Cabinet Composition and Attribution of Responsibility in Presidential Multiparty Systems[*]

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Abstract

An extensive literature on accountability in parliamentary systems has focused on the effect that cabinet type (i.e., single-party or coalition) has on the electoral fate of governments based on their economic performance. In presidential systems, studies on government accountability have focused on the impact of having unified or divided government on the electoral success of the president's party. Neither of these literatures has studied how variations in cabinet composition can affect governments' accountability. In this paper, we examine how different cabinets appointed by presidents can affect presidents' accountability for economic outcomes. Building on the theory of clarity of responsibility, we argue that when the ideological dispersion of the government cabinet is minimal voters will be more likely to punish the president for bad economic outcomes. In turn, when the ideological dispersion of the cabinet is larger, voters will be less likely to punish the president for bad economic outcomes. To test these hypotheses, we combine aggregate-level data on cabinet composition and survey data from 12 Latin American countries. Our preliminary results provide evidence in favor of our argument.

Key Words: Clarity of Responsibility, Economic Voting, Presidential Systems, Coalitions, Cabinet Composition.

Introduction

Government accountability implies that citizens are able to identify responsibility for policy outcomes, such as economic performance, and that they sanction or reward politicians in return (Key Jr., 1966; Fiorina, 1981; Shugart and Carey, 1992). The ability of citizens to hold elected officials accountable varies due to institutional arrangements that either clarify or obscure lines of responsibility for political outcomes (Powell and Whitten, 1993; Whitten and Palmer, 1999; Anderson, 2000). In particular, in contexts where institutions unify policy-making authority in a few actors, responsibility for policy outcomes becomes clearer, and citizens are more likely to punish governments for bad economic performance.

An extensive literature in parliamentary systems has examined the effect that the type of cabinet (i.e., single-party vs coalition) and its combination with other institutional variables (e.g., majority support for the prime minister, the distribution of administrative responsibility within the cabinet, and the cohesiveness of legislative parties) has on the electoral fate of governments based on how voters are able to assign responsibility for government's economic performance (Lewis-Beck, 1986; Powell and Whitten, 1993; Anderson, 1995; Whitten and Palmer, 1999; Duch and Stevenson, 2008; Fisher and Hobolt, 2010). Among the studies on government accountability in presidential systems, scholars found that responsibility judgments are shaped by economic ideology and institutional context (Rudolph, 2003; Johnson and Schwindt-Bayer, 2009), concurrent elections (Samuels, 2004b), global market integration (Hellwig and Samuels, 2007), and by electoral rules present in presidential systems (Valdini and Lewis-Beck, 2018). However, none of the studies in either presidential or parliamentary systems have examined how cabinet composition might affect government's accountability and clarity of responsibility.

The particular question on how variations in presidential cabinet composition might mediate the effect of economic evaluations on citizens' vote choice is still unanswered. Given that more than half of all governments formed in presidential regimes are coalition governments (Deheza, 1998; Lanzaro, 2001; Chasquetti, 2001; Figueiredo, Salles, and Vieira, 2009),

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with a high variation in their composition (e.g., in terms of the amount of partisan ministers and in the proportionality of portfolios allocated among the members of the coalition) (Amorim Neto, 2006a; Martínez-Gallardo and Schleiter, 2015; Silva, 2016; Batista, 2017), there is a clear contribution to be made in this topic. In this study, we seek to fill this gap in the literature by examining how differences in cabinet composition affect citizens' attribution of responsibility for economic outcomes and their subsequent voting decision.

Building on the theory of clarity of responsibility, we argue that when presidents appoint ministers close to their ideological preferences, clarity of responsibility for policy outcomes is higher, as policy-making authority is unified in a set of actors with similar preferences. We hypothesize that citizens are more likely to punish presidents for bad economic outcomes in these contexts. In order to test our hypothesis, we combine aggregate-level data on cabinet composition and on a series of institutional and economic variables, with survey data from the *Latinobarometer* for a set of 12 Latin American countries, ranging from 2002 to 2012.[1] The results obtained from our empirical tests conducting multilevel models provide initial evidence in favor of our argument. We find that voters who have worse evaluations of the economy are more likely to hold presidents to account for this bad performance. However, this is mediated by cabinet composition, as once the ideological range of the cabinet is taken into account, the effect of economic evaluations on presidential punishment becomes moderated. As a result, presidents heading more heterogeneous cabinets are held accountable to a lesser extent than those who head more ideologically homogeneous ones.

