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Electoral Consequences of Individual Politicians' Pledge Fulfilment*

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Abstract

Voters' ability to act upon the fulfilment of election pledges matters profoundly for democratic accountability. Existing literature provides evidence of retrospective voting on pledge fulfilment at the party level. This paper investigates retrospective voting on pledges at the level of individual politicians. It estimates the effect of breaking a pledge to support anti-corruption legislation on Czech deputies' preferential votes. Since the pledge was identical for all the participating deputies and not all deputies pledged, the data permit estimation of the effect of pledge breakage independently of the effect of voting against anti-corruption bills. Results show that retrospective voting on pledges occurs even at the level of individual politicians when information about pledge fulfilment is easily accessible. Voters "punish" pledge breakage alone, *i.e.*, they do not punish voting against anti-corruption bills if the politician has not pledged to act otherwise.

Keywords: Election pledges, pledge fulfilment, retrospective voting, anti-corruption legislation

JEL Classification: D72, D73, C12

1. Introduction

The traditional model of democratic representation accentuates the idea that during electoral campaigns politicians make promises about the policies they will pursue if elected. While election promises serve multiple purposes, that of attracting votes appears as publicly the most prominent –

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and also increasingly common. The increasing volatility of voters, professionalization and marketization of politics, as well as its mediatization incentivize political parties to use promises in election campaigns (Håkansson and Naurin, 2016). Promises (pledges) can drive voters' decision-making at the ballot box both retrospectively and prospectively. Voters may punish (reward) broken (kept) promises, or they may choose whom they support based on promised future policies.

Retrospective voting, *i.e.*, voters casting votes based on politicians' past actions or performance, has profound implications for accountability. Democratic elections are commonly viewed as a disciplining device, providing incentives and constraints to politicians' behaviour (e.g., Aldashev, 2015; Ferraz and Finan, 2011; Pereira *et al.*, 2009); though it is also acknowledged that they do not always succeed in doing so (de Vries and Solaz, 2017). In theory, and to some extent in practice, voters use elections to reward politicians for desirable behaviour and punish them for undesirable behaviour. Retrospective voting has been extensively studied with respect to the economy, corruption and, to a lesser extent, election pledges.

Retrospective voting on election pledges has been investigated in experimental and survey settings (Krishnarajan and Jensen, 2021; Naurin *et al.*, 2019) as well as with real-life data on election results (Matthieß, 2020; Plescia and Kritzinger, 2017; Johnson and Ryu, 2010). Overall, the evidence suggests that retrospective voting on pledge fulfilment occurs at the level of political parties. Yet, with party-level data, it is difficult to distinguish the effect of fulfilling the pledge to implement a certain policy from the effect of the policy implementation itself. Parties tend to promise policies that appeal to citizens – so when they implement them successfully and enjoy voters' increased support in the next election, does that support reflect the desired policy or the fulfilment of the pledge that preceded it?

This paper investigates voters' response to politicians breaking a pledge of support for anti-corruption legislation. It advances the current research in two ways. Firstly, it focuses on retrospective pledge voting at the level of individual politicians. We employ data from a civic initiative that asked candidates for the 2013 election to the Chamber of Deputies of the Czech Republic to sign a pledge of support for anti-corruption legislation. The pledge was identical for all the participating deputies, and subsequent voting on anti-corruption bills of all deputies, regardless of whether they signed the pledge, was monitored. This makes it possible to estimate the effect of pledge breakage (fulfilment) separately from the effect of voting against (in support of) anti-corruption bills. Thus, we should be able to conclude not only whether deputies who fulfil their pledge receive more preferential votes on average than those who break it, as we would expect from the literature on party pledges. We should also be able to establish whether this effect is due to their voting on anti-corruption legislation in itself or whether it has to do with the associated pledge. That would be our second contribution to the existing literature.

From a wider perspective, our paper helps understand how voters in semi-open list systems allocate preferential votes among candidates with respect to the fulfilment of election pledges.

This is of importance because preferential votes not only help individual politicians win a seat; they also matter profoundly to internal party promotions and appointments to leading party positions (Folke *et al*., 2016).

The paper is organized as follows. Section 2 reviews the existing literature. Section 3 describes the data and method used. Section 4 presents the results, which are discussed in Section 5. Section 6 concludes.

2. Review of Existing Literature

When citizens vote retrospectively, they base their decisions at the ballot box on politicians' past performance. A classic example is that of voters rewarding (punishing) the incumbent government for good (poor) economic performance. De Vries and Solaz (2017) characterize retrospective voting as a three-step process that involves information acquisition, blame attribution and behavioural response. Not only are governments or governing parties subject to retrospective voting. Opposition parties, too, can expect to be judged on their past conduct (Plescia and Kritzinger, 2017).

Election (electoral) pledges are "commitments in parties' programs to carry out certain policies or achieve certain outcomes" (Thomson *et al.*, 2017, p. 2). These commitments are an integral part of election campaigns in democratic societies: they inform voters about the goals and policies to be pursued. Probability of fulfilment is not the only criterion based on which parties choose their commitments. Pledges are also made with the aim of appealing to voters and appeasing internal party factions (Thomson *et al*., 2017).

Election pledges are kept to a considerably greater extent than conventional wisdom would suggest (Mansergh and Thomson, 2007; Thomson, 2011; Naurin, 2011; Thomson *et al.,* 2017). Mansergh and Thomson (2007) report partial or full fulfilment rates of 50% in Ireland and 57% in the Netherlands.

The pervasiveness of the pledge-breaking politician stereotype – despite empirical evidence of the contrary – has motivated researchers to take a closer look at how citizens perceive politicians' pledges and how they evaluate the fulfilment thereof.

Messages that are vague (as opposed to specific) and bindingly framed ("we will" as opposed to "we want to") are more likely to come across as pledges (Dupont *et al.*, 2019). Citizens tend to perceive a statement as a pledge when it concerns them personally and when they consider the message sender competent in the respective policy area (Dupont *et al*., 2019) and in control of the promised legislative output (Krishnarajan and Jensen, 2021).

Actual policy performance is the most important determinant of citizens' evaluations of pledge fulfilment. However, a number of subjective factors – party identification, level of information, trust in political parties, and personal experience with the issue to which the pledge pertains – also play a role, and often lead to more negative evaluations of pledge fulfilment than actual performance would merit (Thomson, 2011).

