is in power and that voters are less likely to attribute responsibility for negative outcomes to their in-party government.²⁵ Other studies have shown that this is also true for the evaluation of a party's performance in other policy domains such as national

²⁰See for a similar argument and empirical evidence of this Rundquist et al. (1977)

²¹See for instance Bernhard et al. (2006); Gino et al. (2009); Wright et al. (2013); Hildreth et al. (2016)

²²Tajfel (1974); Tajfel and Turner (1979); Tajfel (1981, 1984); Turner et al. (1987) For a discussion of social identity theory in relation to political behaviour Huddy and Aaroe (2015); Huddy (2001)

²³See Tajfel (1982, p.7) - emphasis mine

²⁴Turner et al. (1987, p.42)

²⁵Gerber et al. (2010); Marsh and Tilley (2009); Tilley and Hobolt (2011)

security.²⁶

In the experiment presented in this chapter I apply these insights and study the punishment of corrupt candidates both in the *absence* and *presence* of group identification in order to assess the detrimental effects of group identification on the accountability mechanism in light of corruption. The experiment thus provides a controlled environment in which cross-pressure is created and the behaviour of cross-pressured partisans can be studied. As discussed in Chapter 2 and in the conclusion of the previous chapter, Chapter 5, there are different types of group identification. The two main categories are *expressive* and *instrumental* group identification.²⁷ The majority of studies in political science do not explicitly distinguish between the two types of group identification. Yet, existing studies in the field of psychology suggest that it is important to do so because different types of group identification may lead to different types of group behaviour.²⁸ Specifically, instrumental group identification often leads to in-group bias that is focused on competition with the out-group in order to attain certain instrumental, often material, goals. Expressive group identification on the other hand facilitates in-group bias aimed to create and express an in-group identity that is distinct, meaningful and positive in comparison to the out-group.²⁹ In the experiment neither candidates nor participants receive financial benefits from bias toward the in-group. As such, if in-group favoritism occurs on behalf of voters, this should be seen as an example of in-group favoritism on the basis of expressive partisanship.

²⁶Bartels (2002); Gaines et al. (2007)

²⁷Scheepers et al. (2006) For an application of these concepts to partisan identification see the work of Huddy (2001); Huddy and Aaroe (2015)

²⁸An exception to this is Lavine et al. (2012) who do discuss the potential difference in partisan identification of ambivalent and univalent partisans.

²⁹Scheepers et al. (2006, p.363)

6.3 Why Use an Experimental Approach?

As discussed in the introduction to this chapter, there are several reasons to study the response of voters to corrupt politicians through the use of experimental rather than observational data. The use of experimental studies in the field of corruption research has increased and the external validity of such studies has been more firmly established.³⁰ The advantages of the use of an experimental method are the following:

Observability and Measurement: first, the study of corruption in real-world settings is complicated by the problem of observability, or direct measurement.³¹ Because corruption is inherently a secretive, and illegal, act, it is difficult to gather accurate data on its existence. For this reason, the observational literature on corruption relies on proxy measures such as corruption indices that are based either on expert measures, self-reporting by individuals or perceptions of corruption. Not only do these measures introduce measurement error, they also measure corruption in an indirect fashion at best. In the lab experiment, however, corrupt behaviour can be directly observed and measured.

Availability of Information and Biased Information Processing: a second advantage of the lab setting lies with the availability of information to voters. In many real life situations, voters act in a context of non-optimal information. Voters might be unaware of corrupt practices due to a lack of interest in politics or due to the fact that political actors successfully hide this information from the electorate. Even if information is available in the real world, voters may discard this information if they believe that the source of information is biased. This will be the case for instance when the information is provided by the opponent. Moreover, voters might engage

³⁰Abbink (2006); Gino et al. (2009); Armantier and Boly (2008); Englmaier and Gebhardt (2016)

³¹Armantier and Boly (2008); Fearon (1999)

in selective consumption of information and motivated reasoning processes whereby they ignore, re-interpret or simply discard any information that casts their preferred candidate in a negative light.³²

Furthermore, many studies have shown that it can be difficult for voters to accurately attribute responsibility because it is not always clear who is responsible for particular government outcomes. This is true especially in systems where governing power is shared between multiple parties or when political power is spread between a range of different institutions.³³ This may be true for corruption to the extent that it might not be clear to voters which political party or which political officials are involved in various elements of policymaking or which political actors have access to power.

There are thus many ways in which the provision and processing of information on corrupt behaviour may break down in real world settings, resulting in a lack of accountability. In the lab experiment, the provision of information is fully controlled, and voters operate under the condition of full information. Voters are informed on the outcome of the independent corruption audit (reliable source) and are made aware of the performance and the honesty of *both* the incumbent and challenger candidate. The more complete information environment in which participants operate in the lab thus suggests that if participants in the experiment fail to punish corrupt candidates, this should not be attributed to a lack of information, nor are lab participants able to selectively consume only positive information about their in-group candidate.

Updating of Group Membership: a third important advantage of the experimental approach is that it prevents voters in the experiment from updating their

³²See Bartels (2002); Tilley and Hobolt (2011); Gerber et al. (2010); Taber and Lodge (2006)

³³See for discussions on institutional and governmental clarity of responsibility Powell and Whitten (1993); Hobolt et al. (2013)

group membership in response to the event (corruption). As discussed in the previous empirical chapters, this response is highly anticipated in real life settings, but is difficult to control for when using observational data. For instance, we may expect that voters update their partisan identification when they are confronted with negative information about their in-group candidate in order to avoid the experience of cross-pressure.³⁴ This act is a direct response to the event, but results in the fact that repercussions in the next election could remain unobserved due to partisan updating. In the lab experiment, voters are unable to update or alter their group membership and are thus ‘forced’ to engage with their cross-pressured experience.

Social Desirability Bias: another important problem in the observational study of corruption is social desirability bias. Voters are unlikely to admit that they consciously vote for corrupt candidates because such behaviour is generally deemed to be socially undesirable. Yet, we know from empirical reality that there is a discrepancy between what voters say in surveys (object to corruption) and what they do in practice (vote for corrupt candidates). In the lab experiment it is possible to observe behaviour directly, rather than rely on voters’ self-reported preferences and actions.

The Challenger Candidate: the majority of observational studies on corruption and voting behaviour rely on a one-sided account of a voter’s attitudes toward the incumbent. Although much of democratic theory assumes that voters make a rational calculation between the performance of the incumbent and: ‘what the challenger would have done had they been in office’,³⁵ in most studies we only measure a voter’s opinion on incumbent performance and do not take account of the challenger. In the experiment, however, voters are provided with the performance and honesty of *both* the incumbent and the challenger candidate. As such, I can observe voter behaviour

³⁴See for an example in response to Watergate Dunlap and Wisniewski (1978)

³⁵See Downs (1957)

in a situation that more closely approximates the type of decisions voters are thought to make: comparisons between incumbents and challengers.³⁶ What is more, an important difference with the previous chapter on Watergate is that candidates in the experiment cannot retire to avoid blame and escape electoral punishment, nor are there any differences in campaign financing between candidates.³⁷

In-Group Favouritism: finally, the lab experiment does not provide candidates with the option to target voters who belong to their in-group. As such, voters do not receive any type of side-benefits from corrupt candidate behaviour, be they material or immaterial. This precludes an important alternative explanation for the (potential) lack of punishment of corrupt politicians. Other studies on the electoral response to corruption have shown for instance that voters are less likely to punish corrupt behaviour when they receive side-benefits such as material advantages or the implementation of favourable policies.³⁸ Candidates cannot offer any specific benefits to in-group voters in the experiment. As such, voters receive no material benefits from voting for corrupt candidates.

The experimental set-up thus allows for the identification of the effect of in-group and out-group membership on the electoral punishment of corruption whilst ruling out several alternative mechanisms such as information deficiencies, partisan updating, (im-)material trade-offs and strategic candidate behaviour through which the absence of electoral punishment of corruption in real life settings may also be explained.

³⁶This is also true for Chapter 4 of this thesis where information about *both* incumbent and challenger is available to voters.

³⁷Jacobsen (1985); Jacobsen et al. (1986)

³⁸Fernández-Vázquez et al. (2016); Zechmeister and Zizumbo-Colunga (2013); Klasnja et al. (2016)

6.4 Explaining the Lab Experiment

The experiment conducted for this chapter is based on the ‘Attorney General Game’ introduced to the literature by Azfar and Nelson and was conducted in the University of Birmingham Experimental Economic Lab (BEEL) in the summer of 2014.³⁹ Subjects were randomly assigned from the subject pool. The standard procedures of experimental economics were followed, granting full anonymity and confidentiality. In line with common practice in experiments in behavioural economics, participants received payment in proportion to their payoffs in the experiment.⁴⁰

The main purpose of the experiment is to assess the effect of group membership on the electoral punishment of both in- and out-group candidates who act in a corrupt fashion. In order to achieve this, group membership is randomly assigned to lab participants such that there is a control group where no one has any group membership and a treatment group in which participants (both voters and candidates) have a particular group identity. Group identities are constructed using a method that relies on the minimal group paradigm introduced by Tajfel.⁴¹ A key feature of a ‘minimal’ group is the fact that groups are created on the basis of ‘a trivial criterion’ and that group members cannot communicate with each other.⁴² As such the groups that are created are deplete of social meaning and offer a ‘thin’ concept of groupness.⁴³ This method is commonly used in lab experiments that aim to study the effects of group identification and in- and out-group mechanisms.⁴⁴ In order to construct a minimal group identity in this experiment, participants are asked at the beginning of

³⁹Azfar and Nelson (2007)

⁴⁰Levitt (2007)

⁴¹Tajfel (1974)

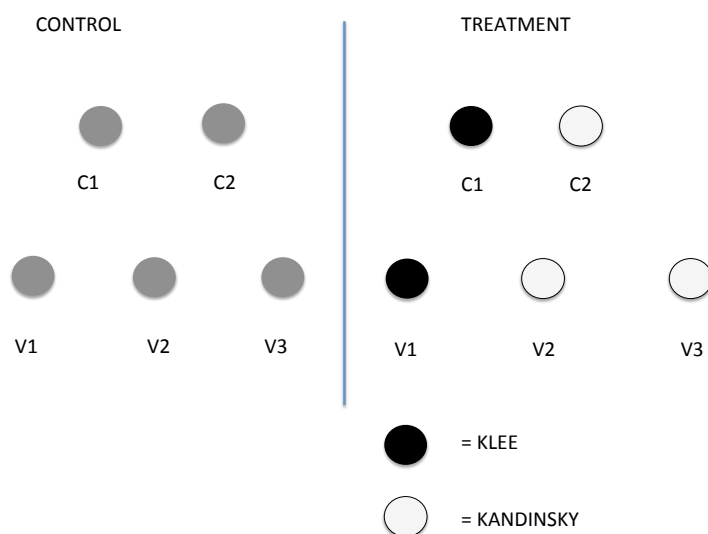
⁴²Scheepers et al. (2006, p.362)

⁴³At the start groupness is expected to have little meaning because the groups are created artificially, but existing research shows that the creation of minimal groups can easily and quickly lead to the development of meaningful group identities and associated group behaviours.

⁴⁴See for a discussion on this topic Lane (2016) For an application of this method see Scheepers et al. (2006)

the experiment to choose between three sets of paintings by two painters: Klee and Kandinsky. Candidates and voters are assigned to either be a ‘Klee’ or a ‘Kandinsky’ on the basis of their painting preferences. These group identities are only revealed in the treatment group. The experiment thus has a control group where participants do not hold any group identification and a treatment group in which participants (both candidates and voters) hold a particular group identity; they are either a KLEE or a KANDINSKY.⁴⁵ An example is provided in Figure 6.2.⁴⁶ Throughout the experiment, voters are informed on the group identity of both candidates. The administration of treatment was randomly assigned across sessions.

Figure 6.2: Example of Voters and Candidates in Control and Treatment Groups

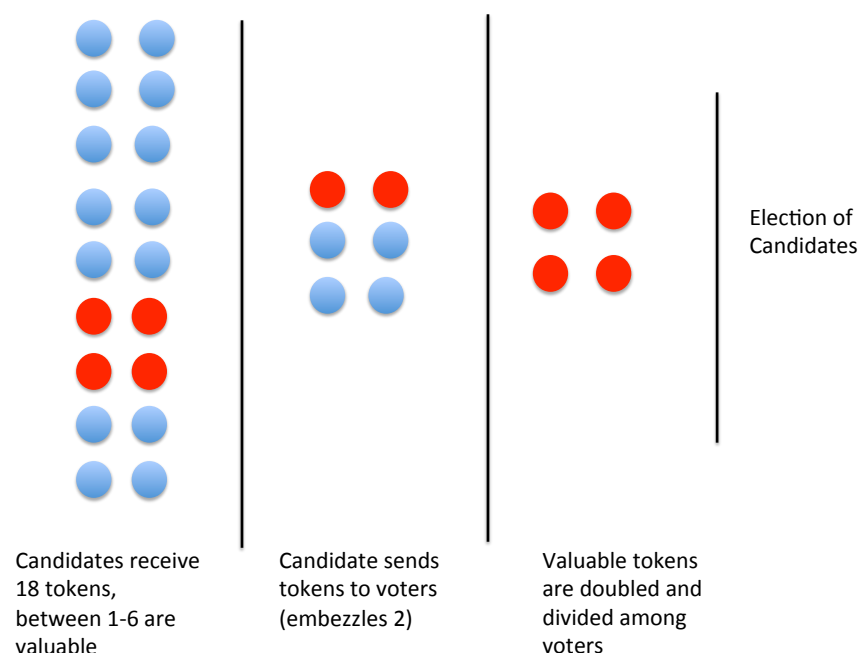


⁴⁵Note that participants in the control group are still asked to select their preferred paintings, but this information is not used when they participate in the experiment.

⁴⁶With regards to Figure 6.2, for the treatment group this example reflects a situation in which the two candidates hold a different identity (1 Klee and 1 Kandinsky) because voters and candidates are randomly assigned to groups, there are also situations in which both candidates hold the same group affiliation.

In the experiment, five subjects (2 candidates and 3 voters) play five rounds of elections. This is repeated across five blocks such that each individual participates in a total of 25 elections. Participants are randomly assigned to either be a candidate ($N = 2$) or a voter ($N = 3$). These roles are fixed throughout the experiment. After each block, voters and candidates are re-assigned to new groups.⁴⁷ In each block, there are five election rounds in which the candidates compete. At the start of every block, each candidate receives 18 tokens. There are two types of tokens, valuable tokens (red) and non-valuable tokens (blue). Valuable tokens can be exchanged for money at the end of the experiment. Between one and six of the tokens are randomly assigned to be red, valuable, tokens. Candidates are asked to send six tokens to voters. These tokens may take any colour the candidate wishes. An example is provided in Figure 6.3.

Figure 6.3: Distribution Valuable Tokens Candidates to Voters



⁴⁷Yet note that someone who started as a voter always remains a voter. The same holds for participants who were assigned to be a candidate.

Candidates may decide themselves how many valuable (red) tokens they send to voters and how many they keep to themselves (embezzle). Valuable (red) tokens benefit voters because they are doubled and divided equally among them. Group welfare is thus maximized when candidates send all valuable tokens they have received to voters, while candidates maximize their own welfare by keeping valuable tokens. This automatically decreases the wellbeing of the group. As such, it is not possible for candidates to selectively favour their in-group, and candidates are unable to offer (im-)material trade-offs to in-group voters. After each round, voters elect one of the two candidates. A candidate wins the election by majority vote.

At the end of each round, the candidate who wins the election receives a valuable token. Each round is independent such that tokens cannot be carried over from one round to the next. The value, i.e. the colour, of the tokens that the candidate keeps are unknown to voters, apart from the fact that the computer randomly reveals the colour of two tokens that the candidate kept back. The revelation of the tokens functions like a corruption audit. The audit thus informs voters whether the candidate has kept back any valuable tokens. If the computer finds one or more valuable red tokens, then all other valuable red tokens kept by the candidate will become worthless and voters will be informed about the misconduct. Similar to a real life situation, there is thus a risk for candidates to engage in corrupt behaviour.