In the next section, we discuss related research on the attribution of responsibility in presidential systems, and present our contributions. In section 3, we present our theory and hypotheses built on the theory of clarity of responsibility. In section 4, we present our data and discuss our empirical strategy. In section 5, we present and interpret our results. The implications of our findings and avenues for future research are discussed in Section 6.

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[1]The countries and years included in this study can be found in Table A.1 of the Appendix.

2   Attribution of responsibility in presidential systems

By testing the economic voting hypothesis in parliamentary systems, Powell and Whitten (1993) first developed the argument that the effect of policy outcomes (particularly, economic performance) on government accountability is mediated by the clarity of responsibility provided by the institutional context. The ability of citizens to hold elected officials accountable varies across different political and institutional contexts as a function of the degree of clarity of responsibility. In order to measure the clarity of responsibility, Powell and Whitten (1993) created an index of factors that affect voters' capacity to assign responsibility for policy outcomes and to punish or reward governments accordingly. This measurement captures the degree to which policymaking authority is unified or diffused within countries' decision-making processes. When institutions unify this authority, responsibility for policy outcomes becomes clearer to citizens and they are more likely to punish incumbent governments for bad policy outcomes, such as for high rates of inflation and unemployment.

The empirical evidence, which has mainly focused on parliamentary systems in developed democracies, has supported this argument (Anderson, 1995, 2000; Whitten and Palmer, 1999; Nadeau, Niemi, and Yoshinaka, 2002; Bengtsson, 2004; Lewis-Beck and Stegmaier, 2007; Hobolt and Banducci, 2013, for a review of the existing literature on the topic see Silva and Whitten, 2017). Moreover, studies have found that the size of the cabinet (i.e., the number of government parties) has a significant effect on government accountability, as voters are less likely to penalize incumbent governments that are made up of multiple parties (Anderson, 1995; Whitten and Palmer, 1999; Lewis-Beck, 1990; Fisher and Hobolt, 2010). The most consistent finding among these studies is that coalition governments make it harder for citizens to distinguish who is responsible for policy outcomes, weakening the relationship between evaluations of government performance and vote choice.

In presidential democracies, it should be clearer to voters that the president is the main actor responsible for policymaking under unified government—i.e., when the president's party controls a majority in the legislature. Conversely, the clarity of responsibility for policy

outcomes becomes less clear in divided governments—i.e., when the control of the executive branch and the legislative branch is split between two parties. The lower level of clarity ensues from the fact that under divided governments, the president can blame the lack of support from the legislature as the root of poor economic performance (Leyden and Borrelli, 1995; Samuels, 2004b; Johnson and Schwindt-Bayer, 2009; Gélineau and Singer, 2015).

Accordingly, studies in presidential systems stress the importance of the balance of power among parties in the legislature and focus on the legislative support for the president's party to understand the effect of the economy on government accountability. The finding from these studies is in line with the clarity of responsibility theory: president's accountability for economic performance is greater under unified government. This means that when both branches of government are controlled by the same party, citizens are more likely to support the president if economic conditions are good (Leyden and Borrelli, 1995; Johnson and Schwindt-Bayer, 2009) and to punish the president if economic conditions are bad (Samuels, 2004a; Gélineau and Singer, 2015). In turn, when the legislature and the presidency are controlled by different parties, presidents are assigned less blame for economic conditions in comparison to Congress (Rudolph, 2003; Nicholson, Segura, and Woods, 2002)[2].

More recent studies have examined the association of other variables to the clarity of responsibility, such as concurrent elections (Hellwig and Samuels, 2007; Valdini and Lewis-Beck, 2018), the distribution of policymaking responsibilities between government branches (Renno and Gramacho, 2010), the legislative powers of the president (Carlin and Singh, 2015), the ties that national markets have to international ones (Gélineau and Singer, 2015), and the electoral rules and the term limits of presidents (Valdini and Lewis-Beck, 2018).The above discussed literature, however, has overlooked the composition of the cabinet as an independent variable capable of affecting clarity of responsibility, either by itself or in interaction with other institutional variables.