Existing empirical evidence regarding retrospective voting on pledges is less mixed than that on corruption or the economy, and suggests that voters do respond to the keeping or breaking of electoral pledges by political parties. Matthieß (2020), for example, found that government parties which fulfil a higher share of their election pledges are less likely to suffer electoral losses. Retrospective pledge voting appears to be asymmetric, with broken pledges often affecting government evaluations more than fulfilled pledges (Naurin *et al*., 2019,; biased by partisan sympathies, as voters are not willing to hold their preferred party accountable (Krishnarajan and Jensen, 2021), and – unlike retrospective voting on the economy – not moderated by clarity of responsibility (Matthieß, 2020). Electoral pledges may themselves act as a moderating factor. Johnson and Ryu (2010) observed that electoral pledges in Latin American presidential democracies condition retrospective economic voting. Abandoning the pledged economic policy increases the salience of economic issues in the next election, intensifying voters' punishment (reward) for poor (good) economic performance.

The goal of this paper is to provide further insight into the empirical relationship between electoral pledge fulfilment and election results. It addresses two specific research questions that arise from our current state of knowledge. The first question concerns the level at which retrospective pledge voting occurs. We know from existing research that governments as well as political parties, both governing and opposition, are subject to the phenomenon. What about individual politicians? Provided with relevant information, do voters respond at the ballot box to the keeping or breaking of pledges by individual members of the national legislature?

Secondly, we are interested in the role of the pledge itself – the moral commitment if you will – separate from the effect of the pledged policy. An election pledge usually entails the promise of a policy that voters find desirable. When such a pledge is kept and rewarded with votes, it is unclear whether voters are rewarding integrity, manifested by the fulfilled pledge, or the adoption of a desired policy. When we observe retrospective pledge voting on a particular issue, would we not observe the same result in the absence of the pledge? Do voters actually evaluate pledge fulfilment or do they vote on the related legislative output or policy outcome?

We reformulate these questions as testable hypotheses once we have familiarized the reader with the case at hand, and conclude this section with a warning note.

This review was written with a strict focus on retrospective pledge voting to avoid getting caught up in the many and complex factors that affect voting decisions. The interested reader may see Frank and Martínez i Coma (2021) for a list of correlates of voter turnout alone, and Arzheimer *et al*. (2017) for complex coverage of electoral behaviour. Electoral pledge fulfilment is just one of the behaviours that bear upon politicians' re-election chances. Other examples are corruption (Vuković, 2020), public spending and debt (Balaguer-Coll *et al*., 2015) and political transformism (Brancati *et al*., 2022). Electoral pledges stand out among these for two reasons.

Firstly, they are becoming increasingly common. In Sweden, the number of electoral pledges by parliamentary parties increased from 46 to 149 per manifesto between 1991 and 2010 (Håkansson and Naurin, 2016). Secondly, pledges give rise to moral expectations and offer citizens a means of control over future political decisions. Unfulfilled pledges elicit disappointment, feelings of betrayal and loss of control. Severe mismatch between pledges and actual policies thereby contributes to trust crises in contemporary democracies and undermines the legitimacy of representative democracy (Guinaudeau and Persico, 2018).

3. Data and Method

The dataset used in this study combines information from two sources. Data on political candidates and preferential votes are downloaded from the Czech Statistical Office; data on deputies' voting are collected from the website of the Reconstruction of the State project.

The two hundred members of the Czech Chamber of Deputies are elected every four years. Before every election, political parties provide ranked lists of their candidates for each of the regions in which the party decides to run, *i.e.*, the party lists are region-specific. The order of candidates on the list is a matter of internal party negotiations. Candidates at the top of the list have higher probability to be awarded a seat in the legislative assembly than candidates further down the list. Voters cast a single vote for their preferred party, and in addition may award up to four preferential votes to individual candidates on that party's regional list. Only parties with five or more percent of the party votes cast nationwide are assigned a share of seats in the Chamber. These seats are filled with candidates in the order in which they appear on the party lists; however, candidates receiving over 5% of the preferential votes cast within their regional party lists move to the top.

"Reconstruction of the State" is a project designed to curtail political corruption in the Czech Republic: a joint effort of politicians, citizens, the civil society and the business community. Prior to the 2013 parliamentary election, its representatives asked candidates to sign a pledge of support for anti-corruption bills. By signing the pledge, candidates promised that, if elected, they would vote in favour of nine specific anti-corruption bills. These included, for example, making the financing of political parties and their election campaigns more transparent, requiring the disclosure of politicians' financial situation or establishing an online register of contracts entered into by public institutions. Out of the 1,500 signatories, 165 appeared in the newly elected Chamber of Deputies (Reconstruction of the State, n. d.). During the following term, participating politicians were notified of key upcoming voting sessions on the specified anti-corruption bills and reminded of their pledge. Deputies' activity (voting and legislative proposals) related to the bills was monitored and relevant data were disclosed online in the form of easy-to-read visualizations.

The projects' website provided a number of different statistics. The main page with pictures and names of all the elected deputies for the term 2013–2017 contained the following information: whether the deputy had pledged, the number of times they voted in serious contradiction of the pledge, and a colour-coded strip that indicated whether the deputy generally supported or opposed anti-corruption legislation (regardless of whether they had pledged). Selecting a deputy's profile led to a page with more detailed statistics: a pie chart of the deputy's voting record showing the percentages of support/opposition/abstention and absence; a bar chart on legislative proposals indicating the extent to which the deputy proposed amendments aimed at advancing and strengthening the anti-corruption bills as opposed to hindering or weakening them; and a pie chart that combined the information on voting and proposals. A document explaining how the statistics were calculated was available for download.

There are reasons to suspect that, in 2017, citizens voted retrospectively on the passing of anti-corruption bills as well as on the pledges thereof. The way the initiative was communicated to the public made it clear that the promise to support anti-corruption bills was in fact a pledge. Obtaining information on every deputy's voting record and pledge fulfilment was nearly costless, requiring no more than a few clicks. And even though immigration replaced corruption in 2017 as the most important political issue of the then upcoming election,[1] corruption continued to remain a recurring concern in the Czech public sphere.

We can now restate our hypotheses in light of the case at hand.

Hypothesis H_1: Deputies who break their pledge more often receive fewer preferential votes in the next election than deputies who breach less often.

Hypothesis H_2: Deputies who pledged to support anti-corruption bills lose more preferential votes when voting against these bills than deputies with similar voting behaviour who did not pledge.

Table 1 lists and describes the variables used henceforth. The unit of observation is the deputy, denoted by the subscript *i*. Data on candidates' characteristics pertain to 2017, while voting data relate to the period 2013–2017.