6.4.1 Information Provided to Participants

All participants are informed of the basic setup of the experiment. Participants are told there will be five blocks of five rounds of elections and are told that two out of five group members will be candidates, whereas the others are voters. In the treatment group, voters are informed about the identity of the candidates, KLEE

or KANDINSKY. Everyone is told that both candidates receive 18 tokens in every round, with between one and six valuable tokens. All participants are informed about the occurrence of the random audit. Participants are provided with the following information with regards to their pay-offs:

- everybody will receive a show-up fee of £2.50
- the exchange rate is 4 valuable tokens = £1
- if you are a candidate: you will receive a fixed amount of two valuable red tokens every round and one extra valuable red token if you are the winner in that given round. In addition, you can keep the number of valuable red tokens that you decide not to send to voters, if none of them are revealed in the audit
- if you are a voter: the amount you receive per round depends on the number of valuable red tokens sent by the candidate who won that round. The number of valuable red tokens sent by the candidate will be doubled and divided equally among voters

At the end of every round, candidates are informed whether or not they have won the round and how many valuable tokens the computer found. Voters are informed on the candidate that won the round, the number of valuable tokens provided by the candidate - as well as the number they would have received if the other candidate had won. Voters are also informed on the outcome of the random audit for both candidates. ⁴⁸

From a theoretical point of view, it is important to note that the creation of group identities on the basis of a minimal group offers a conservative test of the effect of

⁴⁸A copy of the full instructions provided to participants is provided in the Appendix.

group identities on voting behaviour and the punishment (or reward) of candidate behaviour. This is intentional. The group identities are devoid of any political content and, by design, should not have any ‘actual’ meaning to participants. The experiment thus explicitly avoids using political group identifications such as ‘blue’ versus ‘red’ or more specifically ‘Democrat’ versus ‘Republican’, because such group identifiers have actual meaning outside the lab setting. Using labels that are meaningful outside of the lab setting could introduce biases and assumptions with participants that cannot be controlled for but might still affect participants’ behaviour. If the experiment shows effects of group identification when using a minimal group method, then these effects are thought to be even stronger in the real world when group identities are rooted in meaningful and long-established differences between groups.

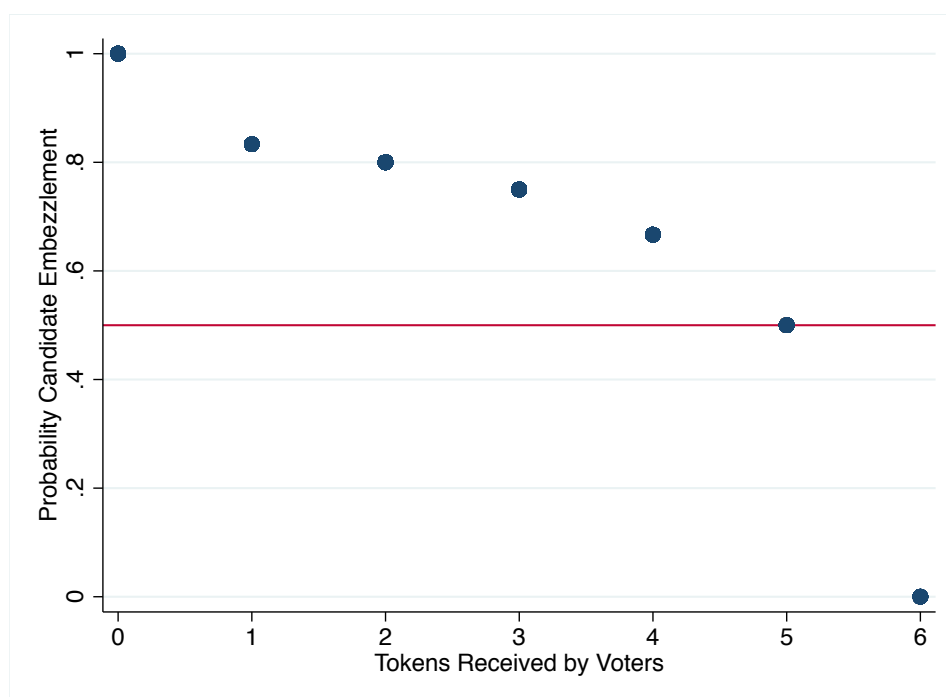
6.4.2 Information on Corrupt Behaviour of Candidates

There are two ways in which voters might learn of the corrupt behaviour of candidates. The first is through the random audit that is performed by the computer. For each candidate, two tokens that the candidates kept back are revealed. The random audit informs voters about the colour of two tokens held back by the candidates. The computer audit thus directly conveys corruption information to voters. Yet, voters may also infer indirectly whether or not candidates have held back valuable tokens. They may do this through the number of valuable tokens they receive from candidates.

As mentioned, candidates received between one and six valuable tokens at the start of a round. Thus, if a voter receives no valuable tokens at all from the candidate, they may infer that the candidate has embezzled tokens. Vice versa, if a voter receives six valuable tokens, they know the candidate has not kept any valuable tokens to themselves and hence is ‘clean’. It is more difficult to infer whether or not a candi-

date embezzled funds when a voter receives between one and five tokens. To give an example: if a voter receives five valuable tokens from a candidate, this means that the candidate received either five or six valuable tokens at the beginning of the round. The candidate may have received five valuable tokens and sent all tokens to voters. Or, if the candidate received six valuable tokens, the candidate kept one valuable token to themselves. In this example, there are two possible scenarios: the candidate either received five or six tokens and only in one of those did the candidate embezzle funds. The probability of embezzlement in this case is thus 0.5. The probability of candidate honesty based on the number of tokens voters receive is displayed in Figure 6.4.⁴⁹

Figure 6.4: Probability of Candidate Embezzlement by Number of Tokens Voters Receive



⁴⁹I'm grateful to Hector Solaz for pointing this out to me.

This embezzlement information is important because it affects how useful the corruption audit is to voters. If the probability of embezzlement is 1 (a voter receives no valuable tokens) or 0 (a voter receives six valuable tokens), then the audit adds little extra information. Voters are able to deduce whether or not the candidates are honest on the basis of the tokens they receive. Yet, if a voter receives between one and five tokens, the tokens received are less informative and the audit should be more important to voters. For example, if a voter receives five tokens, the probability of embezzlement is 0.5 (see Figure 6.4) and thus the corruption audit is expected to be highly informative to voters. To capture the association between the numbers of tokens voters receive and the information this reveals, I create a variable called ‘Embezzlement Information.’

The operationalisation of this variable is displayed in Table 6.2. The embezzlement information variable reflects how informative the tokens received by the voter are. High values indicate that the number of tokens received convey helpful information to voters as to whether or not the candidate embezzled tokens. To construct the variable, I first calculate the probability that the candidate embezzled funds on the basis of the tokens the voter receives (Figure 6.4 and Table 6.2 column 1). Then, I take the absolute distance between the probability of embezzlement and 50%, or chance. Thus, if the tokens reveal a large amount of information to voters (i.e. voter receives zero or six tokens), the embezzlement variable takes the highest value of 0.5. If the voter cannot really infer a candidate’s honesty on the basis of the tokens received (i.e. voter receives 5 valuable tokens), then the embezzlement variable takes the value of 0.

Table 6.2: Embezzlement Information Variable

Tokens Received	Probability Candidate has Embezzled Tokens	Embezzlement Information Variable
0	1	0.5
1	0.84	0.33
2	0.8	0.3
3	0.75	0.25
4	0.67	0.16
5	0.5	0
6	0	0.5

6.5 Results

The experiment simulates 25 election rounds with two competing candidates and three voters in each election. In the control group, none of the participants hold any particular group identification. In the treatment group, both voters and candidates are either a KLEE or a KANDINSKY. Voters may face one KLEE and one KANDINSKY in their election, but can also face two candidates of the same identity. The candidates are offered the possibility to cheat by holding back valuable tokens to benefit themselves. Voters are informed about the level of honesty of the two candidates through both the number of valuable tokens they receive as well as the random corruption audit that is conducted. The analyses presented below aim to establish whether voters are *less* likely to punish corrupt candidates when group identification is introduced in the treatment group.

The data is structured in a matrix format such that for each election an individual voter has two lines of data, one for candidate 1 and one for candidate 2. This data structure is used in various studies that aim to explore the interaction between a voter's decisions and candidate characteristics.⁵⁰ The results presented below are robust to alternative modeling strategies.⁵¹ The main variables in the analyses below

⁵⁰Alvarez and Nagler (1997, 1998b)

⁵¹Please refer to the Appendix, Tables A.30 and A.32 for models using a linear probability mod-

are the following:

- **Caught:** this variable takes the value of 0 or 1. If the variable takes the value of 1 then the candidate was caught holding back valuable tokens back in the corruption audit.
- **Embezzlement Information:** ranges between 0 and 0.5, takes the value of 0 if the number of tokens received conveys no extra information as to whether or not a candidate is corrupt, and takes the value of 0.5 when the number of tokens conveys extra information as to the corruption of the candidate (see explanation Table 6.2)
- **Clean for Sure:** this takes the value of 1 if a voter can be entirely sure that the candidate did not embezzle any valuable tokens (when a voter receives six valuable tokens)

Participants were also asked about a range of background variables such as their gender, age, knowledge of politics and their level of risk aversion.⁵² The results presented in this chapter are robust to the inclusion of these factors into the analysis.⁵³

6.5.1 Punishment of Corrupt Candidates

I present binary probit regression models to analyze the data generated by the experiment.⁵⁴ The dependent variable is a vote for the candidate. I include individual random effects to control for the fact that there are repeated observations for individual voters. I include fixed effects for rounds and blocks, which controls for potential

elling approach.

⁵²See the Appendix, Modules 2 and 4 of the participant questionnaire.

⁵³Please refer to the Appendix, Table A.29 and Table A.31 for these analyses. The Appendix also includes an overview of the descriptive statistics of all variables used in the analyses presented below in Table A.28.

⁵⁴See for a discussion of this Alvarez and Nagler (1998a)

learning effects over the course of the experiment. The results are robust against other model specifications such as the inclusion of individual level covariates (Appendix Table A.29) and a linear probability specification (Appendix Table A.30).

I first present the results of the response of voters to corruption information without taking group membership into account. The experimental results below in Table 6.3 suggest that participants in the experiment *punished corruption*. The probability of voting for a candidate significantly *decreases* when the candidate is caught in a corruption audit (Model 1, coefficient -0.129) or when the probability of embezzlement increases (Model 2, coefficient -3.052). Both types of information on candidate corruption thus decrease the likelihood of a vote for the corrupt candidate. Models 3 and 4 include both types of information on corruption and their interaction. Being caught in a corruption audit is no longer significant when embezzlement information is added (Model 3), but the interaction between the two measures is significant (Model 4, coefficient 1.087). This suggests that - as expected - the effect of being caught in an audit is conditional on the information voters already inferred from the tokens they received.

Table 6.3: Experimental Results: The Effects of Corruption Information on Vote Choice

	Model 1	Model 2	Model 3	Model 4
Caught	−0.129** (0.061)		−0.077 (0.063)	−0.430** (0.185)
Embezzlement information		−3.052*** (0.229)	−3.042*** (0.229)	−3.302*** (0.265)
Caught * Embezzlement				1.087* (0.536)
Clean for sure:		1.390*** (0.233)	1.368*** (0.234)	1.416*** (0.235)
Constant	0.031 (0.074)	0.978*** (0.105)	0.994*** (0.106)	1.079*** (0.115)
Fixed Effects	Yes	Yes	Yes	Yes
N	2,368	2,368	2,368	2,368
Number of Clusters	60	60	60	60

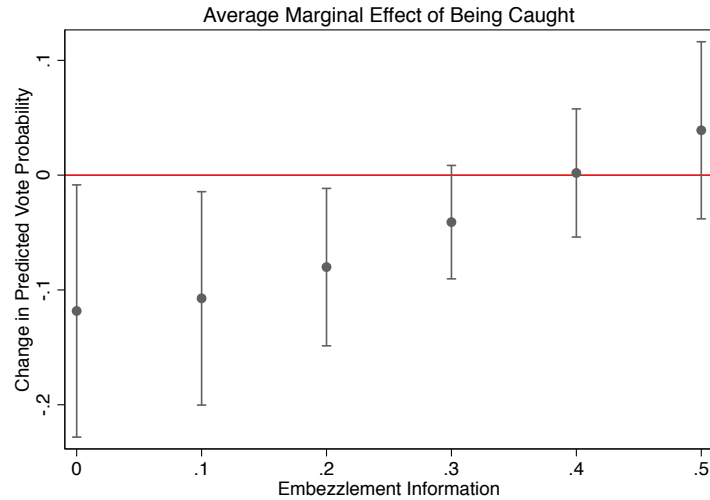
***p < .01; **p < .05; *p < .10

Coefficients are panel probit regression coefficients

Fixed effects for rounds and blocks, standard errors clustered at individual level

Figure 6.5 provides a visual representation of the interaction effect between the corruption audit and the embezzlement information. Figure 6.5 displays the marginal effect of being caught on the probability of voting for a candidate across different probability levels of embezzlement. The figure displays 95% confidence intervals, and all other variables are set to their mean level. As expected, the effect of being caught in an audit significantly reduces the predicted probability of voting for a candidate when voters cannot clearly infer if a candidate embezzled funds based on the number of tokens received (left-side of plot). Substantively, when a voter learns that a candidate is corrupt, the likelihood of voting for this candidate decreases by 10-20%. As the probability of embezzlement increases, the average marginal effect of being caught in a corruption audit on the punishment of a candidate is no longer significant. This suggests that voters in the experiment paid close attention to both the number of tokens they received and the outcome of the corruption audit and were able to accurately value the information provided by the audit. Thus, in the absence of group identities, voters punished candidates that they knew to be corrupt.

Figure 6.5: Effect of Being Caught in Corruption Audit on Predicted Vote Choice as Embezzlement Information Increases



6.5.2 The Effect of Group Identification on the Electoral Punishment of Corruption

Following from this, the main question is now what the effect is of the introduction of group affiliations in the treatment group. These group affiliations simulate the effect of partisan identification on the punishment of in-party candidates who are corrupt. I thus re-run the analyses presented above using the treatment group and split the sample into voters who faced in-group candidates and those who faced out-group candidates. The results are displayed below, the Appendix again offers robustness tests including individual covariates (Table A.31) and a linear probability model (Table A.32). The findings presented here are robust to these alternative specifications.

Table 6.4 suggests that voters are significantly *less* likely to vote for an out-party candidate when this candidate is found to be corrupt (-0.84 , $p < 0.03$), but that there is no significant effect of being caught in a corruption audit for in-party candidates. The most interesting term in the two models is again the interaction between a candidate being caught and the embezzlement information variable since this reflects how

voters respond to *both* pieces of corruption information. Table 6.4 suggests that the interaction term is significant when voters face candidates who belong to the out-group, but that the interaction term is insignificant when voters face candidates who belong to their in-group.

Table 6.4: Exerimental Results: The Effects of Corruption Information on Vote Choice Including Group Membership

	Out-Group Candidate	In-Group Candidate
Caught	−0.84** (0.39)	−0.16 (0.39)
Embezzlement information	−3.54*** (0.53)	−3.39*** (0.55)
Caught * Embezzlement	1.82* (1.05)	0.28 (1.07)
Clean for sure	-	0.28
	-	(0.92)
Constant	1.33*** (0.26)	1.17*** (0.24)
Fixed Effects	yes	yes
N	584	583
Number of clusters	30	30

*** p < .01; ** p < .05; * p < .10

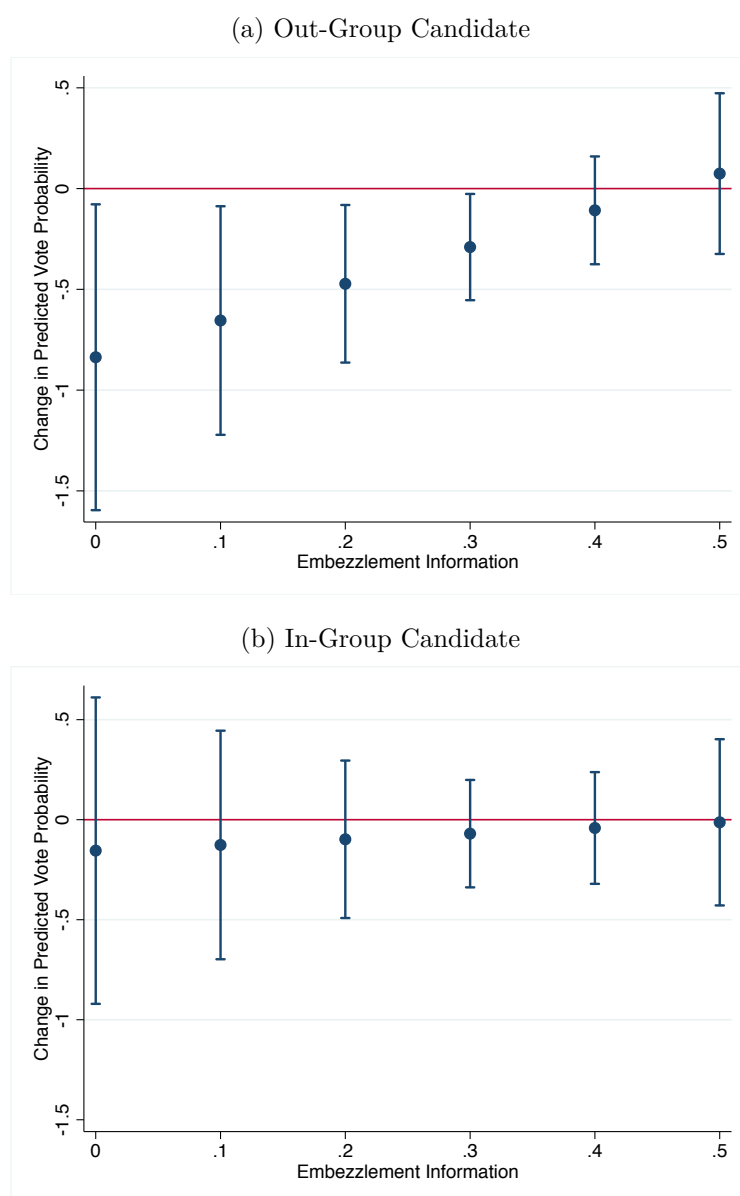
Coefficients are panel probit regression coefficients

Fixed effects for rounds and blocks, standard errors clustered at the individual level

In order to interpret the interaction term, I again plot the marginal effects of a candidate being caught in an audit on the predicted probability of voting for a candidate across different probabilities of embezzlement, but differentiate between voters who face an out-group candidate (top) and those who face an in-group candidate (bottom).⁵⁵ This is shown in Figure 6.6.