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[2]It is important to mention that some studies focused on the United States find inconsistent results of the effect of divided government on the president's accountability for economic outcomes (e.g., Nadeau and Lewis-Beck, 2001; Norpoth, 2001)

Our goal in this study is therefore to fill this important gap in the literature on clarity of responsibility. In addition, by focusing our analysis on presidential democracies we aim to give two further main contributions with this study. First, we aim to bridge the gap between the literature on government accountability and the literature on cabinet composition. The existing studies on cabinet composition in presidential multiparty regimes (Amorim Neto, 2006a,b; Alemán and Tsebelis, 2011; Martínez-Gallardo, 2012; Martínez-Gallardo and Schleiter, 2015) have focused on the executive-legislative relationship and the government's legislative support, without examining its impact on the relationship between the president and voters, and therefore on government accountability. These studies' main findings are that presidents endowed with extensive constitutional powers (such as decree and veto powers) will staff their multiparty cabinets with more cronies and technocrats (making the cabinet less partisan) (Amorim Neto, 2006a,b; Martínez-Gallardo and Schleiter, 2015) and will distribute portfolios on a less proportional basis among governing parties (Amorim Neto, 2006a,b). In addition, when the Congress is institutionally powerful vis-à-vis the executive, presidents will be more likely to form coalition governments (Amorim Neto, 2006a,b), and the probability of coalition formation would be a function of the legislative parties' ideological distance from the president's party (Cheibub, Przeworski, and Saiegh, 2004). Second, this study is a clear contribution to recent efforts in understanding the types of institutions that affect economic voting and clarity of responsibility in presidential systems (Silva and Whitten, 2017; Valdini and Lewis-Beck, 2018).

3   Theoretical Expectations

Our theoretical argument builds on the theory of clarity of responsibility described above and departs from existent studies that concentrate on parliamentary systems and only focus on the effect that the type of cabinet (i.e., single-party or coalition) has on government accountability. We argue that different cabinet compositions can lead to varying levels of

clarity of responsibility. When presidents are able to appoint ministers according to their preferences,the ideological dispersion among the parties that comprise the cabinet will be minimal (i.e., low ideological heterogeneity), as it will be mainly composed of ministers close to the president's ideological preferences.[3] In this context, policy-making authority is unified, responsibility for outcomes becomes clearer to citizens and they are more likely to attribute responsibility and punish governments for bad economic performance. This should hold both for single-party and coalition cabinets.

Otherwise, when presidents are unable to appoint ministers according to their preferences and close to their ideological leaning, the cabinet is likely to be composed of more ideologically heterogeneous parties.[4] In this context, policy-making authority is dispersed among actors with different preferences and, as clarity of responsibility is lower, citizens should be less likely to punish presidents for bad policy outcomes. Thus, according to our theoretical contribution, the difference in the degree of clarity of responsibility should not be solely based on a distinction between single-party and coalition cabinets, but rather on the composition of the cabinet and, particularly, on the level of ideological dispersion within the cabinet that the president heads.

Our theory entails the assumption that voters in Latin America react to past events in their voting behavior. We believe this is a valid assumption to make since single-country and cross-national studies in Latin America have found that voters tend to hold politicians accountable for economic outcomes (Seligson and B., 1989; Remmer, 1991; Dominguéz and McCan, 1995; Poiré, 1999; Remmer and Gélineau, 2003; Benton, 2005; Johnson and Ryu, 2010; Singer, 2013; Lewis-Beck and Ratto, 2013; Gélineau and Singer, 2015; Nadeau et al., 2017). In a region with frequent economic crises and high levels of economic vulnerability,

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[3]This composition of the cabinet could be the result of the fact that the president is able to govern mostly through unilateral executive prerogatives and does not need to negotiate the legislative support of other parties by giving them cabinet portfolios (Amorim Neto, 2006a), or when she is able to secure legislative support of parties through the distribution of pork (Raile et al., 2010).