1 According to the respondents of a nationwide opinion poll carried out by the Czech Academy of Sciences.

Table 1: Definitions of variables

Variable name	Definition
Prefvotes$_i$	Percentage of preferential votes (within regional party lists) the deputy *i* received in 2017.
Pastprefvotes$_i$	Percentage of preferential votes (within regional party lists) the deputy *i* received in 2013.
Pledge$_i$	Binary variable equal to 1 if the deputy *i* signed the pledge; 0 otherwise.
Nbroken$_i$	Number of times the deputy *i* broke the pledge. This means they did not vote in favour of the bill in the third reading or voted for a fundamental amendment that would make it a different bill. The Reconstruction of the State made the participating deputies aware of these voting sessions ahead of time. This variable is available only for deputies who pledged.
%opposed$_i$	Percentage of instances in which the deputy *i* voted against the bills specified in the pledge, *i.e.*, voted for rejecting or softening the bills.
Ptyleader$_i$	Binary variable equal to 1 if the deputy *i* served as party leader during the term 2013–2017.
Age$_i$	The deputy *i*'s age.
Sex$_i$	Binary variable equal to 1 if the deputy *i* is male; 0 if female.
Party$_i$	Political party with which the deputy *i* is associated; employed as a set of dummy variables.
Listrank$_i$	The deputy *i*'s rank on the party list; employed as a set of dummy variables.

Source: Author's own elaboration

The following eliminations are made to the original dataset of two hundred deputies. Firstly, since the 2017 election results are needed for estimation, only observations for deputies who ran for re-election in 2017 are kept. Secondly, the sample is limited to political parties with at least ten observations, leaving the following parties in the sample: ANO, TOP 09, Communists, Civic Democrats, Social Democrats and Christian Democrats. Thirdly, government members are excluded from the dataset. As they have unique responsibilities and also receive substantial media coverage, voters may evaluate their pledge fulfilment in specific ways, differently from ordinary deputies. This leaves us with 124 observations.

Table 2 shows the descriptive statistics of the main variables of interest; values are reported separately for deputies who pledged and those who did not. Deputies who had pledged opposed anti-corruption bills to a lesser extent than those who had not pledged; the difference between the means of *%opposed* by *pledge* is statistically significant (t-test p-value < 0.01). A simple regression of *pastprefvotes* on *pledge* and available control variables *age*, *sex*,

ptyleader and *party* suggests that, in 2013, candidates who pledged did not receive significantly different preferential vote shares from those who chose not to pledge.[2]

Table 2: Descriptive statistics of selected variables

	Mean	Median	Min	Max	SD	N
Prefvotes	**6.93**	**6.22**	**0.35**	**25.05**	**4.32**	**124**
Pledge = 1	6.74	6.14	0.35	25.05	4.35	95
Pledge = 0	7.55	6.31	0.76	15.94	4.23	29
Pastprefvotes	**7.63**	**6.02**	**1.07**	**38.17**	**5.71**	**124**
Pledge = 1	7.13	5.82	1.07	28.24	4.91	95
Pledge = 0	9.27	6.33	1.74	38.17	7.67	29
%opposed	**27.44**	**21.65**	**0.90**	**76.00**	**19.75**	**124**
Pledge = 1	24.52	19.00	0.90	67.60	18.42	95
Pledge = 0	37.00	44.60	2.90	76.00	21.24	29
Nbroken	**4.05**	**3.00**	**0.00**	**14.00**[3]	**4.23**	**95**
Pledge	**0.77**	**1.00**	**0.00**	**1.00**	**0.43**	**124**

Source: Author's own calculations based on data from Reconstruction of the State (n. d.) and Czech Statistical Office. The statistics apply to the sample of 124 observations described below Table 1 (deputies running in 2017, parties with at least 10 observations, no government members).

Prefvotes will be our dependent variable. Defined as the percentage of preferential votes won within a regional party list, it represents the criterion that determines whether the deputy moves to the top of the list. It is preferable to the number of preferential votes because it makes sense when compared between deputies on different party lists. Winning one thousand preferential votes means something very different to a deputy of the ANO party running in, say, the South Bohemi-

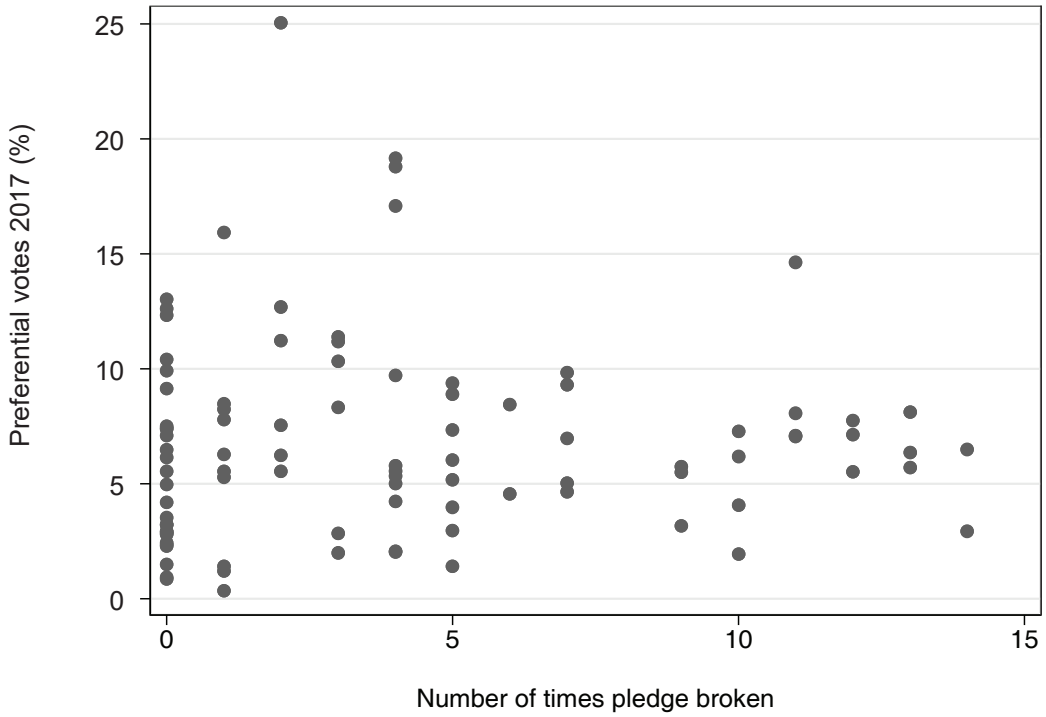
2 This is the case even though a comparison of the means of *pastprefvotes by pledge* indicates otherwise. The significant difference between the mean *pastprefvotes* of pledging and non-pledging deputies reflects the fact that the share of pledging deputies varies by party (from 42% among Civic Democrats to 90% among Christian Democrats) as do mean shares of preferential votes (from 4% in the ANO party to 17% for Civic Democrats). When party dummies are included in a regression of *pastprefvotes* on *pledge*, pledging does not appear statistically significant.