⁵⁵Note that given that the group identity of candidates that voters face is randomized, voters have a 50-50 chance to face either an in- or out-group candidate

Figure 6.6: Effect of Being Caught in Corruption Audit on Predicted Vote Choice as Embezzlement Information Increases When Voters Face In- and Out-Group Candidates



The average marginal effects suggest that when an out-group candidate is caught in the corruption audit, the probability of voting for this candidate *declines*. This is similar to the results in Figure 6.5 when no group identification is present, but the substantive effects are larger when voters face out-group candidates. Importantly, for in-group candidates who are caught embezzling funds (Figure 6.6b), I find that information about corruption does *not* significantly affect the likelihood of voting for this candidate. From this I conclude that corruption information thus matters when no group membership information is provided and *reduces* the likelihood of voting for candidates under these conditions (Figure 6.5). Yet, once groups are created, the likelihood of voting for a corrupt out-party candidate *decreases*, but the likelihood of voting for a corrupt in-party candidate is *not affected by information on corruption*. Negative information about a candidate thus only matters when this candidate does not share a voter's group identity. When a candidate belongs to the same group as a voter, voters in the experiment *ignore* the fact that the candidate has held back valuable tokens.

6.6 Discussion and Next Steps

This chapter has provided an experimental test of the interaction between group loyalty and the punishment of corrupt behaviour of in-group and out-group candidates. The experiment suggests that even the introduction of non-political group identities compromises the accountability mechanism. When no group identities were present, voters in the experiment responded accurately to information about corruption and were less likely to vote for candidates they suspected, or knew, to be corrupt. Yet, once group identities were introduced (on the basis of preferences for a particular painter), the behaviour of voters changed. Voters were still responsive to information

about corruption of the out-party candidate, but neglected to punish candidates with whom they shared a group identity when they received information that this candidate withheld valuable tokens from them.

These results are somewhat surprising due to the low importance of the group identities that were used in the lab. Other studies in which group membership is introduced in a lab-setting confirm however that the behaviour of lab participants changes once even these minimal groups are introduced.⁵⁶ With regard to the study of corruption, the lab experiment suggests that voters may fail to punish corruption even when they have the required information to do so and when no material or immaterial trade-offs are provided. These results reject Hypothesis 5 and suggest instead that cross-pressured partisan neglect to punish in-group candidates for the embezzlement of funds.

These findings contradict some of the findings reported in the previous empirical chapters. In fact, in the previous chapters I found that partisan voters were quite receptive to negative performance signals about their preferred party or candidate. This was true especially when voters were presented with information about the economic performance of the President (Chapter 3), but also when voters were informed about the competence of local candidates (Chapter 4), or when faced with a large scale political scandal (Chapter 5). Yet, across these empirical case studies I also found that individual level characteristics, such as strength of partisan identification, as well as institutional context in the form of levels of polarisation and electoral competitiveness can inhibit electoral punishment of poor performance. In line with these findings, the lab experiment presented here offers convincing evidence to suggest that group identification is an important limiting factor to the punishment of candidate corrup-

⁵⁶Landa and Duell (2015); Scheepers et al. (2006)

tion. Specifically, in the lab experiment group identification is expressive in nature: it does not reflect ideological congruence between candidates and voters, nor does group membership deliver any material gains to voters. The experiment thus suggests that expressive group membership may be especially detrimental to democratic accountability. As I will also discuss in the next chapter (Chapter 7 - Discussion), further research could explore whether different types of partisan identification (expressive versus experimental) might lead to different behavioural responses of voters.⁵⁷ This could be an explanation for the fact that voters are more responsive to economic mismanagement in comparison to corruption. In the next chapter I summarise the main findings of this thesis and discuss how the findings of the four empirical case studies relate to each other and how they inform the central research question of the thesis.

Table 6.5: Evidence for Hypotheses tested in Chapter 6

Hypothesis	Evidence
5. Cross-Pressured Partisans are <i>less</i> likely to vote for the in-party compared to non cross-pressured partisans	X

⁵⁷This is suggested by Scheepers et al. (2006)

Chapter 7

Conclusion and Discussion

Living our lives through a lens

Trapped in our white picket fence (...)

So comfortable, we're living in a bubble, bubble, bubble

So comfortable, we cannot see the trouble, trouble, trouble

- Katy Perry 2017, *Chained to the Rhythm* -

A key expectation of elections is that they foster accountability between voters and politicians because they present voters with the opportunity to either continue or discontinue the power transfer to the incumbent government. This accountability mechanism is captured in the ‘retrospective vote paradigm’ which suggests that voters use information about incumbent performance to either punish or reward the incumbent for its performance whilst in office.¹ As has been discussed in the Theory chapter of this thesis (Chapter 2), a contradiction exists in the literature as to whether or not this mechanism actually performs in line with expectations. On the one hand, studies on the economic vote as well as studies on the electoral response to natural disasters offer empirical evidence of this mechanism in practice.² Yet, other

¹The mechanism of punishment and reward is the most commonly used interpretation of the retrospective vote paradigm, but please see Chapter 2, section 2.1 for a discussion of the other pathways through which retrospective voting may foster accountability and representation.

²Duch and Stevenson (2008); Nadeau et al. (2012); Powell and Whitten (1993); Lewis-Beck (1980, 1988); Lewis-Beck and Paldam (2000); Lewis-Beck et al. (2008); Lewis-Beck and Stegmaier (2013);

studies suggest that voters often fail to accurately exercise accountability toward the incumbent either due to the fact that it is difficult for voters to develop retrospective judgments, or because voters are hampered by their partisan loyalties to do so.³ In light of this contradiction, this thesis has sought to explore the conditions under which partisans hold their in-party to account in light of contradictory signals about its performance.

It is important to understand the conditions under which the accountability mechanism works, or fails to do so, for two reasons. First, the accountability mechanism is thought to facilitate representative and high quality governance. If voters do not actually hold politicians to account for their actions whilst in office, then the quality of democratic representation and governance cannot be guaranteed. Second, the concept of democratic accountability is one of the two main pillars underlying the majority of scholarship on democratic governance.⁴ If elections do not actually establish a relationship of accountability between voters and politicians, then our current understanding of democracy is incorrect and must be reconceptualised.⁵

This thesis has presented four case studies in which I have tested the accountability mechanism of elections in the US context. In these case studies I focused on ‘cross-pressured’ partisans. These are voters who identify with one of the two major parties (Democrat or Republican), but who receive a quality signal about the performance of the incumbent that *contradicts* this partisan affiliation. I have argued that the study of cross-pressured partisans is a helpful tool to explore the conditions under

Chzhzen et al. (2014); Bermeo and Bartels (2014); Bechtel and Hainmueller (2011); Malhotra and Kuo (2008); Healy and Malhotra (2009)

³Bartels (1996, 2000, 2002); Bartels and Achen (2004); Achen and Bartels (2013, 2016); Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013); Huber et al. (2012); Gaines et al. (2007); Taber and Lodge (2006)

⁴Przeworski et al. (1999)

⁵This is the argument made by Achen and Bartels (2016) in light of their criticisms on the retrospective vote model and consequent democratic accountability.

which voters exercise accountability and when they fail to do so. This is the case because cross-pressured partisans are faced with a direct conflict between their desire for partisan representation and their requirement to uphold the quality of democratic government and exercise accountability. Put simply, I assume that the majority of voters will want to elect politicians who both represent their views *and* are competent, honest and effective office holders. For a large part of the electorate these two motivations *align*. This alignment occurs either because these voters are presented with choice options that satisfy these two desires or because voters' partisan biases affect their perception of the quality and competence of the choice options. Yet, such a scenario of *alignment* does not shed any light on whether or not voters are responsive to performance information, rather it suggests that only partisan loyalty matters to elections. As such, I have presented four case studies in this thesis that focus on scenarios in which partisans face a *conflict* between their partisan attachment and information they receive about incumbent performance. These case studies have allowed me to explore which voters are receptive to contradictory information signals, and hence experience cross-pressure, how these voters behave at election time and which individual and contextual level factors affect this process.

In this Discussion chapter I first review the main findings of the empirical case studies in section 7.1. Because all empirical chapters already include a summary of the results, I only discuss the most important empirical findings to leave more space for the discussion of these findings and suggestions for further research in sections 7.2 and 7.3.

7.1 Summary of the Main Findings

This thesis has presented four empirical case studies in which I have tested a framework of cross-pressured voting that set out to explore the conditions under which partisans hold their in-party to account and when they fail to do so. Across the case studies I focused on different *types* of cross-pressure. Thus, in Chapter 3, I focused on cross-pressure on the basis of the economic performance of the incumbent. In Chapter 4, I explored cross-pressure rooted in the competence of the incumbent and challenger candidate in midterm elections. In Chapters 5 and 6 I focused on corruption allegations. Across the chapters I have used observational, quasi-experimental and experimental data.

7.1.1 Chapter 3: Economic Cross-Pressure in the 1992, 2000 and 2004 Presidential Elections

In the first empirical case study I tested my framework of cross-pressured voting in the domain that has been studied most extensively in the field of retrospective voting: the performance of the national economy. In the chapter I explored whether partisans punished their in-party and rewarded the out-party in light of the President's management of the national economy across three presidential elections (1992, 2000 and 2004). In line with existing literature on the economic vote, I found that partisans are responsive to signals about the president's ability to manage the national economy and that this information affects voters' behaviour at election time.⁶

First, in the chapter I have shown that there is a large segment of the partisan electorate that reports evaluations of the president's management of the national economy

⁶This is in line with studies such as Duch and Stevenson (2008); Nadeau et al. (2012); Powell and Whitten (1993); Lewis-Beck (1980, 1988); Lewis-Beck and Paldam (2000); Lewis-Beck et al. (2008); Lewis-Beck and Stegmaier (2013); Chzhen et al. (2014); Bermeo and Bartels (2014)

that *contradict* their own partisan loyalties. Importantly, voters' retrospective economic evaluations are in line with the economic reality. Thus, for the 1992 presidential election I found that 57% of Republican partisans were of the opinion that Republican President George H.W. Bush did a poor job of handling the economy. Similarly, in 2000, 59% of Republican partisans provided a positive evaluation of incumbent Democratic incumbent President Bill Clinton. The number of cross-pressured partisans was lower in 2004 when the economic signal was less clear. These findings confirm the expectation that the number of cross-pressured partisans is *higher* when the economic signal is either clearly positive or clearly negative (H3).⁷

After having established that a sizeable proportion of the partisan electorate experiences cross-pressure, I examined whether the likelihood that a partisan voter offers a contradictory evaluation of the president's handling of the economy is affected by either the strength of the voter's partisan affiliation or their level of political knowledge. I found no evidence to suggest that political knowledge affects partisans' responsiveness to economic signals (H2). I did find that the strength of partisan affiliation has an effect, but only on *negative* information signals about the incumbent President (H1). Thus, partisan loyalty offers a strong barrier to negative information about the in-party, but does not appear to affect the reception of *positive* information about the out-party. In the final section of Chapter 3, I explored the behaviour of these cross-pressured partisans at election time. I found that, in comparison to non cross-pressured partisans, cross-pressured partisans are no more likely to abstain, but are more likely to vote for the out-party in response to the experience of cross-pressure. This confirms both behavioural hypotheses of the theory of cross-pressured voting developed in this thesis (H4, H5).

⁷This confirms the findings of Dickerson (2016); Parker-Stephen (2013)

In general, the findings in Chapter 3 are encouraging from the perspective of the theory of retrospective voting. At least in the 1992, 2000 and 2004 presidential elections, partisans were responsive to information about the economic performance of the president and were able to overcome partisan biases in response to clearly positive and clearly negative signals about the state of the national economy. What is more, once partisans experienced this type of cross-pressure, they responded to this information at election time. Compared to non cross-pressured partisans, cross-pressured partisans were significantly less likely to vote for the in-party across the elections under study and more likely to alter their voting behaviour over time.

7.1.2 Chapter 4: Candidate Cross-Pressure in the 2006 Midterm Elections

Although the study of economic cross-pressure thus offers positive evidence of a responsive electorate, several important elements of voting were not discussed in Chapter 3. Thus, in Chapter 4, I set out to explore whether the electoral context in which cross-pressured voters find themselves affects their behaviour at election time. In order to test this, I focused on midterm elections for the US House of Representatives. I used data on the 2006 elections which includes both voter and expert measures of candidate and incumbent competence. In the chapter I conceptualize cross-pressure as a contradiction between a voter's partisan loyalty (toward either incumbent or challenger) and the evaluation of the competence of both candidates. If a voter identifies with the party of the incumbent, but the challenger candidate is thought to be more competent, a voter is considered cross-pressured. The reverse is true for a voter who identifies with the party of the challenger candidate.

In keeping with Chapter 3, I again found that there is a substantive group of cross-pressured partisans, that these voters are no more likely to abstain and that cross-

pressured partisans are significantly less likely to vote for the in-party (H4, H5). It must be noted that the number of cross-pressured partisans was much higher when using expert evaluations of competence as compared to voter evaluations. This reflects the well-known effects of partisan bias on candidate evaluations.⁸ The effects of cross-pressure on vote choice are robust to using either of the two measures, but were much more substantive when using the voter measure of cross-pressure. I also tested whether the behaviour of cross-pressured partisans is affected by the electoral context in which they vote (H6, H7). I found that this is the case. Specifically, I found that the behavioural response of cross-pressured partisans is moderated by the level of ideological polarisation between candidates and the competitiveness of the election. Put simply, if the partisan stakes of an election are *high*, democratic accountability is *low*. Although partisans are thus receptive to competence signals in general, this response is moderated by the political environment in which an election takes place. Elections in which candidates are highly polarised along the ideological spectrum or elections that are highly competitive are unlikely to foster an accountability response from cross-pressured partisans.

7.1.3 Chapter 5: Electoral Responsiveness to the Watergate Scandal

In the third empirical chapter, Chapter 5, I used the Watergate scandal as a case study. The Watergate scandal has the advantage that it was a highly salient event in which voters received a clearly negative signal about the performance of the incumbent President, Richard Nixon. In the chapter I explored whether Republican partisans punished the Republican party in the 1974 midterm elections for its involvement in Watergate. I conducted both an individual-level analysis using observational data and a district-level analysis that was set up according to a difference in difference design.

⁸Evans and Andersen (2006); Evans and Pickup (2010); Bartels (2002)

The individual level analysis suggested that the electorate altered its opinion of President Nixon in the wake of Watergate. This was true even for Republican partisans with strong levels of attachment to the party. Yet, the behavioural response varied across Republican partisans. Republican partisans with weak or moderate levels of affiliation to the party were most likely to abandon the Republican party between the 1972 and 1974 midterm elections and voted for the Democratic party instead. This again suggests that under certain conditions, such as a large-scale political scandal, partisan voters do hold their own party to account for performance failures (H5).

The district level analysis however did not offer any confirmatory evidence to suggest that Republican partisans targeted their response to the members of the Judiciary Committee on Impeachment. Individual House representatives were neither rewarded nor punished for the position they took with respect to President Nixon's impeachment. Rather, the electorate punished the Republican party across the board. The lack of a response to the individual members of Congress might be explained by the fact that President Nixon resigned prior to the 1974 elections and hence effectively diverted blame from loyalist representatives.⁹ Another option is that voters simply punished all Republican representatives because they assumed that most politicians are corrupt, or supportive of corrupt behaviour. The fact that several House representatives actively sought punishment of their own president did not alter this view among voters. Finally, the low levels of campaign funding for Republican candidates and the high levels of campaign support for Democratic challengers likely harmed all Republican candidates, regardless of their position on the impeachment of President Nixon.¹⁰

It is important to note that the Watergate scandal appears to have increased absten-

⁹See for a study on strategic retirement Stone et al. (2010)

¹⁰Jacobsen (1985); Jacobsen et al. (1986)

tion rates, specifically among Republican partisans with moderate levels of identification with the party. This confirms other studies that have found that highly visible instances of corruption or increased levels of knowledge about corrupt practices have a detrimental effect on democratic participation.¹¹ Importantly, from the perspective of normative democratic theory, increased levels of abstention may counteract the positive effect of vote-switching in light of a political scandal. This could explain why there are many examples in which corrupt politicians retain office. If core partisans, those with strong affiliations, continue to vote for a corrupt incumbent, but partisans with weaker levels of affiliation neglect to vote, then the incumbent may retain her position. I did not find an effect of cross-pressure on abstention rates in Chapters 3 or 4 in response to economic and candidate competence cross-pressure. This therefore raises the question whether different types of government performance incite different electoral responses. Studies in the field of psychology would suggest this is the case.¹² I discuss this in more detail in section 7.2 below.