[4]This could happen when the president needs the legislative support of other parties for the enactment of his legislative agenda, either because he does not possess strong legislative powers or because the seats won by his party alone do not provide him a majority in the legislature. In addition, it could be the result of pre-electoral arrangements with other parties.

elections generally focus on economic policy, and the retrospective evaluation of the economy makes a difference for the incumbents' vote (Singer, 2013). As Gélineau and Singer (2015, p. 294) claim, there's little "doubt that voters in Latin America respond to the economy in ways that are quite comparable to those of other regions[. . .] [O]ur analysis demonstrates that Latin America's votes are conditionally shaped by the economy in ways that suggest that voters judiciously evaluate the incumbent and carefully weigh their electoral alternatives."

Building on the main empirical implication derived from the economic voting theory, we expect that,

Hypothesis 1: The worse voters retrospectively evaluate the economy, the more likely they will be to punish the president for bad economic outcomes.

According to our argument, to the extent that governments are held accountable for economic outcomes, presidents not able to appoint ministers aligned with their preferences (i.e., presidents heading more ideologically heterogeneous cabinets) are less likely to be punished for bad outcomes. In turn, we posit that if governments are held accountable for economic outcomes, presidents heading homogeneous cabinets (either single-party or coalition cabinets) should be more likely to get punished for bad economic outcomes. The following empirical implication can be derived from our theory:

Hypothesis 2: The worse voters retrospectively evaluate the economy and the greater the ideological dispersion within the president's cabinet, the less likely voters will be to punish the president for bad economic outcomes.

4 Empirical Strategy

In order to test our main theoretical expectation, we combine aggregate-level data on cabinet composition with survey data from 12 Latin American countries from 2002 to 2011.[5] Examining our theory in the Latin American context is relevant for several reasons besides

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[5]The reason for limiting our analysis to this time span is that information for creating our dependent variable is available beginning in 2002. The year 2007 is excluded from the analysis because there's no data for one of our main independent variables, retrospective evaluations of the economy.

data availability. Almost all Latin American democracies are presidential systems. Between 1979 and 2011, of all governments formed in the region 74.6% of them were coalition governments (Figueiredo, Canello, and Vieira, 2012, p. 848), with presidential cabinets comprising a high variance on ideological heterogeneity (Power and Zucco Jr., 2009; Gaylord and Rennó, 2015; Freudenreich, 2016). There is also an already well-documented body of research on how essential coalition formation is for Latin American presidents to govern (Deheza, 1998; Lanzaro, 2001; Cheibub, 2007; Figueiredo, Salles, and Vieira, 2009; Alemán and Tsebelis, 2011).

Our dataset includes all Latin American countries for which we were able to obtain data on the ideological composition of the cabinet: Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, El Salvador, Honduras, Nicaragua, Panama, and Uruguay. This data on the ideological range of cabinets comes from Freudenreich (2016). Following Freudenreich's (2016) definition, a new cabinet is formed when a new president takes office, when a party decides to leave or enter the cabinet, and when legislative or presidential elections are held.

We merged this information on cabinet composition with survey data from the *Latinobarometer*, which has been collecting cross-national public opinion data in the region since 1995.[6] To combine both datasets, we matched cabinet data with information from the closest wave of the *Latinobarometer* survey.[7] The resulting dataset has over 40,000 observations.[8]

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[6]Latinobarometer(`http://www.latinobarometro.org/latContents.jsp`).

[7]For example, if a new cabinet in a country was formed during the last months of 1999, we matched this cabinet with the 2000 wave of the survey because it was fielded during the first months of the year and was closest in time to the cabinet than the 1999 wave. We repeated this procedure for each country and year. In addition, in cases where there were multiple cabinets during the same year in a country, we only considered information from the cabinet that was in place during the months closest to the time in which the wave of the survey was fielded.

[8]Table A.1 in the Appendix presents the countries and survey years included in our dataset. Table A.2 in the Appendix gives information on the number of respondents we have in our dataset by country.