3 The maximum number of *nbroken* is higher than the number of bills (9) since some bills were voted on multiple times.

an region, and a Communist deputy running in the Capital City of Prague. Winning one percent of preferential votes, however, is of similar importance to both of them as it constitutes one fifth of what they need to move to the top of the list.

Multiple statistics that capture deputies' voting on anti-corruption bills and the associated pledge are available. The main indicator calculated and published by the project is the number of times a deputy broke the pledge (*nbroken*). Based on the existing literature, a negative relationship between *nbroken* and *prefvotes* is expected (H_1). Figure 1 plots the relationship between the two variables.

Figure 1: Scatter plot of *prefvotes* against *nbroken*



Source: Author's own elaboration

The variable *nbroken* is available only for deputies who pledged. In order to estimate the effect of breaking the pledge independently of the effect of opposing anti-corruption bills, a variable available for all deputies, whether or not they pledged, is needed: *%opposed*. Table 3 displays correlation coefficients between *pledge*, *nbroken*, and *%opposed*.

Table 3: Pairwise correlation coefficients

	Pledge	***Nbroken***	***%opposed***
Pledge	1 [124]		
Nbroken		1 [95]	
%opposed	-0.268 [124] (0.003)	0.959 [95] (0.000)	1 [124]

Note: Number of observations in [], *p*-value in (). Correlation between *pledge* and *nbroken* cannot be established as *nbroken* is available only for deputies who pledged.

Source: Author's own calculations

The variables *nbroken* and *%opposed* are closely related. Yet, while *%opposed* is calculated with regard to all roll call votes on the anti-corruption bills, *nbroken* concerns only the most important ones (as determined by the project's collaborating experts; usually the third reading). The project made pledging deputies aware of these important sessions ahead of time. The *nbroken* statistic was presented on the project's main website, while *%opposed* was available after clicking on a deputy's name or headshot.

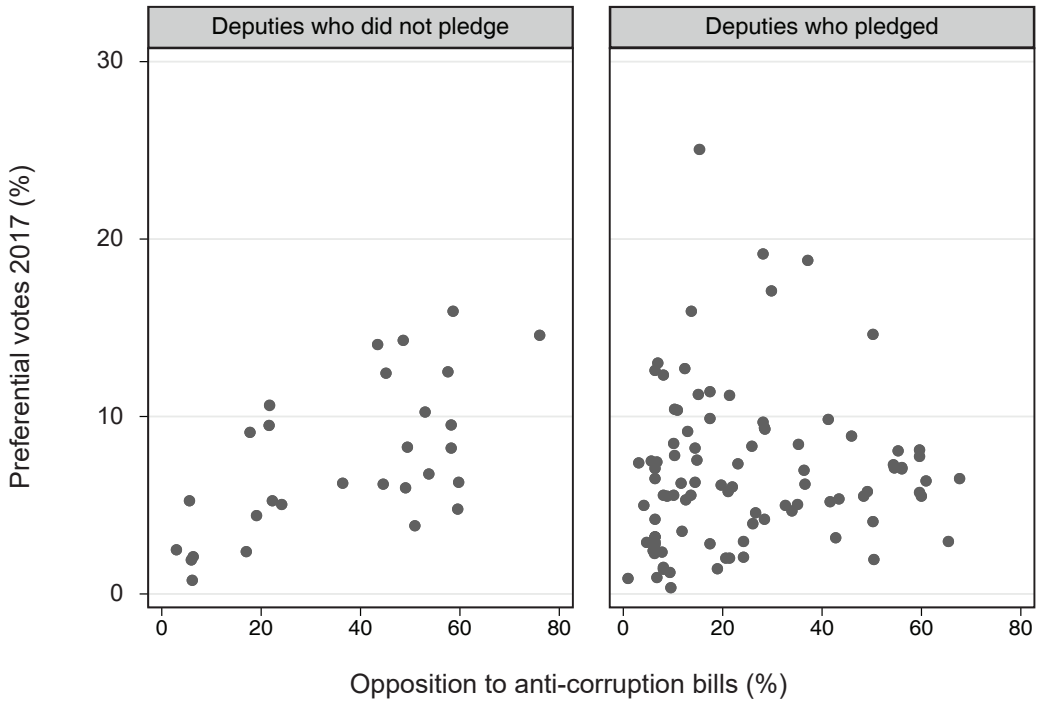
We expect voters who dislike corruption to also dislike deputies' opposition to anti-corruption bills. However, the electoral punishment for opposing anti-corruption bills need not be uniform across deputies. It is of interest to this paper whether it is conditioned by pledging (H_2).

Figure 2 plots the relationship between *prefvotes* and *%opposed* by values of *pledge*. We expect that a broken pledge will intensify the anticipated negative relationship between opposition to the bills and preferential votes (H_2), if retrospective voting occurs at all (H_1).

We test our hypotheses by means of OLS. To test H_1, we regress *prefvotes* on *nbroken* and a number of control variables (yet to be discussed) and run the estimation on the subgroup of pledging deputies. The coefficient estimate of *nbroken* and its statistical significance will provide a conclusion concerning H_1.

To test H_2, we regress *prefvotes* on *%opposed*, *pledge*, and their interaction, plus the control variables. The estimation sample consists of both pledging and non-pledging deputies. The coefficient estimate on the interaction term and its statistical significance will enable us to reach a conclusion concerning H_2.

Figure 2: Scatter plots of *prefvotes* against *%opposed* by *pledge*



Source: Author's own elaboration

The regression model with an interaction effect, which we use to test H_2, is appropriate specifically for determining whether the marginal effect of a variable differs between two subgroups; in our case, between deputies who had and had not pledged to support anti-corruption bills. It is, however, overly restrictive if we are interested in the relationship between two variables in the subgroup of pledging deputies, as we are in the case of H_1. With one interaction term, only the coefficient of the variable entering into the interaction (in our case, *%opposed*) is allowed to vary between the subgroups. Running a regression on pledging deputies only, by comparison, produces the best fit for that subgroup. This is why we refrain from deriving conclusions concerning H_1 from the interaction-term model constructed to test H_2.

Besides the main variables of interest, a number of control variables are part of the regressions to be estimated. Including past preferential votes won (*pastprefvotes*) is crucial, as it prevents deputies' stable characteristics from interfering with the effects of interest, should they be correlated with voting on anti-corruption bills or pledging. Also available are deputies' *age* and *sex.*

Higher visibility of party leaders may constrain the extent of pledge breakage. In our data, these deputies breached no more than four times, compared to the mean of 4 and the maximum of 14 in the rest of the sample. We construct a dummy variable *ptyleader*, which equals 1 for deputies who served as party leaders in 2013–2017.

Another variable we consider is list rank. In open- and flexible-list systems, candidates at the top of the ballot receive more preferential votes than candidates further down the list. The 2017 Czech Chamber of Deputies election was no exception.