7.1.4 Chapter 6: Lab Experiment on Corruption

In the final empirical chapter I presented a lab experiment that explored whether voters punished candidates who embezzled funds. The treatment that was administered in the experiment was expressive group identification. In the experiment I tested the accountability mechanism both in the absence and presence of group identity. In light of the findings of Chapter 5 on Watergate, the lab experiment was able to rule out various explanations of the lack of electoral punishment of corruption. First, the lab experiment ensured that voters acted in a context of full information. Lack of punishment of corrupt candidates could therefore not be explained by voter ignorance. Second, due to the controlled environment in which the experiment took place, it was

¹¹Chong et al. (2012); Davis et al. (2004); McCann and Dominguez (1998); Slomczynski and Shabad (2011); Stockemer et al. (2012)

¹²For an overview see Scheepers et al. (2006)

impossible for candidates to offer material or immaterial trades to voters. This eliminated the possibility that voters neglected to punish corruption because they received material gains. Furthermore, the group identities that were constructed in the lab setting did not reflect any type of ideological viewpoints which suggests that voters were unlikely to allow corruption in exchange for particular policy implementations. Finally, there were no differences in financing between candidates and candidates were unable to strategically withdraw (retire) from politics to avoid being punished for corrupt behaviour.

The results from the lab experiment suggest that voters punished corrupt candidates when no group identities were present, but neglected to do so once group membership was introduced. Specifically, voters punished out-party candidates for corrupt behaviour, but neglected to punish in-group candidates when they received information that the in-group candidate was corrupt. The lab experiment hereby provided clear evidence of the fact that partisan identities are a major barrier to democratic accountability. In fact it is surprising that lab participants responded so strongly to group loyalties that were constructed in a lab setting. It is important to note that lab participants received no benefits from turning a blind eye to corruption, rather, they failed to maximize their own pay-offs by supporting corrupt in-group candidates. The group identities that were created in the lab reflect an expressive party attachment as opposed to an instrumental attachment, suggesting that partisan loyalty rooted in social identification might be especially damaging to democratic accountability. Question remains whether the results of the lab experiment would have been different if instrumental group identities were created. I discuss this further in section 7.3

7.1.5 Summing Up

The central research question of this thesis has been:

under what conditions (if any) do partisans hold their in-party accountable when faced with negative performance signals - and what form does this response take?

Based on the empirical analyses presented, I conclude that partisans hold their in-party to account for mismanagement of the national economy as well as for a lack of competence in local elections and in the wake of a large-scale scandal. In all these scenarios punishment takes the form of a vote for the opposing party. This suggests that there are situations in which partisans overcome their biased response and vote against their partisan preferences.

Yet, the empirical analyses also reveal important heterogeneity among the electorate and across performance domains. The four case studies show that the electorate response is moderated by three main factors: (1) individual level characteristics of voters; (2) contextual factors of the election and; (3) the type of performance that voters evaluate. Put simply, partisans are *less likely* to exercise accountability when their affiliation to the party is very strong or when the partisan stakes of an election are high. Furthermore, although I find that voters are responsive to information signals about the national economy and candidate competence in local races, I find that the electoral response to corruption is more mixed. Some partisans are dissuaded from voting in response to the Watergate scandal and lab participants neglect to punish corrupt candidates who belong to their in-group. This suggests that partisan-performance cross-pressure does not always foster democratic accountability. Voters are not incapable of holding their in-party to account, but neither should they be expected to always do so.

7.2 Implications for Our Understanding of Democratic Accountability

In this thesis I have highlighted a contradiction between two existing views on the retrospective vote paradigm and the state of democratic accountability. On the one hand, studies on the economic vote and the electoral response to natural disasters have shown that voters are responsive to government performance.¹³ On the other hand, studies that focus on the interaction between partisan loyalty and retrospective evaluations suggest that partisan loyalty severely hampers the accountability process.¹⁴ It may be clear by now that I believe that neither of these views entirely reflect reality: the first seems too positive, the latter too negative. Rather, I have attempted to show that there are conditions under which voters act as ‘blind partisans’, consciously or subconsciously unable to process information that counteracts their partisan views. Yet, at other times, at least part of the electorate *does* respond to information about government performance. Although much is now known about the former, we still know little about the latter. This thesis has attempted to offer a first exploration of the conditions under which partisans exercise accountability toward their own party. I now discuss the implications of the findings presented in this thesis for the two main fields of study that this thesis relates to: the retrospective voting paradigm and literature on partisan loyalty and bias.

¹³Duch and Stevenson (2008); Nadeau et al. (2012); Powell and Whitten (1993); Lewis-Beck (1980, 1988); Lewis-Beck and Paldam (2000); Lewis-Beck et al. (2008); Lewis-Beck and Stegmaier (2013); Chzheng et al. (2014); Bermeo and Bartels (2014); Bechtel and Hainmueller (2011); Malhotra and Kuo (2008); Healy and Malhotra (2009)

¹⁴Bartels (1996, 2000, 2002); Bartels and Achen (2004); Achen and Bartels (2013, 2016); Evans and Andersen (2006); Evans and Pickup (2010); Pickup and Evans (2013); Huber et al. (2012); Gaines et al. (2007); Taber and Lodge (2006)

7.2.1 Voter and Performance Heterogeneity in Retrospective Voting

The empirical analyses conducted in this thesis suggest that two types of heterogeneity are important when studying retrospective voting: voter heterogeneity and heterogeneity in performance domains. First, in Chapter 3 I found that strong partisans were *less* likely to experience cross-pressure in light of economic information and in Chapter 5 I found that although partisans with a strong attachment to the Republican party were aware of the Watergate scandal and altered their opinion of President Nixon accordingly, they did not alter their behaviour at election time. Rather, Republican partisans with strong levels of partisan attachment voted for the Republican party in 1974 regardless of their knowledge of the Watergate scandal. On the contrary, Republican partisans with moderate and weak levels of affiliation were significantly *more* likely to alter their behaviour between 1972 and 1974 compared to non cross-pressured Democratic partisans. Moderate Republican partisans were also more likely to abstain from voting in the 1974 midterm elections. I also found that Republican partisans with *medium* levels of knowledge of politics were *more* likely to update their opinion on President Nixon in light of the Watergate scandal.

These patterns are in line with the expectations set out in the framework of cross-pressure in Chapter 2 and confirm existing literature on political knowledge and the formation of opinions and beliefs.¹⁵ Although it is widely documented that partisans with strong levels of party affiliations are least receptive to contradictory information and most resistant to behavioural change, the consistent finding that partisans with *moderate* levels of party affiliation behave in the opposite fashion offers a new insight to the literature on partisan identification. These findings suggest that levels of partisan identification might operate in a similar fashion to a voter's political

¹⁵See de Vries and Giger (2014); Duch et al. (2000); Zaller (1992) and sections 2.6.1 and 2.6.2 of the Theory chapter

sophistication which has been shown to have a non-linear effect on behavioural and attitudinal change.¹⁶

It is thus clear that important differences exist between voters as to the extent to which they exercise accountability toward the in-party. Leaning and moderate partisans as well as voters with moderate levels of knowledge about politics appear more likely to act against their own party. This thus suggest that it is important to explore retrospective voting and democratic accountability among specific subgroups of the electorate. If the electorate is studied at the aggregate level this might obscure important differences between subgroups of voters. Furthermore, if the aim is to increase the likelihood that partisans hold their own party to account, then it is crucial to develop a better understanding of the processes through which voters come to identify strongly with a particular party or why some voters develop high levels of interest in politics. The combination of these two factors appears especially damaging to democratic accountability.

A second source of heterogeneity in retrospective voting lies with the performance domain which voters evaluate. The empirical case studies presented in this thesis suggest that voters were responsive to some types of performance signals (the national economy, candidate competence), responded in a mixed fashion to some (the Watergate scandal) and ignored others (candidate corruption). In Chapters 3 and 4 I found that cross-pressured partisans were significantly more likely to vote for the out-party at election time. I did not find any evidence to suggest that partisan voters who experienced cross-pressure were more likely to abstain however. This was different in Chapter 5 where I found that cross-pressured partisans, specifically Republican partisans with moderate levels of affiliation to the party, were both more likely to switch to

¹⁶Taber and Lodge (2006); Zaller (1992); Stoker (1993)

the Democratic party in 1974, but were also significantly more likely to abstain from voting. This suggests that information on corruption or political scandals depresses turnout, whereas contradictory information on a government's economic performance or information on local candidate competence do not appear to have this effect.

If cross-pressure increases abstention then this could have negative as opposed to positive effects on democratic accountability. A decrease in voter turnout could facilitate the re-election of corrupt politicians if the incumbent has a large base of core supporters. The lab experiment further adds to the impression that voters might be less receptive to information about political corruption than to information about a government's economic performance or candidate competence. In the lab experiment participants neglected to punish candidates they knew to be corrupt solely because the candidate belonged to the in-group that was artificially constructed in the experiment. One explanation for this divergence could be that a voter's individual wellbeing is less directly affected by corruption as opposed to the state of the national economy. Another option could be that voters expect governments to be competent economic managers, and hence punish malperformance by selecting an alternative, but expect all politicians to be corrupt (to some extent) and are therefore dissuaded from voting or neglect to punish politicians when they receive information on corruption.

Much of the existing literature on retrospective voting focuses on the economic domain, yet findings in this area might not translate to other areas of government performance. Existing work on retrospective evaluations in other domains is rare but suggests that performance based voting relates strongly to the salience voters ascribe to a particular policy domain.¹⁷ As such, retrospective voting should perhaps not be seen in general terms, but rather in terms specific to individual voters. Thus,

¹⁷See de Vries and Giger (2014)

it appears unrealistic to assume that voters exercise accountability when they care little about a topic. Even if political scientists or democratic theorists are of the opinion that voters *should* care about an issue, such as corruption for instance, a failure of voters to respond to this type of performance does not necessarily imply that the entire retrospective mechanism does not work. Rather, in order to assess the retrospective vote mechanism we should identify performance areas that voters care about and then assess whether or not voters hold politicians to account for their performance on the specific topic.

7.2.2 The Multifaceted Nature of Partisanship

In this thesis I have focused my attention on partisans and their abilities to exercise accountability toward their in-party. At many points in the empirical analyses, the importance of partisanship to American politics was confirmed. I found for instance that strong partisan affiliations are an important barrier to negative information about the in-party (Chapter 3, Chapter 4) and also found that accountability is severely reduced when partisan stakes of an election are high (Chapter 4). Perhaps most surprisingly, I found that the introduction of non-partisan, non-ideological group identities significantly reduced the likelihood that lab participants punished corruption (Chapter 6). This highlights the importance of group and ‘us’ versus ‘them’ dynamics of elections.

Yet, in my view the most important contribution this thesis offers is to show that although partisan identification exerts strong effects on voters, these effects are not always present and may vary across electoral context. I found for instance that more than half of the Republican electorate stated that President George W.H. Bush did a poor job in handling the economy in 1992, but that President Clinton did a good job in 2000. Thus, partisans are ‘not fools’.¹⁸ Rather, these evaluations were in line

¹⁸In the words of Key (1966)

with the economic reality. Furthermore, a scandal the size of Watergate was impossible to ignore even for Republican partisans. Importantly however, I found that *not all partisans are the same*: some partisans, especially leaning and moderate partisans, are more receptive to contradictory performance information than partisans with strong levels of party affiliation and are more likely to alter their behaviour at election time. These partisans are less susceptible to the partisan filter and are less likely to join the fold on election day. The fact that partisanship mattered most in competitive elections and elections where ideological polarisation was high (Chapter 4) furthermore suggests that the salience of partisan identities might vary across elections. Importantly, this suggests that there is clear potential for political candidates to manipulate the salience of partisan identities to avoid electoral punishment for performance failures.¹⁹ If ideological polarisation increases, then partisan identities become more salient and performance differentials matter less to voters.

Taber and Lodge (2006) offer a helpful and nuanced discussion on the implications of partisan bias to democratic accountability.²⁰ The authors argue that although at first sight partisan bias appears mostly detrimental to the accountability process, it is not irrational for voters to attach significant importance and meaning to their partisan identity. In their words: ‘it is perfectly reasonable to give heavy weight to one’s own carefully constructed attitudes.’ In fact, it would seem fickle for voters to give up their identification with a particular party in light of the first bit of negative information. Similarly, we might judge a sports fan who abandons her team when it loses its first game. An important question that remains however is whether the majority of the electorate actually hold: ‘carefully constructed’ partisan attitudes’,²¹ or whether partisan affiliation operates more akin to: ‘a virus contracted from your

¹⁹See Stone et al. (2010); Buttice and Stone (2012) who show that candidates use their ideological position to either emphasize or downplay quality (dis)advantages.

²⁰See General Discussion section, p.767-768 of Taber and Lodge (2006)

²¹Taber and Lodge (2006, p.767)

parents at an early age.’²²

The difference between these two views likely affects our normative opinion on how ‘reasonable’ it is for voters to hold on to partisan loyalty in light of negative performance information. In light of this, Taber and Lodge (2006, p.768) state that:

skepticism is valuable and attitudes should have inertia. But skepticism becomes bias when it becomes unreasonably resistant to change and especially when it leads one to avoid information.

Suggesting that:

how we determine the boundary line between rational skepticism and irrational bias is a critical normative question, but one that empirical research may not be able to address.

I agree that statements as to when partisans *should* or *should not* update their views or exercise accountability and turn against their own party are within the domain of normative research. Yet, empirical research can certainly assess the empirical conditions under which partisans *do* and *do not* exercise accountability.²³ In other words, empirical research may explore where partisans *themselves* draw the line between bias and skepticism: when do voters hold preferred parties to account and when do they fail to do so? Existing research in the field of psychology suggests that in-group bias:

serves different functions (instrumental versus identity functions) for different persons (e.g. low and high identifiers with the group) in different contexts.²⁴

²²Fiorina (1981, p.ix)

²³Examples of studies moving in this direction are Parker-Stephen (2013); Dickerson (2016)

²⁴Scheepers et al. (2006, p.360)

These dynamics have been revealed in this thesis. I have found different types of responses to cross-pressure (vote-switching, abstention and no response) for different group members (low, medium and high sophisticates as well as weak and strong partisans) across different contexts (economy, candidate competence, political scandal and corruption). This thus suggests that research on partisan bias needs to take into account the multifaceted nature of partisan identification and partisan bias. Partisan bias might operate differently for different voters and may vary across context and performance domain. Moreover, different types of partisanship (expressive versus instrumental) and consequent biases might activate different types of behaviour.

7.3 Avenues for Further Research

In this final section I briefly highlight two avenues of further research on the basis of the findings presented in this thesis.

7.3.1 Cross-Pressured Partisans

In my opinion one of the most striking findings of this thesis is the fact that across the empirical chapters I have found that substantive segments of the partisan electorate reported performance evaluations that contradicted their partisan preferences. Similar findings have been reported in other studies on ambivalent and persuadable partisans.²⁵ The main point to take from this finding is the fact that although many partisans may be characterised as ‘blind loyalists’, partisans are not ‘fools’.²⁶ What is more, the focus on partisan bias and stability in the existing literature ignores, or

²⁵Lavine et al. (2012) for instance find that more than half the partisan electorate can be classified as ambiguous to the two main parties and Hillygus and Shields (2008) find that 76% of partisans in the 2004 presidential elections held issue positions that directly contradicted with the issue positions held by their in-party.

²⁶Lavine et al. (2012) use the term ‘blind partisans’ and Key (1966) uses the term ‘fools’.

hides, the fact that there are substantive portions of the partisan electorate who experience *internal conflicts* between their partisan attachment and either issue positions or performance evaluations. Currently, the decision-making process of these voters is poorly understood.²⁷ Yet, these ambivalent, persuadable or cross-pressured partisans form the basis of electoral change across elections and are the potential safe-keepers of democratic accountability. It would therefore be interesting for future studies on partisanship to focus more on partisans who break out of the partisan fold rather than adhere to it.

Related to this, it seems clear that if we want to understand the sources and consequences of partisan *bias*, we need to better understand the sources of partisanship itself.²⁸ This has been a longstanding discussion in the field of political science, but many contemporary studies on partisan bias in decision-making do not specifically address the question whether partisanship and its consequent bias are rooted in expressive or instrumental concerns. The lab experiment presented in this thesis showed that voters were reluctant to punish corrupt candidates on the basis of group identities that were clearly expressive rather than instrumental in nature. Here real advances might be made to improve our understanding of the effects of different types of partisan identification both on voting behaviour as well as on democratic accountability specifically. Research by Lavine et al. (2012) for instance suggests that the two types of partisan identification might reflect two different types of information processing: critical versus non-critical. Here, clear issues of causality exist: are voters critical decision-makers and therefore they develop instrumental rather than expressive partisan attachments or vice versa? Experimental research might untangle this relationship further.

²⁷Of course here the exceptions are the works of Hillygus and Shields (2008); Lavine et al. (2012)

²⁸For recent studies on this Green et al. (2002); Huddy and Aaroe (2015); Groenendyk (2012)

In line with Lavine et al. (2012), studies in the field of psychology suggest that different types of group identification (instrumental versus expressive) facilitate different motivational processes and consequent behavioural responses. More specifically, instrumental group membership is known to incite competitive behaviour aimed at attaining particular goals, usually increasing material gains for the group. Expressive group membership on the other hand has been shown to foster symbolic behaviour that is aimed at creating meaningful and positive ingroup identities that are distinct from outgroup identities.²⁹

This differentiation may speak to the difference found in this thesis between voters' response to economic and competence cross-pressure on the one hand and the lack of an electoral response to corruption in the lab experiment. If we assume that economic performance relates directly to citizens' sense of threat of losing material gains, then this may explain the electoral response of vote-switching. Corruption allegations on the other hand damage the positive self-image of the group because ingroup candidates fail to live up to moral standards. The electoral response to this may either be to ignore it (lab experiment) or to disengage from the process entirely (Watergate). It is clear that the application of the existing knowledge in the field of psychology on different types of group identification and consequent differences in group behaviour could be fruitful to further develop our understanding of various mechanisms of partisan bias.³⁰ Little is known to date as to whether voters *either* relate to a party in expressive or instrumental terms or whether *both* may exist at the same time. Another option might be that the electorate is heterogeneous in the type of partisan identification it holds. Furthermore, it remains an open question whether expressive or instrumental types of partisan identification may be activated, or primed, under different electoral conditions or across different performance domains.

²⁹Scheepers et al. (2006)

³⁰For an example Huddy and Aaroe (2015)

A final related question that I do not touch upon in the thesis is the topic of voters who identify as Independents. If partisan identification is indeed a major barrier to democratic accountability, then does this imply that voters who identify as Independents are in fact most likely to vote along retrospective lines? Debate exists whether Independents in the US are simply less interested in politics and therefore do not identify with either of the two parties,³¹ whether Independents possess a distinct, third type of party identification which reflects a more centrist view on politics, or whether most Independents actually lean toward a particular party and consequently behave similar to leaning or weak partisans.³² As such, a study on voters who identify as Independent could utilise this separation into three categories and assess the trade off between these various types of political identification and their effects on retrospective evaluations and democratic accountability.

7.3.2 Cross-Pressure in Multi-Party Systems

A second interesting way in which I think the research in this thesis may be extended is to explore cross-pressure in a non-US context. An obvious, and relatively easy, extension would be to apply the framework of cross-pressured voting to another two-party majoritarian system such as the UK. This would offer a helpful robustness test for the findings reported here. A second, more complicated, but perhaps also more interesting, option is to extend the research into cross-pressured partisans to multi-party contexts. The concept of cross-pressure as used in this thesis would have to be adapted for this purpose since partisan identification is much weaker in most multi-party systems. An alternative option would be to conceptualise cross-pressure as a tension between a voter's ideological placement that is salient in the

³¹Converse (2006); Berelson et al. (1954); Campbell et al. (1960), note that Converse (2006) is a reprint of his 1964 article.

³²See Klar (2014) for a discussion on the literature on Independents.

particular context. In some countries this might mean that voters identify as either ‘left’ versus ‘right’, in others the division might lie with ‘conservative’ versus ‘liberal’.

The study of cross-pressured voters in multi-party contexts might bring interesting dynamics to light about the effects of the institutional context on the trade-off that voters strike between their ideological (partisan) preferences and retrospective performance evaluations. For instance, existing literature on the ‘clarity of responsibility’ and ‘clarity of governance’ suggests that the level of economic voting is higher in systems where lines of accountability are clear and governing power is shared between either a small or a cohesive set of actors.³³ This suggests that the retrospective voting mechanism might perform less well in multi-party contexts where various parties share governing responsibility. On the other hand, the availability of multiple parties might actually *increase* the likelihood that cross-pressured voters hold their preferred party to account, simply because there will be more feasible alternative options available for a voter to vote for. The so-called ‘expressive costs’ of voting for the opposition party are much higher in a two-party system compared to a multi-party system, because in the latter more parties will fall within the ‘ideological comfort zone’ of a voter.³⁴ It would be important however that the potential ‘second choice’ party would actually have a feasible chance of gaining power or otherwise voters might not view the party as an actual alternative.³⁵ As such, a study of cross-pressure in multi-party contexts could relate to existing literature on clarity of responsibility as well as multi-party competition and offer a more in-depth account of the interaction between party loyalty and retrospective performance evaluations.

³³Powell and Whitten (1993); Anderson (2000); Tillman (2008); Hobolt et al. (2013)

³⁴Expressive costs is used by Fauvelle-Aymar et al. (2000)

³⁵Schleiter and Voznaya (2012)

7.3.3 Concluding Remarks

A mis-match exists between the theoretical understanding of democratic accountability and the behaviour of many voters at election time. As such, it is important to recognise that retrospective voting is more complicated than the theory might suggest and that for most voters it is but one of many considerations at election time. I disagree however with the argument that this implies we should revert to a theory of voting behaviour that relies solely on social identities.³⁶ This would completely ignore the advances that have been made in the study of retrospective voting as well as a wealth of empirical evidence that suggests that voters do care about incumbent performance.

It seems apparent that voters do not always act as: ‘rational Gods of vengeance and reward’,³⁷ but neither are they just blind party loyalists. As such, new avenues of research should aim to reconcile the demands of electoral democracy with our existing knowledge about the actual, biased, behaviour of partisans. It seems clear to me that although many partisans in the US vote for the party they identify with at election time, this behaviour hides a more complicated decision-making process that most voters engage in. Partisans are often presented with information about incumbent performance that conflicts with their partisan views. The majority of partisans ignore this information or simply do not receive it. Yet, an important minority receive the message and for these voters this conflicting information can incite behavioural change at election time. It is important to come to a better understanding of who these voters are and when and how they respond. This thesis has offered a first step in this process and has explored the behaviour of voters who peek over their white picket fence and see the trouble that threatens the safety of their partisan bubble.

³⁶This is advocated by Achen and Bartels (2016)

³⁷Key (1966, p.568)

Appendix A

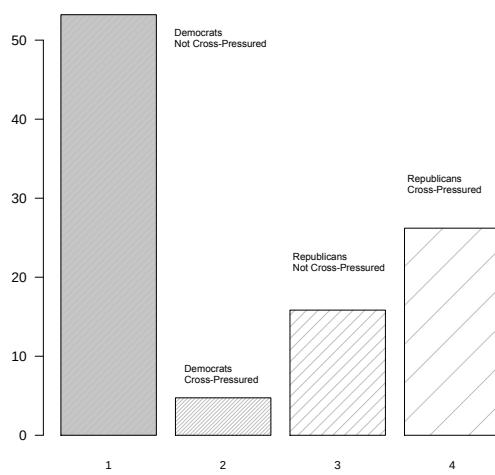
Appendix

Chapter 3 - Economic Cross-Pressure

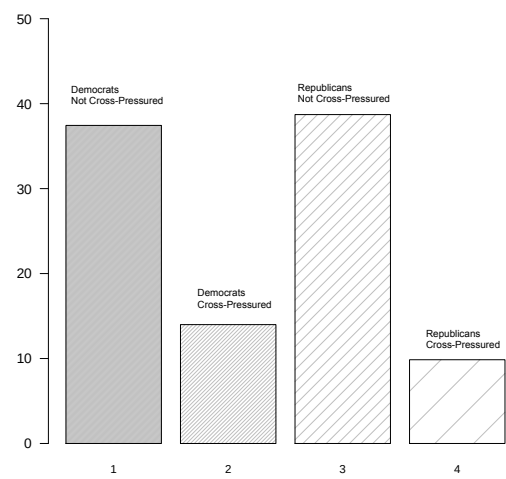
I re-estimate the number of cross-pressured voters using a lagged version of partisan identification, measured in t-1. Figure A.1 below shows the results. Because there is no wave prior to 2000; I only display the estimates for the 1992 Presidential Elections and the 2004 Presidential Elections. The estimates provided below are very similar to those presented in the main text of the thesis, Figure 3.2. In fact, in 1992 the percentage of Republican cross-pressured voters is somewhat *higher*; up from 57% to 62%. In 2004 the estimate for Republicans increases from 16% to 20%. For Democratic partisans the difference is 1.5% in 1992 (from 6.5 to 8%) and 6% in 2004 (from 21 to 27%). It is thus reassuring to note that the estimates are very similar when using the lagged version of partisan identification and are actually more conservative when we use partisan identity and economic evaluations that are recorded in the same survey wave.

Figure A.1: Number of Cross-Pressured Voters Using Lagged Partisan Identification

(a) 1992 Presidential Elections



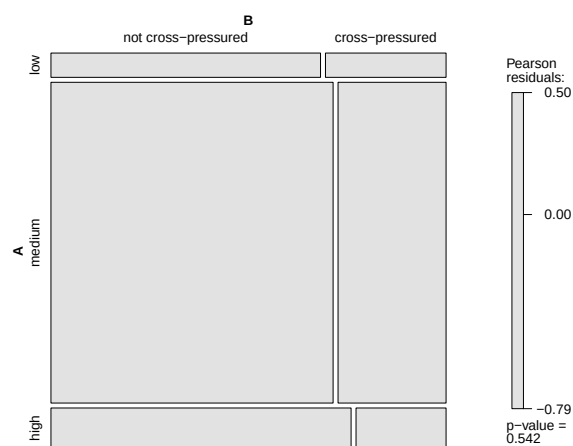
(b) 2004 Presidential Elections



Association Political Knowledge and Cross-Pressure

Figure A.2 displays a robustness check for the association between political knowledge and cross-pressure. In the main text the political knowledge score of the 1992 survey is used, here I use the political knowledge questions from the 1990 survey wave and cross-pressure as measured in 1990. This does not alter the substantive findings as presented in the main text.

Figure A.2: Association Political Knowledge and Cross-Pressure 1992 - Robustness Test



Variables included in Regression Models

In addition to the measure of cross-pressure that is discussed in the chapter, the following variables are included in the regression models presented in the chapter:

Partisan Strength

Variable consists of 3 categories: leaning; weak and strong partisan. Taken from the 7-point variable that asks respondents ‘Generally speaking do you think of yourself as a Republican, a Democrat, an Independent or what? Would you call yourself a strong or not very strong? If independent: would you consider yourself closer to the Democrat or Republican Party?’ Only the ‘true’ independents, those who responded independent to both the initial and follow-up question, are removed from the analysis. Leaning partisans are set as the reference category throughout all analyses.

Age

Continuous variable denoting respondents age based on year of birth.

Education

In panelwaves 1990, 1992, and 2000 the survey question distinguishes between 7 categories : 1 = 8 Grades or less - no diploma; 2 = 9-11 Grades -no further schooling ; 3 = High School or Equivalent ; 4 = More than 12 yrs of schooling, no higher degree; 5 = Junior or Community Level Degree; 6 = BA Degree; 7 = Advanced Degree. In panelwave 2004 the only information that is reported is whether or not respondents have completed high school (HS). Therefore, throughout all analyses education represents a binary variable that takes the value 0 is no HS was completed and 1 if respondent did complete high school.

Income

Categorical variable consisting of the following categories: 1 = 0-14,999 ; 2 = 15 - 34,999 ; 3 = 35 - 49,999 4 = 50 - 64,999; 5 = 65 - 84,999 ; 6 = 85 or above. For 2002 and 2004 the categories 'just about 50,000' and 'less than 50,000' were included in category 3. The categories 'more than 50,000' was included in category 4. Across all analyses the median level of income is calculated and a binary variable is created that indicates whether a respondent's household income was above or below the median.

Political Alienation

The questions asked in relation to respondents' trust in the political system and their sense of political efficacy are displayed below in Table 3. The question wordings differ somewhat between the 1990-1992 panel and the 2000-2004 panel. I use Mokkenscale analysis to create a political alienation scale.¹ The automated selection procedure (aisp) of the Mokkenscaling indicates that all three items load to 1 scale. The overall Loevinger H coefficients of scaleability and the scaleability score of individual items are displayed below. In line with general recommendations, an H-coefficient of 0.3 or above is considered to be acceptable, anything above 0.4 is considered strong. As can be seen below, all items have H-coefficients that can be considered acceptable or strong and the overall scales are strong. To create a scale, all individual items are simply added together. Higher values indicate higher levels of alienation with politics.

¹van der Ark (van der Ark, 2012); van Schuur (2003); Sijtsma et al. (1990); Sijtsma and Verweij (1992)

Table A.1: Mokken Scale Analysis - Political Alienation 1992

Survey Question Item	1990	1992
Government serves the interests of voters	0.52	0.40
Public officials care about what people like me think	0.35	0.36
People like me don't have a say in government	0.44	0.52
We can trust the government to do what is right	0.45	0.47
Overall Scale	0.42	0.46

Table A.2: Mokken Scale Analysis - Political Alienation 2000, 2004

Survey Question Item	2000	2004
My vote matters	0.47	0.49
Public officials care about what people like me think	0.52	0.48
People like me don't have a say in government	0.51	0.48
Elections make governments pay attention	0.39	0.35
Overall Scale	0.46	0.44

Political Knowledge Score

Table A.3 below provides an overview of the items used to construct the political knowledge variable. In line with the work conducted by John Zaller, factual political knowledge items are used as opposed to self-reported interest in politics.² Zaller and Price (1993) show that prior political knowledge is a more reliable measure of political sophistication than self-reported media use which is often based on over-estimation. The factual questions posed in the various surveys differ and are listed below. Note that no political knowledge questions were asked in 2004, as such I use respondents' political knowledge score from the 2000 survey. The newly created political knowledge score is recoded to range between 0 and 1, higher scores indicate a higher level of knowledge about politics.

²Stoker (1993); Zaller (1990); Price and Zaller (1993)

Table A.3: Political Knowledge Score

1990 and 1992 Survey		
Question Item	Score	Else
If respondent could identify:		
President of Russia Michael Gorbachev / Boris Jeltsin	2	1
Vice President Dan Quale	2	1
Speaker of the House Tom Foley	2	1
Chief Justice William Rehnquist	2	1
If Respondent knew:		
Which Party held the Majority in the House (1990)	2	1
Which Party held the Majority in the Senate (1990)	2	1
Who decides whether a law is constitutional (1992)	2	1
Who appoints justices to the Supreme Court (1992)	2	1

2000 Survey		
Question Item	Score	Else
If Respondent Could Identify		
Senate Majority Leader Trent Lott	1	0
Chief Justice Rehnquist	1	0
UK Prime Minister Tony Blair	1	0
Attorney General Reno	1	0
If Respondent knew:		
Home state George Bush	1	0
Home state Al Gore	1	0
Home State Dick Cheney	1	0
Home State Joseph Lieberman	1	0
If respondent knew:		
Name and Party House Candidate	2	-
Name only	1	-
Else	0	-
Name and Party second House candidate	2	-
Name only	1	-
Else	0	-

Table A.4: Robustness Check 1: Binary Logistic Regression - Likelihood Experiencing Cross-Pressure Including State Fixed Effects

	1992		2000		2004	
	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>
Age	0.02 (0.02)	-0.02* (0.01)	-0.03 (0.03)	0.00 (0.01)	-0.04* (0.02)	0.00 (0.01)
Female	-0.70 (0.63)	-0.15 (0.42)	0.78 (0.93)	0.00 (0.36)	-0.60 (0.47)	0.12 (0.42)
Race	0.99 (1.09)	-14.56 (1294)	1.11 (1.72)	0.04 (0.74)	0.26 (0.73)	1.29 (0.94)
College degree	-0.47 (0.78)	-0.18 (0.48)	-0.18 (0.93)	0.28 (0.39)	-1.75** (0.57)	-0.04 (0.47)
Income above median	0.62 (0.65)	0.25 (0.40)	-0.16 (0.76)	0.33 (0.39)	-0.13 (0.56)	-0.49 (0.44)
<i>base cat: leaning</i>						
Moderate partisan	0.27 (0.65)	0.20 (0.47)	-1.85 (1.09)	0.54 (0.51)	0.82 (0.56)	-0.91* (0.46)
Strong partisan	-0.78 (0.86)	-1.42** (0.48)	-1.24 (0.91)	-0.66 (0.45)	-0.96 (0.59)	-1.84*** (0.52)
Political knowledge	-1.08 (0.25)	0.18 (0.47)	-0.58 (0.53)	0.17 (0.24)	0.44 (0.33)	-0.05 (0.27)
Political knowledge squared	0.11 (0.12)	-0.03 (0.07)	0.04 (0.05)	-0.01 (0.02)	-0.06 (0.04)	0.00 (0.02)
Political alienation	-0.65* (0.32)	0.13 (0.19)	0.11 (0.29)	0.10 (0.17)	-0.63** (0.24)	0.44** (0.16)
N	183	148	83	162	200	247
LR Chi-Squared	16.55*	24.62**	10.22	11.91	39.21***	34.68***
State Level Fixed Effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>

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

Robust Standard Errors in Parentheses

Table A.5: Robustness Check 2: Binary Logistic Regression - Lagged Independent Variables Measured in 1990 and 2000