Despite the fact that ballot position effects are empirically relevant and have a causal effect on election results (Blom-Hansen *et al*., 2021), Folke and Rickne (2020) refrained from including list-rank fixed effects as controls in their analysis of who wins preferential votes. They argued that the decision to control for list rank depends on the focus of the research question. If the overall allocation of preferential votes among candidates based on their behaviour is of interest, one should not include list-rank controls. More importantly, they cautioned that doing so may induce endogeneity. When assigning ballot ranks, parties consider past preferential votes (Folke *et al*., 2016). Since past and current preferential votes are correlated, list rank may be endogenous to current preferential votes.

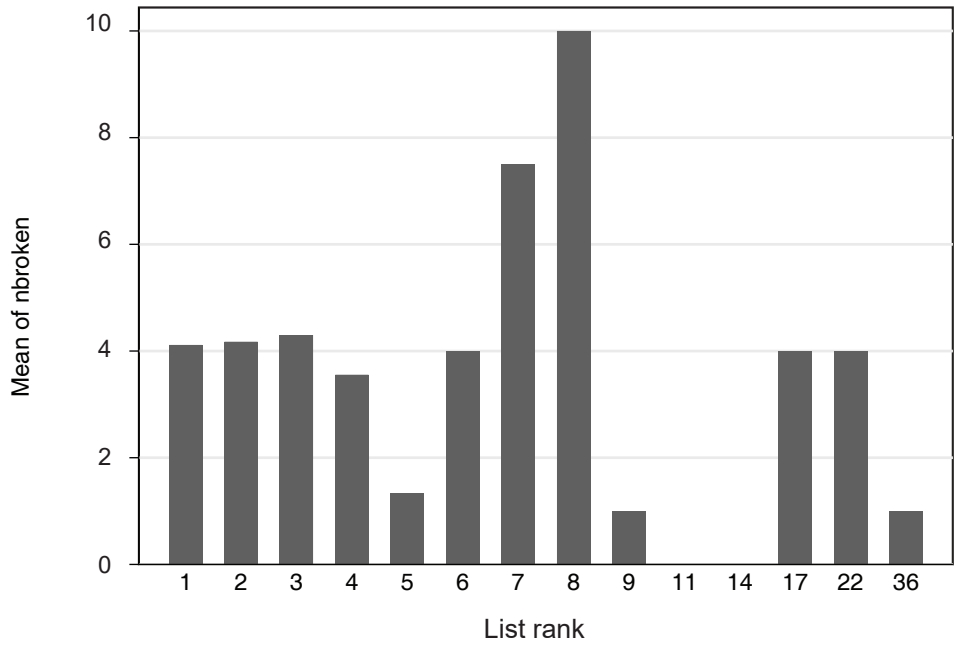
We are, however, concerned that not accounting for list rank may lead to an omitted variable bias. Parties might take politicians' pledge-related behaviour into account when deciding ballot positions, *e.g.*, they might refrain from placing pledge-breaking deputies in top positions, especially if they suspect that such behaviour is linked to preferential votes, and thus, by extension, party votes (as casting a preferential vote is only possible in conjunction with a party vote). If pledge-breaking during the term is linked to ballot positions in the next election and we omit list-rank controls, our coefficient of interest would absorb the effect of rank on preferential votes.

Figure 3 shows the mean value of *nbroken* by list rank. Starting with rank eight, the bars pertain to single observations, so extreme values are easy to get. *Nbroken* is similar across the top three ranks, with more variability appearing in positions 4–7. An omitted variable bias from failing to include *listrank* does not seem likely. Nonetheless, we report our results both without and with list rank fixed effects. In the latter case, we restrict our sample to observations with rank seven or better to avoid single observations per rank.

Finally, we consider including sets of dummy variables for *party* and *region*. The consequence of doing so is a substantial loss of degrees of freedom as there are six parties and fourteen regions. Including both *party* and *region* dummies essentially leads to a model that explains variation in preferential votes within individual regional party lists, a specification we consider overly restrictive.

The link between *prefvotes* and voting on anti-corruption bills appears to be a matter of party membership. Figure 4 plots *prefvotes* against *%opposed* by *party*. Party members tend to vote similarly. Preferential votes received, too, vary with party. Among the Christian or Civic Democrats, no candidates received less than 5% of preferential votes, whereas shares below this figure are common among the ANO members and the Social Democrats. Including party fixed effects in our regressions is therefore highly desirable. By comparison, scatter plots by *region* (not shown) look considerably more alike than the scatter plots by *party.* Preference is, therefore, given to including *party* dummies.

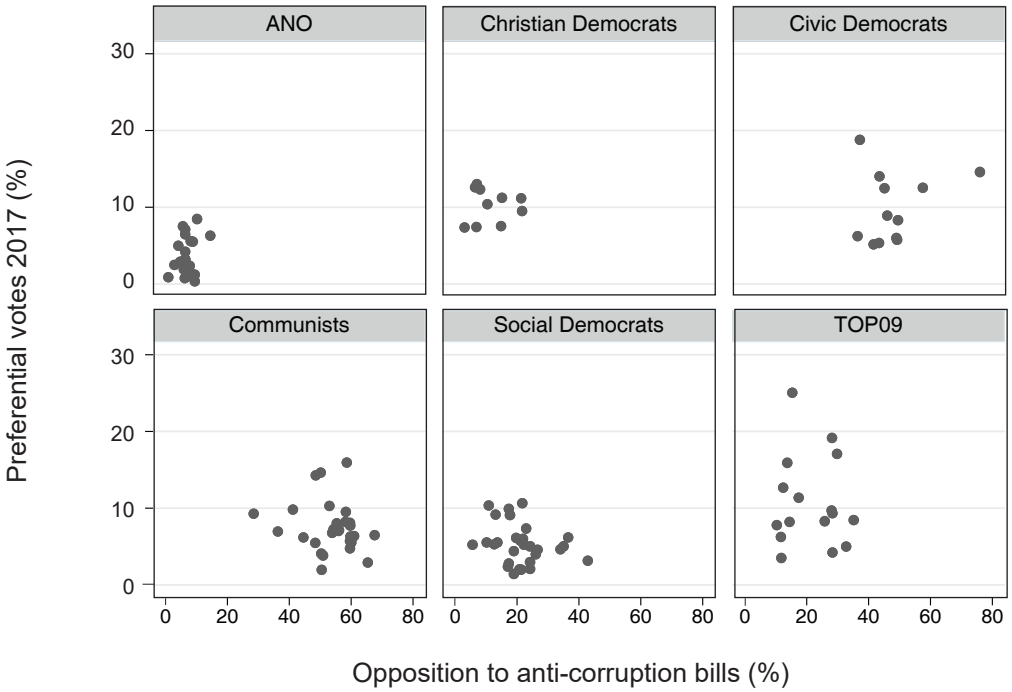
Figure 3: Mean pledge breakage by list rank



Note: Empty list-rank categories omitted.