	1992		2004	
	<i>Democrats</i>	<i>Republicans</i>	<i>Democrats</i>	<i>Republicans</i>
Age	0.02 (0.01)	0.00 (0.01)	0.00 (0.02)	0.00 (0.02)
Female	-0.19 (0.48)	0.30 (0.27)	-0.93 (0.56)	-0.36 (0.56)
Race	0.79 (0.76)	-0.34 (0.55)	-0.17 (0.79)	-0.58 (1.29)
College degree	0.43 (0.53)	0.08 (0.28)	0.09 (0.59)	0.22 (0.62)
Income above median	-0.25 (0.46)	-0.30 (0.26)	-0.09 (0.55)	-0.44 (0.62)
<i>base cat: leaning</i>				
Moderate partisan	0.15 (0.54)	-0.41 (0.30)	-0.07 (0.59)	-1.58* (0.75)
Strong partisan	-0.64 (0.59)	-1.20*** (0.34)	-1.35* (0.67)	-1.85* (0.79)
Political knowledge	0.42 (0.70)	0.22 (0.32)	0.29 (0.41)	0.17 (0.41)
Political knowledge squared	-0.10 (0.12)	-0.04 (0.05)	-0.06 (0.05)	0.00 (0.04)
Political alienation	-0.17 (0.23)	0.03 (0.12)	-0.04 (0.23)	0.67* (0.28)
N	326	327	125	127
LR Chi-Squared	7.11	20.43*	13.94	16.75*
State Level Fixed Effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>

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

Robust Standard Errors in Parentheses

Independent variables measured in 1990 and 2000

Chapter 4: Candidate Competence Cross-Pressure in the 2006 Midterm Elections

This section provides robustness checks for the models presented in Chapter 4. Specifically:

- Table A.6 replicates the binary regression model of cross-pressure on abstention but includes a *categorical* variable of cross-pressure
- Table A.7 displays the statistical differences between the 4 categories of voters (cross-pressured and non cross-pressured)
- Table A.8 displays the full model of the effects of ideological polarisation on the behaviour of cross-pressured partisans
- Table A.9 displays the full model of the effects of electoral competitiveness on the behaviour of cross-pressured partisans

Table A.6: Binary Logistic Regression Model Effect Cross-Pressure on Abstention and Vote-Choice in 2006 House Elections

	Abstention		In-Party Vote	
	Model 1	Model 2	Model 3	Model 4
<i>base: incumbent partisan non Cross-Pressured</i>				
Incumbent Partisan				
Cross-Pressured - Voter Evaluations	1.27 (1.17)	— —	−3.78*** (1.16)	— —
Challenger Partisan				
Not Cross-Pressured - Voter Evaluations	0.47 (0.56)	— —	−0.25 (0.71)	— —
Challenger Partisan				
Cross-Pressure - Voter Evaluations	— —	— —	−3.89** (1.47)	— —
<i>base: incumbent partisan non Cross-Pressured</i>				
Incumbent Partisan				
Cross-Pressured - Expert Evaluations	— —	−0.31 (0.54)	— —	−1.64* (0.65)
Challenger Partisan				
Not Cross-Pressured - Expert Evaluations	— —	−1.05 (0.65)	— —	−1.06 (0.64)
Challenger Partisan				
Cross-Pressure - Expert Evaluations	— —	−1.60* (0.78)	— —	−1.08* (0.56)
Age	−0.04 (0.02)	−0.06*** (0.02)	0.02 (0.02)	−0.01 (0.01)
Female	0.18 (0.58)	0.73 (0.43)	0.89 (0.02)	0.60 (0.34)
Non-White Ethnicity	0.89 (0.69)	0.63 (0.56)	0.89 (0.76)	0.93 (0.62)
College degree	−0.76 (0.60)	−0.68 (0.40)	0.75 (0.72)	−0.37 (0.47)
<i>base: leaning partisan</i>				
Moderate Partisan	−0.31 (0.80)	−0.23 (0.39)	−1.07 (0.84)	−1.29*** (0.47)
Strong Partisan	−0.17 (0.52)	−0.89* (0.45)	−0.21 (0.76)	0.28 (0.57)
<i>base: below 25,000</i>				
Income 25,000-60,000	1.81 (1.29)	1.12 (0.60)	−0.06 (1.25)	−0.54 (0.67)
Income 60,000 - 100,000	1.78 (1.30)	0.88 (0.58)	0.04 (1.28)	0.04 (0.74)
Income 100,000 and above	1.54 (1.41)	1.20 (0.70)	−0.61 (1.57)	−0.24 (0.85)
<i>base: economy stayed the same</i>				
Economy got worse	−0.89 (0.72)	0.09 (0.43)	0.09 (0.93)	0.39 (0.50)
Economy got better	−0.33 (0.64)	−0.75 (0.58)	0.09 (0.84)	0.67 (0.45)
Republican Incumbent	0.07 (0.65)	1.64* (0.65)	0.49 (0.88)	0.72 (0.66)
Spending Democratic Candidate	−0.01 (0.03)	0.03 (0.02)	0.05 (0.03)	0.00 (0.03)
Spending Republican Candidate	−0.02 (0.03)	−0.03 (0.03)	−0.00 (0.02)	0.01 (0.02)
Constant	−1.34 (2.41)	−1.49 (1.30)	−0.08 (2.13)	3.03* (1.17)
N	301	544	283	496
Pseudo R-Squared	0.10	0.15	0.23	0.13
Wald Chi Square Statistic	10.91*	32.25***	32.06***	48.24***

*** p < .001; ** p < .01; * p < .05
Standard errors clustered at the district level

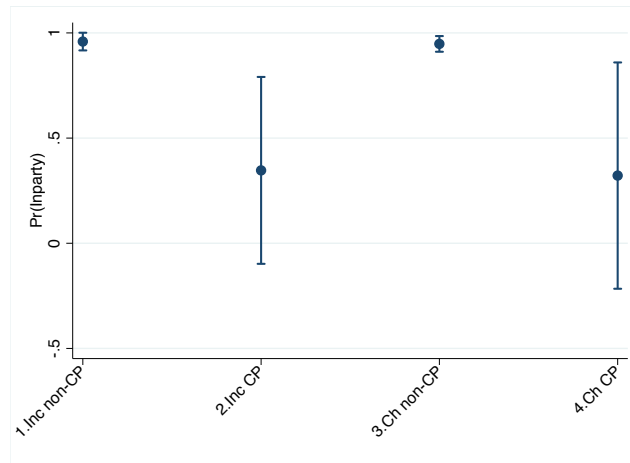
Table A.7: Statistical Differences Cross-Pressured and Non Cross-Pressured Partisans

Voter Evaluations				
Candidate Competence				
			Abstention Model	Vote Choice Model
Incumbent Partisans	Cross-Pressured	Non Cross-Pressured	p <0.281	p <0.001
Challenger Partisans	Cross-Pressured	Non Cross-Pressured	p <0.401	p <0.006
Expert Evaluations				
Candidate Competence				
			Abstention Model	Vote Choice Model
Incumbent Partisans	Cross-Pressured	Non Cross-Pressured	p <0.564	p <0.012
Challenger Partisans	Cross-Pressured	Non Cross-Pressured	p <0.103	p <0.978

The predicted likelihoods of an in-party vote using the categorical version of cross-pressure are shown below in Figure A.3. The difference between incumbent partisans who are cross-pressured and incumbent partisans who are not cross-pressured are statistically significant both when using the voter ratings of candidates and when using expert ratings of candidate competence (also see Table A.7). The difference between challenger incumbents is significant only when using voter measures (upper panel).

Figure A.3: Predicted Likelihood of Voting for the In-Party for Cross-Pressured and Non Cross-Pressured Partisans in the 2006 House Elections

(a) Voter Evaluations of Candidate Competence



(b) Expert Evaluations of Candidate Competence

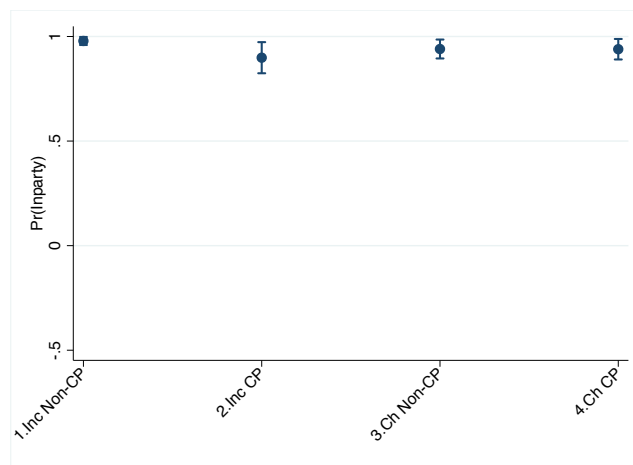


Table A.8: Conditional Effect Ideological Polarisation on Behaviour of Cross-Pressured Partisans

	Model 1	Model 2
Cross-Pressure - Voter Evaluations	-4.18* (1.96)	— —
Cross-Pressure Expert Measure	— —	-1.46 (0.87)
Level of Ideological Polarization	0.62* (0.32)	0.00 (0.22)
Interaction CP * Ideological Polarization	-0.37 (0.76)	0.27 (0.34)
Age	0.04 (0.03)	-0.01 (0.01)
Female	2.26** (0.86)	0.56 (0.37)
Ethnicity	1.63 (1.05)	0.95 (0.69)
Education	0.46 (0.79)	-0.39 (0.52)
<i>base: leaning partisan</i>		
Moderate partisan	-0.48 (0.98)	-1.23** (0.46)
Strong partisan	-0.37 (0.93)	0.23 (0.57)
<i>base: below 25,000</i>		
Income 25,000-60,000	-1.79 (1.16)	-0.56 (0.77)
Income 60,000 - 100,000	-0.64 (1.03)	-0.03 (0.76)
Income 100,000 and above	-1.90 (1.51)	-0.24 (0.85)
<i>base: economy stayed the same</i>		
Economy got worse	0.23 (1.28)	0.29 (0.49)
Economy got better	0.09 (1.09)	0.77 (0.49)
Republican Incumbent	0.68 (1.31)	0.23 (0.54)
Spending Democratic Candidate	0.06 (0.06)	-0.01 (0.03)
Spending Republican Candidate	-0.03 (0.03)	0.01 (0.02)
Constant	-2.46 (3.44)	2.99 (1.48)
N	264	492
Pseudo R-Squared	0.34	0.13
Wald Chi Square Statistic	44.42***	52.17***

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

Standard errors clustered at the district level

Table A.9: Conditional Effect District Competitiveness on Behaviour of Cross-Pressured Partisans

	Model 1	Model 2
Cross-Pressure - Voter Evaluations	-3.37 (5.56)	— —
Cross-Pressure Expert Measure	— (1.14)	-0.54 (1.14)
District Competitiveness	1.21 (0.90)	0.35 (0.59)
Interaction CP * District Competitiveness	-0.32 (2.46)	-0.18 (0.60)
Age	0.02 (0.02)	-0.01 (0.01)
Female	1.10 (0.80)	0.55 (0.33)
Ethnicity	0.74 (1.16)	0.98 (0.61)
Education	0.95 (0.77)	-0.37 (0.47)
<i>base: leaning partisan</i>		
Moderate partisan	-0.59 (0.74)	-1.24* (0.52)
Strong partisan	-0.08 (0.71)	0.21 (0.60)
<i>base: below 25,000</i>		
Income 25,000-60,000	0.00 (1.10)	-0.55 (0.67)
Income 60,000 - 100,000	0.49 (1.10)	0.02 (0.77)
Income 100,000 and above	-0.51 (1.54)	-0.23 (0.86)
<i>base: economy stayed the same</i>		
Economy got worse	0.02 (0.97)	0.39 (0.50)
Economy got better	0.33 (1.01)	0.70 (0.41)
Republican Incumbent	-0.19 (0.81)	0.01 (0.58)
Spending Democratic Candidate	0.01 (0.03)	-0.01 (0.03)
Spending Republican Candidate	0.01 (0.01)	0.01 (0.02)
Constant	-2.47 (2.43)	2.51 (1.51)
N	277	496
Pseudo R-Squared	0.27	0.12
Wald Chi Square Statistic	35.84**	53.83***

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

Standard errors clustered at the district level

Chapter 5: Cross-Pressure in Response to the Watergate Scandal

Watergate: District Level Analysis: Changing District Boundaries

Boundary changes occurred in the electoral districts for the House between 1972 and 1974. The majority of the candidates under study ran in the same district in both years. Yet, the boundary changes led to a change in electoral district for the representatives from California. In 1972 House representative Moorhead ran in district 20, in 1974 he ran in district 22. In 1972 House representative Wiggins ran in district 25 and in 1974 he ran in district 39.

Table A.10: Changes in District Boundaries

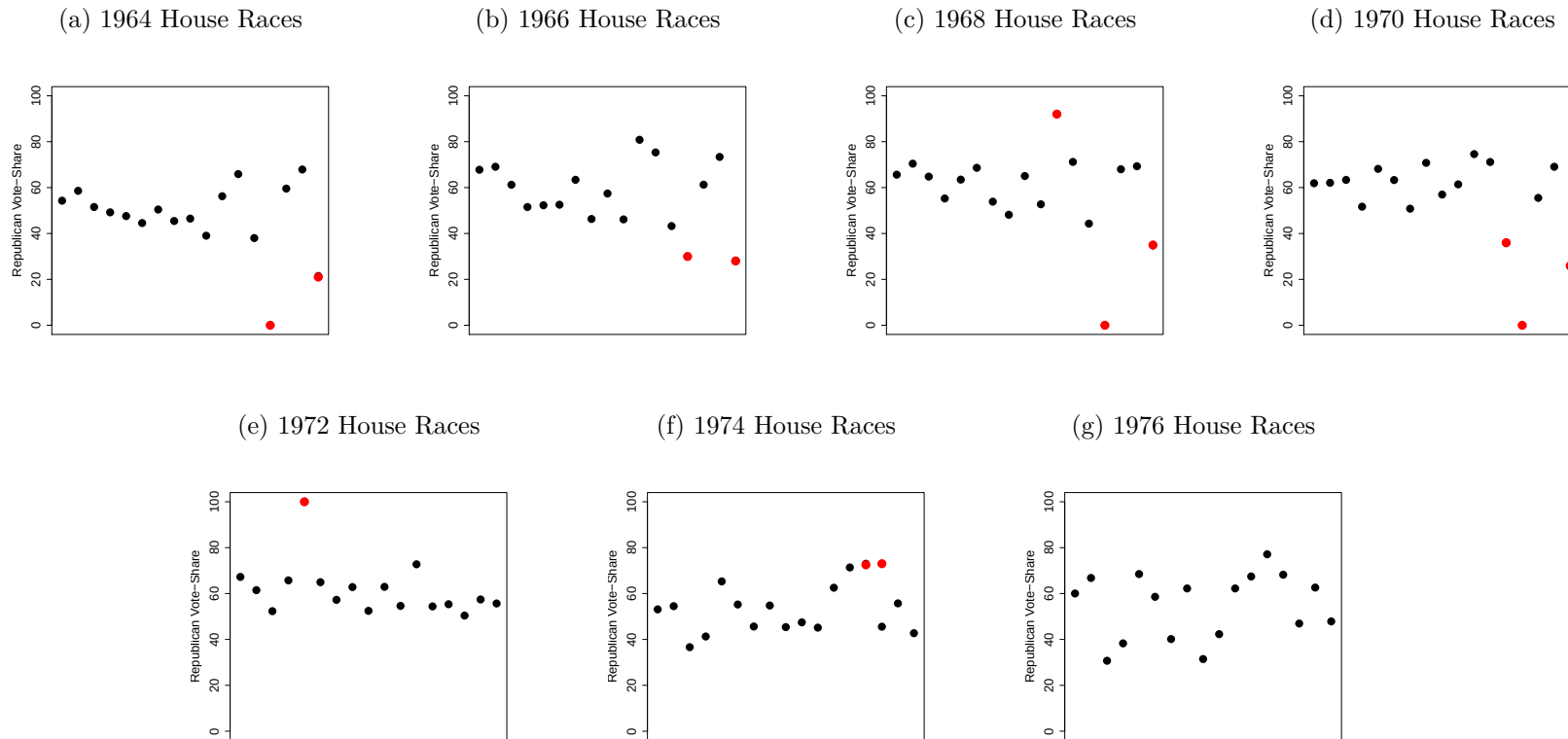
Change	District	Representative
In 1972		
Prior to 1972	NY 28	Fish Jr.
Post- 1972	NY 25	Fish Jr.
Prior to 1972	NY 40	Smith III
Post-1972	NY 36	Smith III
Prior to 1972	ILL 12	McClory
Post 1972	ILL 13	McClory
Between 1972-1974		
1972 and prior	CAL 20	Moorhead
1974 and post	CAL 22	Moorhead
1972 and prior	CAL 25	Wiggins
1974 and post	CAL 39	Wiggins

Statistical Outliers in District Level Analysis

Potential outliers are highlighted as red dots in Figure A.4. I calculate whether or not an observation should be considered an outlier using the rule of thumb that outliers are observations that fall 1.5 IQR below or above Q1 or Q3. Based on these calculations the following observations are considered outliers:

- 1964: New Jersey District 13 - Republican vote share 21.5%
- 1970: New Jersey District 13 - Republican vote share 25.5%
- 1974: Maine District 13 - Republican vote share 71.3%
- 1974: Mississippi District 14 - Republican vote-share 73%

Figure A.4: Statistical Outliers in House Races 1964-1972



Mapping Census Data

Because the census data does not identify data at the level of the electoral district, but rather at the level of city units, cities listed in the census needed to be mapped to the relevant electoral districts in order to create Table 5.3 in the main text. *the Historical Atlas of United States Congressional Districts 1789-1983* was used to map cities listed in the census to the relevant electoral district. Note that for some districts no cities were listed in the census. For instance, no cities were included for the districts in Maine, Maryland and Michigan. For some districts (those in California), a large number of cities were listed. If this was the case, then the average value across cities is reported. For most districts only one city could be mapped. This might be problematic because it is not clear whether the cities listed in the census data are necessarily representative of the district as a whole.

Table A.11: Mapping Cities to Electoral Districts

District	Member	Skeptic or Loyal	Cities Available in Census
California 20	Moorhead	Loyal	Los Angeles
California 25	Wiggins	Loyal	Anaheim, Arcadia, Azusa, Baldwin park Buena Park, Cobina, El Monte, Fullerton La Habra, La Puente, Temple City, Walnut Creek West Covina, Whittier
Illinois 13	McClory	Skeptic	Palatine
Illinois 19	Railsback	Skeptic	Rock Island
Indiana 10	Dennis	Loyal	Fort Wayne
Iowa 6	Mayne	Loyal	Sioux City
New Jersey 2	Sandman	Loyal	Atlantic City
New Jersey 13	Maraziti	Loyal	Elizabeth, Linde, Rahway Bayonne, Jersey City
New York 25	Fish Jr.	Skeptic	Yonkers
New York 36	Smith III	Loyal	Buffalo
Maine 2	Cohen	Skeptic	None Available
Maryland 5	Hogan	Skeptic	None Available
Michigan 4	Hutchinson	Loyal	None Available
Mississippi 5	Lott	Loyal	Jackson
Ohio 5	Latta	Loyal	Sandusky
Virginia 6	Butler	Skeptic	Roanoke
Wisconsin 8	Froehlich	Skeptic	Manitowoc

Comparability of Districts using Individual Level Data

In addition to the aggregate level data, there is sparse individual level data available on voters living in districts that are represented by Republican members of the Judiciary Committee on Impeachment. The ANES 1972-1976 panel study includes information on four of the seventeen districts under study. The ANES panel includes respondents from New York district 25 [N=17], Michigan district 4 [N=24], Iowa district 6 [N=25] and California district 39 [N=12]. This gives a total of 78 individuals on whom I have information. Unfortunately however, only one of these districts (New York 25) was represented by a Nixon skeptic, the other three were represented by Nixon loyalists. Table A.12 provides some descriptive information on the opinion of

the respondents in the available districts in the 1972 ANES wave prior to Watergate. Of most interest is the comparison between the ‘skeptic’ [NY 25] and the ‘loyal’ [other three] districts. The data shows that the proportion of respondents who identified as Republican in the skeptic district is comparable to the proportion in two of the loyal districts. Yet, Nixon appears somewhat less popular in the New York district based on both his average thermometer score and the percentage of respondents who report to have voted for him in the 1972 elections. This could suggest that the skeptic position taken by district 25’s House Representative [Fish Jr.] was reflective of the opinion of his constituents. It must be noted however that these descriptive statistics are based on a very small number of observations.

Table A.12: Comparison Individual Level Data on House Judiciary Districts

	NY 25	MICH 4	IOWA 6	CAL 39
Republican Identifiers 1972	30%	17%	36%	33%
Republican Identifiers Nixon Thermometer 1972	76.7	97.3	89.1	86
Nixon Vote 1972	57.1%	72.3%	60%	71.4%

Data Simulations Excluding California - Vote Shares

Table A.13: Descriptive Statistics Vote Share Distribution - Excluding California

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
-0.33	0.32	4.87	-0.01 p < 0.59	0.13 p < 0.00

Figure A.5: Simulation Treatment Effect Vote Share - Excluding California

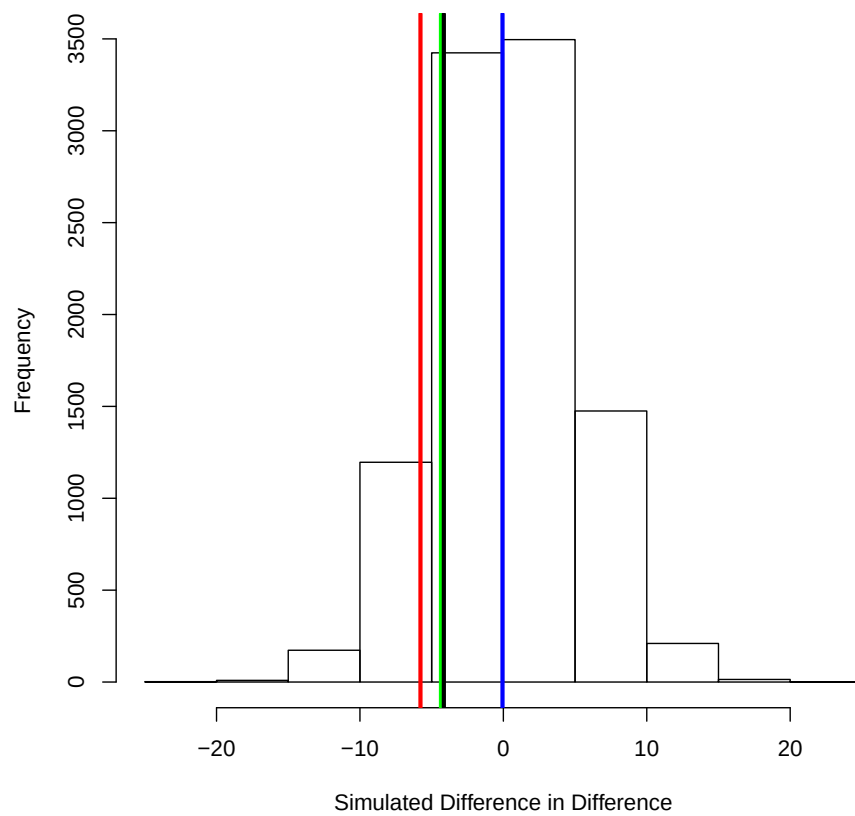


Table A.14: Significance Difference in Difference Model Estimates - Excluding California

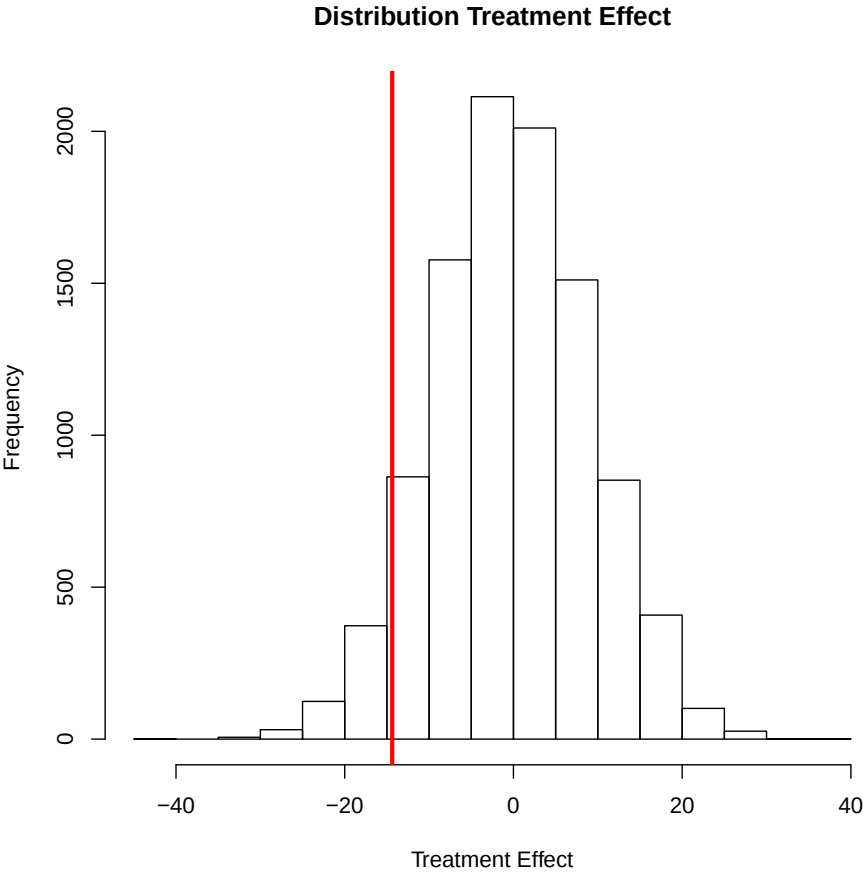
Model Estimate	Two-Tailed Test	One-Tailed Test
Model 1: -0.06	p <0.99	p <0.48
Model 2: -4.35	p <0.38	p <0.17
Model 3: -5.78	p <0.24	p <0.11
Model 4: -4.15	p <0.40	p <0.18

Data Simulations Excluding California - Victory Margins

Table A.15: Descriptive Statistics Margins of Victory Distribution - Excluding California

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
-0.12	-0.07	9.34	0.00 p <0.76	0.05 p <0.22

Figure A.6: Simulation Treatment Effect Margins of Victory - Excluding California



Difference in Difference Estimate Turnout

Table A.16: Difference in Difference Estimates 1972-1974 Voter Turnout

	Model 1	Model 2
Treatment Effect	5.92 (5.71)	3.46 (5.73)
Intercept	-28.45*** (3.93)	-26.15*** (3.96)
District Fixed Effects:	yes	yes
N	16	14
R-squared	0.66	0.62
Sample:	Subset	Subset

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

Standard Errors clustered at District Level

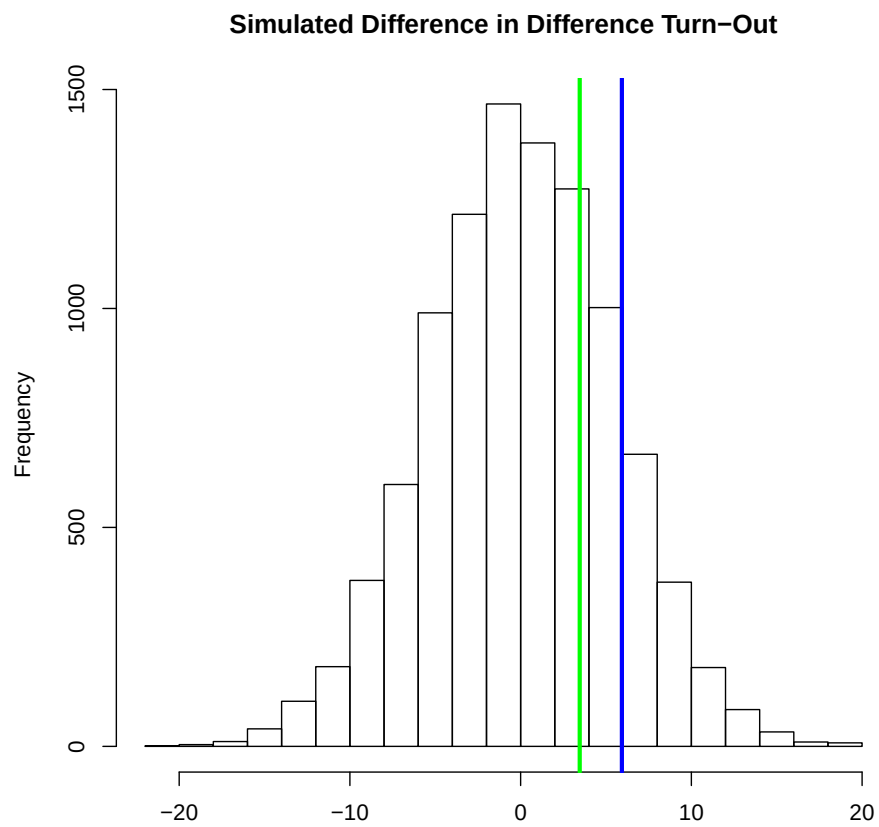
Table A.17: P-Values Model Estimates Turnout

Test	Model 1	Model 2
Model Estimate	5.92	3.46
One-Tailed	p < 0.13	p < 0.25
Two-Tailed	p < 0.26	p < 0.52

Table A.18: Descriptive Statistics Distribution Turnout

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
-0.01	-0.00	5.36	-0.03 p < 0.25	2.96 p < 0.53

Figure A.7: Distribution Simulation Difference in Difference Turnout 1972-1974 Elections



Difference in Difference Estimates Third Party Vote Share

Table A.19: Difference in Difference Estimates 1972-1974 Third Party Vote Share

	Model 1	Model 2	Model 3
Treatment Effect	-1.75 (3.19)	- 2.28 (3.62)	- 2.76 (4.24)
Intercept	3.15 (2.96)	3.67 (3.41)	4.41 (4.03)
District Fixed Effects:	yes	yes	yes
N	17	16	14
R-squared 0.14		0.16	0.14
Sample:	All Districts	Subset	Subset

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

Standard Errors clustered at District Level

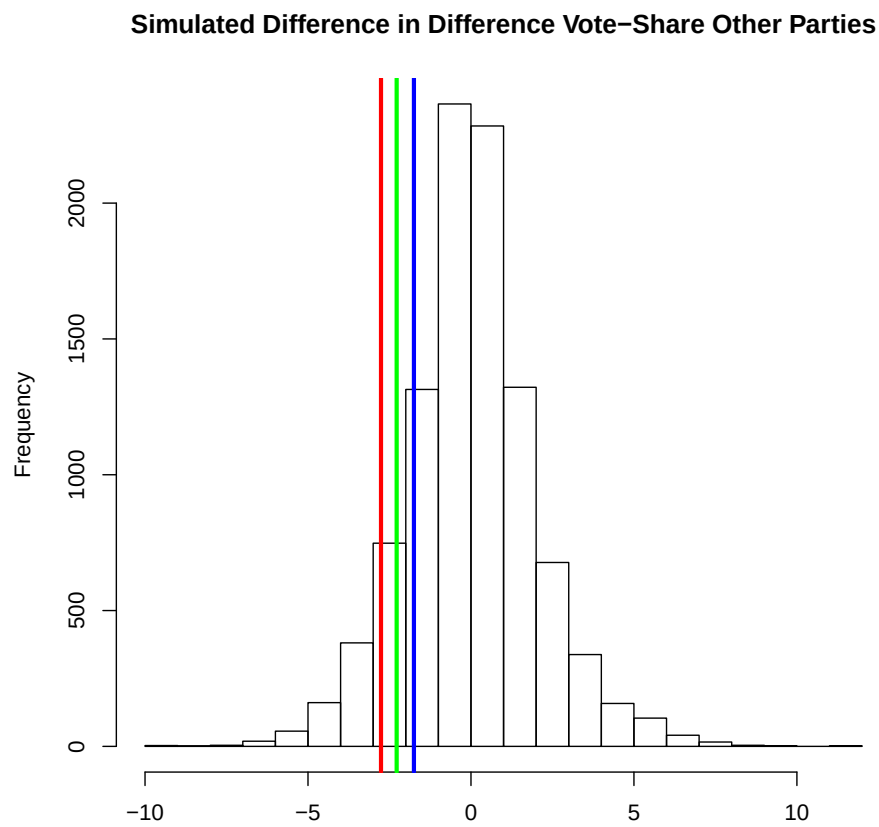
Table A.20: Descriptive Statistics Distribution Third Party Vote Share

Mean	Median	Standard Deviation	Skewness Test	Kurtosis Test
-0.00	-0.02	2	0.19*** p <0.00	4.30*** p <0.00

Table A.21: P-Values Model Estimates Third Party Vote Share

Test	Model 1	Model 2	Model 3
Model Estimate	-1.75	-2.28	-2.76
One-Tailed	p <0.17	p <0.12	p <0.08
Two-Tailed	p <0.32	p <0.22	p <0.16

Figure A.8: Distribution Simulation Difference in Difference Third Party Vote Share 1972-1974 Elections



Robustness Checks Individual Level Analysis Watergate

- Table A.22 provides a robustness check for the model on the change in Nixon thermometer but includes two additional variables, race and religion
- Table A.23 provides a robustness check for the model on turnout of Republican partisans but includes three additional variables: opinion on the Vietnam war, race and religion
- Table A.24 provides a robustness check for the model on in-party voting of Republican partisans but includes three additional variables: opinion on the Vietnam war, race and religion

Table A.22: Robustness Check: Change in Nixon Thermometer Scores - Republican Partisans

Model 1	
Age	−0.10 (0.12)
Female	4.52 (3.22)
Non-White	−5.46 (8.03)
In Employment	0.68 (2.56)
High School	−3.86 (3.83)
High School +	−7.61* (3.56)
Religious	1.61 (3.00)
Economy Got Better	−1.65 (4.00)
Economy Got Worse	−4.19 (3.61)
Moderate Partisan	− 1.38 (2.70)
Strong Partisan	3.48 (3.11)
Medium Interest	−8.20** (2.42)
High Interest	− 4.27 (2.71)
Constant	− 20.88 (15.61)
State Fixed Effects	Yes
N	566
Adj. R-squared	0.03