Source: Author's own elaboration

Figure 4: Scatter plots of *prefvotes* and *%opposed* by *party*



Source: Author's own elaboration

One should only trust OLS estimates under certain assumptions. Notably, coefficient estimates are biased in the presence of endogeneity. We already discussed potential endogeneity related to list-rank fixed effects. There are two other issues to consider.

Voting behaviour *(%opposed, nbroken*) may be endogenous to preferential votes via deputies' expectations. Politicians who are confident they will win by a large margin may care less about pledge fulfilment. However, correctly predicting one's share of preferential votes is unlikely. Pre-election polls are aimed at party votes, not the preferential votes of individual politicians; and popularity polls are generally available for prominent politicians only. If deputies base their voting on expected preferential votes at all, chances are that some overestimate and some underestimate future preferential votes, thereby mitigating the endogeneity of voting to preferential votes.

Deputies' voting on anti-corruption bills may be correlated with how they vote on other issues. It is, however, nearly certain that the effects estimated in this paper pertain to deputies' voting on anti-corruption bills, given both the salience of corruption in the Czech society and the visibility of the Reconstruction of the State campaign. Data on deputies' voting are publicly available, but accessing them requires a level of IT proficiency that the majority of the electorate does not possess. Even so, the interpretation of whether a particular vote opposes or supports a certain goal is not straightforward. The proposals are formal, complicated, and can be formulated as supporting or opposing the goal. The project had collaborating legal experts evaluate whether voting against a particular proposal was in line with the pledge, and this information was then made available online to the public. Prior to the 2017 election, information about individual deputies' voting on anti-corruption bills was accessible to citizens far more easily and systematically than information about their voting on any other issue.

As vote choice is a complex decision and our dataset concerns individuals, it is expected that our data will often be less well-behaved than the OLS requires.

We use the residuals vs fitted plot and the Breusch-Pagan test (H_0: homoscedasticity) to assess variance of residuals. For *p*-values below or little above 0.1, we employ robust standard errors.

The normality of residuals is inspected visually with a kernel density plot and checked with the Shapiro-Wilk test (H_0: normality). One may relax the assumption of normality when the sample size is large enough because of the central limit theorem (Pek *et al*., 2018). Since how large is large enough depends on the extent of the departure from normality and the number of independent variables, where we succeed in identifying the cause of non-normality, estimations on altered samples are run to ensure reliability of reported *p*-values.

Variance inflation factors (VIFs) are used to identify variables affected by multicollinearity. As a rule of thumb, we deem values greater than 5 suspicious and values above 10 troublesome. Since multicollinearity only affects the variables that are correlated, steps to mitigate it are taken under two conditions: when it concerns our variables of interest (*nbroken*, *%opposed*, *pledge, %opposed* × *pledge*), and when the *p*-value associated with our variable of interest is greater

than 0.1. This means that we do not address multicollinearity of control variables (although we do report the suspicious VIF) and of variables of interest which appear at least mildly statistically significant (p-value <0.1). We rest knowing that, in the latter case, the effects would be at least as significant if the multicollinearity was resolved. For models with an interaction term, and hence unavoidable structural multicollinearity, we report Wald test p-values so that joint statistical significance of the main term and its interaction can be assessed.

4. Results

Table 4 presents results that speak to H_1. Specification 1 estimates the effect of the number of instances in which the pledge was broken on deputies' preferential vote shares, controlling for past preferential votes, party leader position, age, sex and party membership. Specification 2 also controls for list rank.

A single outlier is responsible for the non-normality of residuals in both specifications. It is a deputy from the TOP 09, who would become the party leader in 2019. Yet, even after studying the deputy's career in detail, we cannot justify excluding the single observation. We can, however, argue that the TOP 09 party is a special case as its every member pledged to support anti-corruption bills.

Specifications 3 and 4 are run on a sample that excludes TOP 09 deputies, and hence is more similar to the estimation sample used to test H_2, from which TOP 09 deputies are omitted for the same reason, *i.e.*, no variability with respect to pledging. Reducing multicollinearity in Specification 4 would require excluding 19 Communist observations[4] from the sample, a step we do not take given that the p-value of the *nbroken* coefficient estimate is just slightly above 0.1.

Overall, the results in Table 4 provide some, albeit in terms of statistical significance mild, support to hypothesis H_1. They suggest that deputies who break the pledge more frequently receive on average smaller shares of preferential votes than their less-breaching colleagues. This applies when we consider pledging incumbents of similar age, sex, party position (leader vs ordinary member) and past election results, as well as of similar list rank. The magnitude of the effect is negligible: between about −0.1 and −0.2 percentage point per instance of pledge breakage. This means that the difference in preferential vote shares between a never-breaching (*nbroken* = 0) and a fully-breaching (*nbroken* = 14) deputy would amount to some 1.4 to 2.8 percentage points of preferential votes.

Table 5 presents estimation results pertaining to hypothesis H_2. Specification 5 includes the main effects of *%opposed* and *pledge*, their interaction, and control variables. Specification 6 adds *listrank.* In both of them, the VIF of *%opposed* exceeds 10. In Specifications 7 and 8, Communist deputies are excluded from the estimation sample to mitigate multicollinearity.

4 Communist deputies have by far the highest mean value of *nbroken* (11) among all the parties. It is the result of the highest mean *%opposed* (54) in combination with 66% of party members pledging. Civic Democrats have similar mean *%opposed* (48), but with 42% of their members pledging, their mean *nbroken* is 5, the second highest in the sample.

Table 4: OLS results (H_1)

	Dependent variable: *prefvotes*			
	(1)	**(2)**	**(3)**	**(4)**
Nbroken	−0.158 * [0.095] (0.099)	−0.133 * [0.078] (0.093)	−0.199 * [0.104] (0.058)	−0.154 [0.093] (0.105)
Pastprefvotes	0.580 *** [0.072] (0.000)	0.511 *** [0.072] (0.000)	0.607 *** [0.069] (0.000)	0.546 *** [0.065] (0.000)
Ptyleader	5.557 *** [1.842] (0.003)	5.417 *** [1.450] (0.000)	1.848 [2.209] (0.406)	2.287 [1.950] (0.246)
Age	−0.070 ** [0.031] (0.027)	−0.070 ** [0.029] (0.019)	−0.052 ** [0.023] (0.025)	−0.051 ** [0.024] (0.035)
Sex	−0.230 [0.601] (0.703)	−0.609 [0.538] (0.261)	−0.194 [0.532] (0.717)	−0.323 [0.491] (0.512)
Constant	5.058 *** [1.905] (0.009)	6.854 *** [1.744] (0.000)	4.008 *** [1.325] (0.003)	5.531 *** [1.411] (0.000)
R^2 (Adjusted R^2)	0.809	0.876	0.796 (0.769)	0.855 (0.817)
F-test *p*-value	0.000	0.000	0.000	0.000
Number of observations	95	88	79	73
Wald test *p*-values (joint sig.)				
Party	0.000	0.000	0.000	0.001
List rank		0.001		0.053
OLS assumptions				
Breusch-Pagan test *p*-value			0.778	0.586
Shapiro-Wilk test *p*-value	0.014	0.002	0.385	0.451
Variance inflation factors				
Nbroken	5.8	6.1	6.2	6.5
Communists (party dummy)	7.8	8.6	8.1	8.9

Note: Standard errors in [], *p*-values in (). Single, double and triple asterisk indicate statistical significance at the 10%, 5% and 1% levels, respectively. The estimation sample consists of deputies who pledged to support anti-corruption bills; government members are always excluded. Specifications 3 and 4 exclude TOP 09 members. *Party* dummies are included in all specifications, *listrank* in Specifications 2 and 4 (see variables listed under joint significance *p*-values). Specifications with *listrank* only include rank 7 or better. Specifications 1 and 2 are estimated with robust standard errors. The Breusch-Pagan test *p*-values and adjusted R^2 are available only for estimations without robust standard errors.

Source: Author's own calculations

Table 5: OLS results (H_2)

	Dependent variable: *prefvotes*			
	(5)	**(6)**	**(7)**	**(8)**
%opposed	0.032 [0.026] (0.230)	0.019 [0.025] (0.448)	0.066 * [0.035] (0.065)	0.068 ** [0.030] (0.025)
Pledge	0.835 [0.784] (0.290)	−0.018 [0.585] (0.975)	1.517 * [0.880] (0.089)	0.529 [0.847] (0.535)
%opposed × pledge	−0.039 ** [0.019] (0.043)	−0.015 [0.016] (0.335)	−0.089 *** [0.033] (0.009)	−0.063 ** [0.029] (0.036)
Pastprefvotes	0.490 *** [0.096] (0.000)	0.458 *** [0.088] (0.000)	0.397 *** [0.056] (0.000)	0.354 *** [0.050] (0.000)
Ptyleader	4.192 *** [1.188] (0.001)	3.721 *** [1.003] (0.000)	6.248 *** [2.219] (0.006)	5.901 *** [1.845] (0.002)
Age	−0.061 ** [0.024] (0.014)	−0.052 ** [0.021] (0.015)	−0.085 *** [0.026] (0.002)	−0.058 ** [0.024] (0.020)
Sex	0.079 [0.547] (0.886)	−0.056 [0.481] (0.907)	−0.399 [0.613] (0.517)	−0.142 [0.557] (0.799)
Constant	3.840 ** [1.518] (0.013)	6.118 *** [1.231] (0.000)	5.385 *** [1.595] (0.001)	6.671 *** [1.457] (0.000)
R^2 (Adjusted R^2)	0.785	0.852	0.800 (0.770)	0.073 (0.836)
F-test *p*-value	0.000	0.000	0.000	0.000
Number of observations	108	100	79	72
Wald test *p*-values (joint sig.)				
%opposed, interaction	0.129	0.603	0.025	0.038
Pledge, interaction	0.038	0.177	0.025	0.027
Party	0.000	0.000	0.000	0.000
List rank		0.000		0.003
Marginal effects (%opposed)				
Non-pledging deputies	0.032 (0.230)	0.019 (0.448)	0.066 * (0.065)	0.068 ** (0.025)
Pledging deputies	−0.008 (0.731)	0.004 (0.864)	−0.023 (0.512)	0.005 (0.865)
OLS assumptions				
Breusch-Pagan test *p*-value			0.319	0.704
Shapiro-Wilk test *p*-value	0.212	0.291	0.696	0.599
Variance inflation factors				
%opposed	10.6	10.7	6.7	6.8
%opposed × pledge	5.5	6.4	4.1	4.6
Pledge	4.0	5.3	3.3	4.1
Civic Democrats (party dummy)	6.1	6.5	7.5	8.4
Communists (party dummy)	11.1	11.9		

Note: Standard errors in [], *p*-values in (). Single, double and triple asterisk indicate statistical significance at the 10%, 5% and 1% levels, respectively. Government members and TOP 09 deputies are excluded from all specifications (all TOP 09 deputies pledged, so the effect of TOP 09 membership cannot be distinguished from pledging). Communist deputies are excluded from Specifications 6 and 8. Party dummies are included in all specifications, *listrank* in Specifications 7 and 8 (see variables listed under joint significance *p*-values). Specifications with *listrank* only include rank 7 or better. Specifications 5 and 7 are estimated with robust standard errors. The Breusch-Pagan test *p*-values and adjusted R^2 are available only for estimations without robust standard errors.

Source: Author's own calculations

Overall, Table 5 suggests that voters respond differently to deputies opposing anti-corruption legislation depending on whether they have pledged to do so. Deputies who have pledged see their preferential vote shares fall at a higher rate. This particular conclusion follows from the negative and statistically significant coefficient estimate on the interaction term *%opposed × pledge.*

In Specifications 6 and 8, we observe a positive marginal effect of *%opposed* for deputies who did not pledge. Because regression with a single interaction effect is not particularly informative when it comes to subgroup-specific relationships, we check this result by running separate regressions for non-pledging deputies (not shown). The regressions are similar to Specifications 1 and 2 in Table 4, with *%opposed* replacing *nbroken* as the explanatory variable of interest. The coefficient estimate revolves around a positive 0.1, nearly twice the size of the marginal effect in Table 5, and it is statistically significant (p-values 0.084 and 0.049). We revisit this finding in the discussion.

What is the threshold value of *%opposed*, beyond which pledging deputies would be better off had they not pledged? We divide the coefficient estimate on pledge, *i.e.*, the pledge bonus at hypothetical 0% opposition to the bills, by the absolute value of the coefficient of the interaction term; and then check the distribution of *%opposed* in the specific sample used for estimation to determine the corresponding percentile. We only use specifications where *pledge* is jointly significant in this exercise.

Deputies of similar characteristics other than list rank benefit from having pledged as long as they remain within the less bill-opposing half of the sample (*i.e.*, below the 50th and 55th percentile according to Specifications 5 and 7). When a deputy's voting record places them into the more opposing half of the sample, they would have been better off had they not pledged. The criterion tightens once we consider deputies who are also comparable in terms of list rank. In such a case, the deputy has to remain within the least-opposing third of the sample (33rd percentile according to Specification 8).