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

Robust Standard Errors in Parentheses

Models include Republican partisan identifiers only

Table A.23: Robustness Check 1: Binary Logistic Regression Model Voter Turnout in the 1974 Midterm Elections Including Vietnam, Race and Religion

	Model 1	Model 2
Voted in 1972	2.11 (1.54)	– 0.53 (0.97)
<i>base category: Democrats and Independents</i>		
Weak Republican	–0.99 (1.14)	– 0.67 (0.78)
Moderate Republican	–2.09* (1.04)	–1.46 * (0.71)
Strong Republican	–0.56 (1.50)	–0.18 (0.96)
Age	0.00 (0.03)	0.01 (0.02)
Non-White	–2.72 (2.05)	–0.58 (1.01)
Religious	–1.18 (0.88)	–0.58 (0.59)
Female	0.60 (0.86)	–0.17 (0.63)
Employed	0.51 (0.92)	0.11 (0.62)
<i>base category: no high school</i>		
High School	1.29 (1.42)	0.77 (0.87)
High School +	1.22 (1.26)	0.97 (0.74)
<i>base category: economy stayed same</i>		
Economy got better	–1.62 (1.11)	–1.14 (0.77)
Economy got worse	0.13 (1.13)	–0.15 (0.81)
<i>base category: not interested in politics</i>		
Medium Interest	0.00 (1.50)	–0.53 (0.96)
High Interest	–0.00 (1.42)	–0.50 (0.94)
Opinion on Vietnam War	–0.06 (0.22)	–(0.07) (0.16)
Constant	- -	3.90 (3.45)
N	116	379
LR Chi-Squared	11.34	-
Wald Chi Square Statistic	-	10.81
State Fixed Effects	Yes	No

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

Robust Standard Errors in Parentheses

Table A.24: Robustness Check - Binary Logistic Regression Model: In-Party Vote in the 1974 Midterm Elections including Vietnam, Race and Religion

	Model 1
<i>base category: Democratic partisans</i>	
Weak Republican	−1.13*** (0.43)
Moderate Republican	−0.65 (0.46)
Strong Republican	−0.23 (0.47)
Age	0.00 (0.01)
Female	−0.29 (0.34)
Non-White	1.79* (0.90)
Religious	0.42 (0.32)
Employed	−0.16 (0.35)
<i>base category: no high school</i>	
High School	−0.72 (0.52)
High School +	−0.56 (0.46)
<i>base category: economy stayed same</i>	
Economy got better	−0.05 (0.42)
Economy got worse	−0.56 (0.42)
<i>base category: not interested in politics</i>	
Medium Interest	−0.06 (0.31)
High Interest	0.37 (0.38)
N	330
LR Chi-Squared	19.45
State Fixed Effects	Yes

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

Robust Standard Errors in Parentheses

Chapter 6: A Lab-Experiment on Corruption and Cross-Pressure

Background Information on the Lab Experiment

Information about Subjects, Procedures, Information and Payoffs

The experiments were conducted with subjects who were randomly assigned to the experiment from a subject pool from the University of Birmingham Experimental Economic Lab (BEEL) where the experiments were conducted. All the participants in the session received the same treatment, but the treatment was administered in each session was randomized. Before each treatment, participants were asked questions about their socio-economic background in a questionnaire. The subject pool consists of undergraduate students from all faculties at the University of Birmingham. The standard procedures of experimental economics were followed, granting full anonymity and confidentiality.

Candidates and voters received the following information. While in the control group, voters only received information about which of the two candidates is the winner that round, what their earnings are given the winning candidate, what their earnings would have been if the other candidate would have been the winner, and how many valuable red tokens the computer found while flipping the tokens that were kept by each candidate. In the treatment groups, voters were also informed about their own group identity [KLEE or KANDINSKY] and that of the candidate. In the control group, candidates were informed about whether or not they were the winner in that round or not, and how many valuable red tokens the computer found while flipping the tokens they decided to keep. Voters received information about which of the two candidates was the winner that round, what their earnings are given the winning candidate, what their earnings would have been if the other candidate would have

been the winner, and how many valuable red tokens the computer found while flipping the tokens that were kept by each candidate. In the treatment groups, candidates were informed about their own group identity and those of the voters. The payoffs in the experiment are calculated in the following way. First, everybody participating in the experiment received a show-up fee of £2.50. Second, candidates receive a fixed amount of two valuable red tokens every round and one extra valuable red token if they are the winner in that given round. In addition, they can keep the number of valuable red tokens that they decided not to send to voters, if none of them are flipped. The exchange rate is: 4 tokens equals £1.00. Third, the amount voters receive per round depends on the number of valuable red tokens sent by the candidate who won that round. The number of valuable red tokens sent by the candidate are doubled and divided equally among voters. The average earning per participants was £17.00 and the average duration of each session was one-and-a-half hours.

Experimental Instructions

The purpose of this experiment is to study how people make decisions in a particular situation. Feel free to ask the monitor any questions you may have as they arise. No communication with other participants is allowed until the end of the session. This experiment consists of four modules. Outcomes of past modules can be used in future modules. We ask that you stay until the end of the session, which will last about 90 minutes. Instructions will be handed out at the beginning of each module.

Module 1

Note that the instructions were used for both control and treatment, but the information was only used for Klee/Kandinsky treatment.

In this module everyone will be shown five pairs of two paintings by the artists Paul Klee and Wassily Kandinsky. You will be asked for each pair to choose which painting you like most. On the basis of your five choices of paintings you will be classified as a member of the KLEEs (or as a KLEE as a shorthand) or a member of the KANDINSKYs (or KANDINSKY as a shorthand). To be more precise, participants will be divided into two roughly equally sized groups, KLEEs and KANDINSKYs, based on the similarity of their members' artistic preferences.

Everyone's identity as a KLEE or KANDINSKY will stay fixed throughout the experiment. You will be informed about your classification, whether you are a KLEE or KANDINSKY, privately.

Module 2

Questionnaire used for both control and treatment

Question 1: Gender

Are you?

1. Male
2. Female

Question 2: Age:

What year were you born?

Question 3: Social Class

Taking everything into account, how would you characterise the standard of living of the family in which you were born? If you think of a scale from 1 to 7, where 1 means a poor family, 7 a rich family, and the other numbers are for the positions in between, about where would you place the family you were born in?

Table A.25: Social Class

1	2	3	4	5	6	7
Poor Family						Rich Family

Question 4: Political Ideology

In political matters people talk of “the left” and “the right”. What is your position? Please indicate your views using any number on a scale from 0 to 10, where 0 means “left” and 10 means “right”. Which number best describes your position?

Table A.26: Political Ideology

0 Left	2	3	4	5	6	7	8	9	10 Right
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Question 5: Political Knowledge

Now some questions about Britain. For each one, could you please tell me whether you believe they are true or false?

Table A.27: Political Knowledge

	TRUE	FALSE
The unemployment rate in Britain is approx. 8 percent at the moment	1	2
The British Secretary of Health is Jeremy Hunt	1	2
Individuals must be 25 yrs or older to stand as candidates in Britain general elections	1	2
There are 969 members of the British House of Commons	1	2

Question 6: Political Interest

To what extent would you say you are interested in politics? Very, somewhat, a little, or not at all?

- 1 - Very
- 2 - Somewhat
- 3 - A little
- 4 - Not at all

Module 3: Control

This module consists of five blocks of five rounds. At the beginning of each block you will be assigned to a group with four other participants, and the composition of the group will remain the same throughout the block. You will not know the identity of the other group members.

In each group two members will act as candidates – Candidate X and Candidate Y – and the other three as voters. Your role stays fixed throughout the different blocks of this module. Your identity and those of other group members will not be revealed after the experiment is completed.

In every round each candidate receives 18 tokens. There are two types of tokens: red tokens which are valuable and blue tokens that are worthless. Between one and six of the 18 tokens that each candidate receives are randomly assigned to be valuable red tokens.

At the beginning of each round voters need to decide which candidate to vote for. The candidate that receives the majority of votes, two or more, will be called the winner.

Before knowing the outcome of the vote, candidates send six tokens to voters and can decide how many of these six are valuable red tokens. The candidates keep the rest of the tokens (12 in total). The value, i.e. colour, of the 12 tokens that each candidate keeps for himself is unknown to voters except for two tokens which are randomly flipped by a computer. The flipping of the tokens by the computer will reveal if none, one or both tokens that a candidate keeps are valuable. If the computer finds one or more valuable red tokens, all other valuable red tokens held by the candidate will

become worthless. This information will be made available to the voters in the group.

Your earnings in this module will be calculated in the following way:

Everybody will receive a show-up fee of 2.5 GBP

The exchange rate is: 4 tokens = 1 GBP

If you are a candidate: You will receive a fixed amount of two valuable red tokens every round and one extra valuable red token if you are the winner in that given round. In addition, you can keep the number of valuable red tokens that you decide not to send to voters, if none of them are flipped.

If you are a voter: The amount you receive per round depends on the number of valuable red tokens sent by the candidate who won that round. The number of valuable red tokens sent by the candidate will be doubled and divided equally among voters.

The information you receive is:

As a candidate: whether you are the winner in that round or not, and how many valuable red tokens the computer found while flipping the tokens you decided to keep.

As a voter: which candidate is the winner that round, what your earnings are given the winning candidate, what your earnings would have been if the other candidate would have been the winner, how many valuable red tokens the computer found while flipping the tokens that were kept by each candidate.

Module 3: Treatment Klee/Kandinsky

This module consists of five blocks of five rounds. At the beginning of each block you will be assigned to a group with four other participants, and the composition of the group will remain the same throughout the block. You will not know the identity of

the other group members.

In each group two members will act as candidates – Candidate X and Candidate Y – and the other three as voters. Your role stays fixed throughout the different blocks of this module. Your identity and those of other group members will not be revealed after the experiment is completed.

At the beginning of the block voters are informed about which type of player each candidate is, KLEE or KANDINSKY.

In every round each candidate receives 18 tokens. There are two types of tokens: red tokens which are valuable and blue tokens that are worthless. Between one and six of the 18 tokens that each candidate receives are randomly assigned to be valuable red tokens.

At the beginning of each round voters need to decide which candidate to vote for. The candidate that receives the majority of votes, two or more, will be called the winner.

Before knowing the outcome of the vote, candidates send six tokens to voters and can decide how many of these six are valuable red tokens. The candidates keep the rest of the tokens (12 in total). The value, i.e. colour, of the 12 tokens that each candidate keeps for himself is unknown to voters except for two tokens which are randomly flipped by a computer. The flipping of the tokens by the computer will reveal if none, one or both tokens that a candidate keeps are valuable. If the computer finds one or more valuable red tokens, all other valuable red tokens held by the candidate will become worthless. This information will be made available to the voters in the group.

Your earnings in this module will be calculated in the following way:

Everybody will receive a show-up fee of 2.5 GBP

The exchange rate is: 4 tokens = 1 GBP

If you are a candidate: You will receive a fixed amount of two valuable red tokens every round and one extra valuable red token if you are the winner in that given round. In addition, you can keep the number of valuable red tokens that you decide not to send to voters, if none of them are flipped.

If you are a voter: The amount you receive per round depends on the number of valuable red tokens sent by the candidate who won that round. The number of valuable red tokens sent by the candidate will be doubled and divided equally among voters.

The information you receive is:

As a candidate: whether you are the winner in that round or not, and how many valuable red tokens the computer found while flipping the tokens you decided to keep.

As a voter: which candidate is the winner that round, what your earnings are given the winning candidate, what your earnings would have been if the other candidate would have been the winner, how many valuable red tokens the computer found while flipping the tokens that were kept by each candidate.

Module 4:

Instructions used for control and treatment

In this Module you will make ten choices, but only one of them will be used in the end to determine your earnings in this module. Each decision is a paired choice between "Option A" and "Option B." Even though you will make ten decisions, only one of these

will end up affecting your earnings, but you do not know in advance which decision will be used. Each decision has an equal chance of being used in the end. When you have made your ten choices, the server will select one of them at random, and a second random selection will determine your earnings for that particular choice (A or B).

The sort of decisions you are going to make are the following: Decision 1, Option A pays 2.00 pounds with a probability of 10%, and it pays 1.60 pounds with a probability of 90%. Option B yields 3.85 pounds with a probability of 10%, and it pays 0.10 pounds with a probability of 90%. The other decisions are similar, except that the probability of getting the highest payoff for each option will increase. In fact, for decision 10, Option A pays 2.00 pounds with probability 100%, and Option B yields 3.85 pounds with probability 100%. In this case, the second random selection of the server will not be needed in order to calculate the payoffs since both options definitely yield the highest payoff, so here your choice is between 2.00 pounds or 3.85 pounds.

These earnings will be added to your previous earnings, and you will be paid all earnings in cash when we finish.

Descriptive Statistics Main Variables

Table A.28: Descriptive Statistics Variables Used in Analyses

Variable	Mean	Standard Deviation	Min	Max
Caught in Corruption Audit	0.19	0.38	0	1
Embezzlement Information	0.36	0.23	0	0.5
Clean For Sure	0.01	0.11	0	1
In-Group Loyalty	0.5	0.5	0	1

Robustness Check Experimental Results

- Table A.29 replicates the experimental results for the control group but includes individual level covariates
- Table A.30 replicates the experimental results for the control group but uses a linear probability model
- Table A.31 replicates the experimental results for the treatment group but includes individual level covariates
- Table A.32 replicates the experimental results for the treatment group but uses a linear probability model

Table A.29: Experimental Results: The Effects of Corruption Information on Vote-Choice - Including Individual Covariates

Dependent Variable: Vote-Choice				
Independent Variables	Model 1	Model 2	Model 3	Model 4
Caught	-0.129** (0.061)		-0.077 (0.063)	-0.431** (0.185)
Embezzlement Information		-3.069*** (0.230)	-3.059*** (0.230)	-3.321*** (0.265)
Caught * Embezzlement				1.093** (0.537)
Clean For Sure:		1.396*** (0.234)	1.375*** (0.234)	1.423*** (0.235)
Constant	-0.570 (24.26)	7.450 (24.86)	7.015 (24.86)	6.704 (24.88)
Fixed Effects	Yes	Yes	Yes	Yes
Individual Level Co-Variates	Yes	Yes	Yes	Yes
N	2,368	2,368	2,368	2,368
Number of Clusters	60	60	60	60

***p < .01; **p < .05; *p < .10

Coefficients are panel probit regression coefficients

Fixed Effects for rounds and blocks, standard errors clustered at individual level

Table A.30: Linear Probability Model: The Effects of Corruption Information on Vote-Choice

Dependent Variable: Vote-Choice				
Independent Variables	Model 1	Model 2	Model 3	Model 4
Caught	-0.051 (0.034)		-0.0281 (0.034)	-0.146* (0.076)
Embezzlement Information		-1.141*** (0.101)	-1.136*** (0.101)	-1.216*** (0.112)
Caught * Embezzlement				0.362** (0.195)
Clean For Sure:		0.507*** (0.073)	0.499*** (0.0720)	0.514*** (0.074)
Constant	0.512*** (0.009)	0.865*** (0.036)	0.807*** (0.0381)	0.897*** (0.043)
Fixed Effects	Yes	Yes	Yes	Yes
N	2,368	2,368	2,368	2,368
R-Squared	0.002	0.082	0.083	0.084

***p < .01; **p < .05; *p < .10

Table Entries are ordinary least squares regression coefficients

Robust standard errors clustered at the individual level

Fixed Effects are included for round and block

Table A.31: Experimental Results: The Effects of Corruption Information on Vote-Choice Including Group Membership - Including Individual Covariates

Dependent Variable: Vote-Choice		
Independent Variables	Out-Group Candidate	In-Group Candidate
caught	−0.84** (0.39)	−0.15 (0.39)
embezzlement information	−3.53*** (0.53)	−3.26*** (0.56)
caught * embezzlement	1.86* (1.05)	0.26 (1.08)
clean for sure -	0.39	
	-	(0.92)
constant	−54.69 (59.18)	−11.78 (66.45)
fixed effects	yes	yes
individual covariates	yes	yes
N	583	584
Number of clusters	30	30

***p < .01; **p < .05; *p < .10

Coefficients are panel probit regression coefficients

Fixed Effects for rounds and blocks

Standard errors clustered at individual level

Table A.32: Experimental Results: The Effects of Corruption Information on Vote-Choice Including Group Membership - Linear Probability Model

Dependent Variable: Vote-Choice		
Independent Variables	Out-Group Candidate	In-Group Candidate
caught	−0.30* (0.17)	−0.02 (0.13)
embezzlement information	−1.28*** (0.21)	−1.17*** (0.22)
caught * embezzlement	0.64 (0.47)	0.02 (0.39)
clean for sure	0.70*** 0.06	0.12 (0.39)
constant	0.98*** (0.09)	0.92*** (0.09)
fixed effects	yes	yes
N	584	583
R-Squared	0.10	0.09

***p < .01; **p < .05; *p < .10

Coefficients are panel probit regression coefficients

Fixed Effects for rounds and blocks

Standard errors clustered at individual level

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