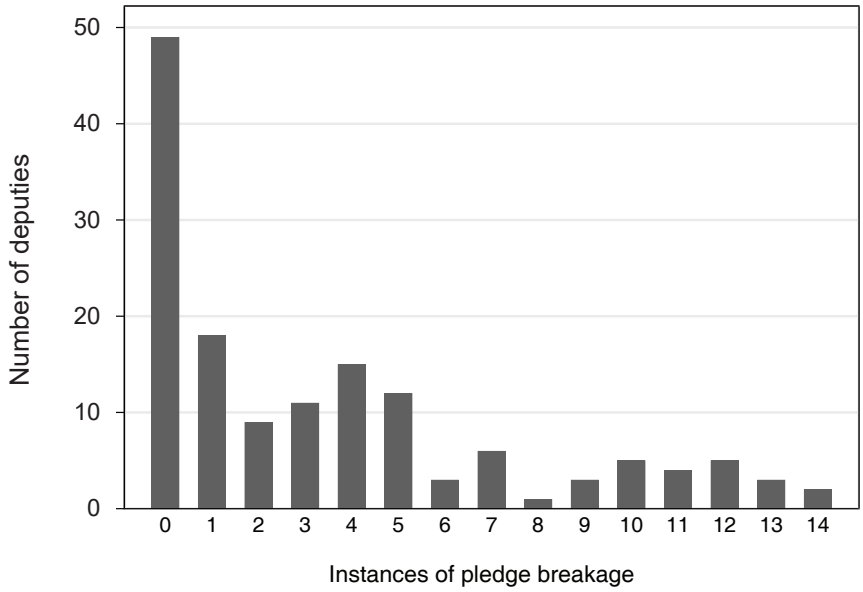
In addition to the reported specifications, we ran alternative estimations that allowed for non-linearities in the relationship between politicians' behaviour (voting, pledging) and preferential votes. These included square terms, dummies for top and bottom quartiles, and a full set of fixed effects for pledge breakage instances. The results were no improvement over the reported linear estimates, and were less robust to changes in control variables.

5. Discussion

The literature suggests that politicians – political parties, more precisely – keep their election pledges to a greater extent than the public gives them credit for. We have no data on the public's evaluation of Czech deputies' pledge fulfilment, but we can report that out of the 146 deputies

who had pledged to support anti-corruption legislation prior to the 2013 election and won a seat in the Chamber (and may or may not have run again in 2017), 48 did not break their pledge even once, and half breached once or twice. Figure 5 shows that the distribution of pledge breakage is skewed in favour of fewer breaching instances. It is up to the reader to compare these numbers with their own intuition about the rate of politicians' pledge fulfilment.

Figure 5: Distribution of pledge breakage (*nbroken*)



Note: Sample of 146 deputies who pledged to support anti-corruption bills and held office in 2013–2017, regardless of whether they ran for re-election in 2017. Government members are included.

Source: Author's own elaboration

As expected on grounds of research on political parties' pledges, we, too, find that deputies who break their pledge to support anti-corruption legislation receive on average smaller shares of preferential votes than those who fulfil their pledge to a greater extent. Voters' punishment of politicians' pledge breaking thus occurs not only at the level of political parties, governing and opposition, but also at the level of individual politicians. As with all cases of retrospective voting, it is helpful to keep in mind that this occurrence is conditional. In this particular case, the Reconstruction of the State project made access to the information about deputies' pledge breaking extremely simple.

Only pledging deputies are "punished" for voting against anti-corruption bills. This suggests that the cause of the punishment is not deputies' voting: it is the failed pledge. In fact, preferential vote shares of non-pledging deputies rise with increasing opposition to anti-corruption bills.

What is the likely mechanism through which pledge affects voters' response to deputies' voting behaviour? Between increased salience of the pledged behaviour (Johnson and Ryu, 2010) and citizens' disappointment (Guinaudeau and Persico, 2018), our results support the latter. While increased salience would allow a stronger reaction to bill opposition among pledging deputies, it does not explain the different direction of effect between pledging and non-pledging deputies.

There appears to be some room for breaching before voters' punishment kicks in to the extent that the deputy would be better off without the pledge. Combining the results of models in Table 5 with descriptive statistics on the instances of pledge breakage, a deputy can breach up to three or four times before they are made worse off by the pledge. This room may exist because voters give politicians some benefit of doubt when breaching instances are rare, pledge breakage is not at the top of the voters' agenda at the ballot box or simply does not matter to some parts of the electorate.

In light of the positive and significant effect of opposition to anti-corruption bills on preferential vote shares among non-pledging deputies, we ought to correct the simplistic assumption we made prior to our empirical analysis, that citizens, in general, dislike corruption, and by extension should disapprove of politicians voting against anti-corruption bills. Low levels of corruption may be acceptable to, or even desired by, certain parts of the electorate. Vuković (2020) found that low shares of suspicious procurement increase the probability of re-election of Croatian mayors as a result of rent-extracting relationships between firms and politicians. The probability of re-election begins to decline at 20% of procurement funds allocated suspiciously and mayors lose office when suspicious allocations exceed 50%. We add that even when citizens are not part of rent-extracting relationships, they may deem some level of corruption preferable to the increased regulation and bureaucracy associated with anti-corruption measures.

6. Conclusion

This paper investigated retrospective voting on pledges at the level of individual politicians. The case studied was Czech deputies' voting on proposals of anti-corruption bills between two general elections. Prior to the first election, in 2013, a large number of candidates pledged to support these bills if elected.

We found that individual politicians are subject to retrospective voting on elections pledges. Deputies who break the pledge more frequently during the term win smaller shares of preferential votes in the next election than deputies who breach less often. We find it encouraging that individual deputies are not exempt from voters' response when information about their pledge fulfilment is easily accessible. The loss of preferential votes we observe for breaching deputies stems from pledge breakage, not from opposing anti-corruption bills, as deputies who do not pledge even see their preferential votes rise with higher opposition. Depending on the model, having pledged pays

off as long as the deputy remains within the less-opposing half or least-opposing third of the estimation sample.

Our results contribute to the literature by showing that retrospective pledge voting can indeed occur even at the level of individual politicians – in addition to, as previously observed by other authors, governments and political parties, both governing and opposition. Furthermore, we resolved an issue that was not clear from the existing studies: when voters "punish" governments or parties for unfulfilled pledges, are they reacting to the breach of trust (from the unfulfilled pledge) or to the content of the pledge? Our estimates suggest that it is indeed the broken pledge, not the associated policy outcome or legislative output that matters in retrospective pledge voting.

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