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4.1 Dependent Variable: President Punishment

The main dependent variable in the analysis is *government punishment*, which was created based on the following *Latinobarometer* question: "Do you approve or disapprove the way the President is running the country?" Respondents can answer "approve", "disapprove" or "don't know" to this question. For the purposes of our analysis we coded respondents who answered that they "approved" as a 0, those that answered "disapprove" as a 1 (i.e. government punishment), and those who answered "don't know" as missing. As depicted in Figure 1, there is variation in government punishment across countries and within countries over time.

Figure 1: Presidential Punishment in Latin America
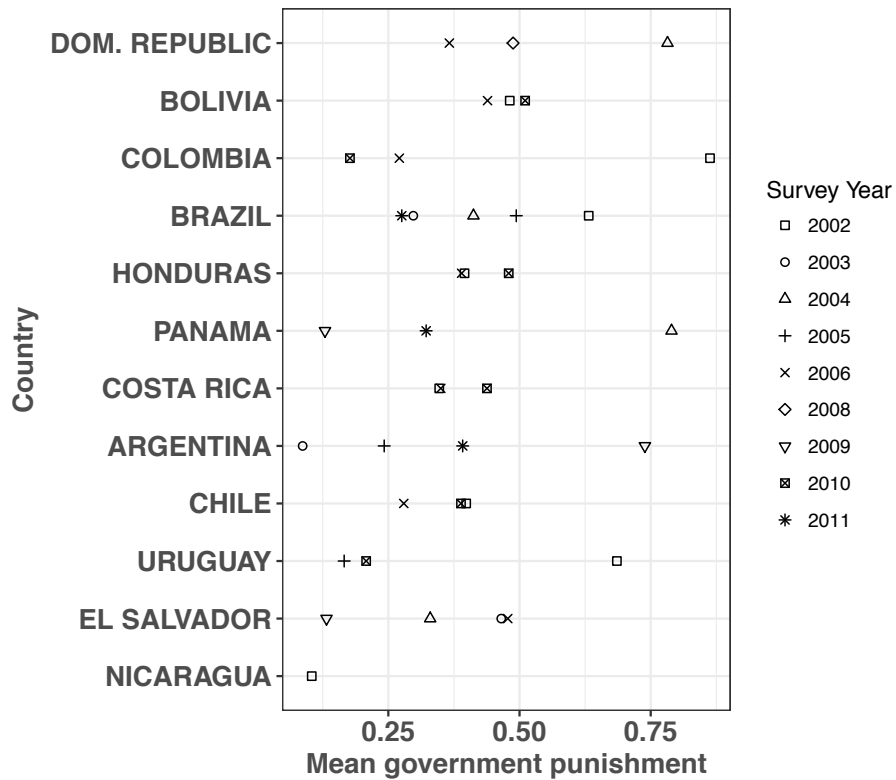
4.2 Independent Variables

The main independent variables in our analysis are: *ideological composition of the cabinet* and *respondent's retrospective evaluations of the economy*.

4.2.1 Ideological Composition of the Cabinet

As previously mentioned, information about ideological cabinet composition was obtained from Freudenreich (2016).[9] Freudenreich (2016) ordered parties along a left-right dimension based on Wiesehomeier and Benoit's (2009) estimates of parties' policy positions from expert surveys conducted in 18 Latin American countries from 2006 to 2007.

Wiesehomeier and Benoit (2009) provide evidence that the positioning of Latin American political parties on traditional policy issues is strongly based on a left-right ideological dimension. Their results corroborate the findings of elite surveys data in the region, revealing that Latin American political parties (Colomer and Escatel, 2005; Zoco, 2006; Kitschelt et al., 2010; Wiesehomeier and Doyle, 2012) are ideological and consistently located on the left-right dimension.

The ideological range of cabinets was then measured by Freudenreich (2016) as the maximum ideological distance between parties that hold cabinet posts, i.e., the ideological distance between the most leftist and rightist parties within each cabinet, producing a score that ranges from 0 to 4. A value of 0 means a cabinet comprised by parties located in the same position in a left-right scale. A value of 4 means the most ideologically heterogeneous cabinet possible, i.e., a cabinet comprised by parties located most further away from each other on the left-right ideological spectrum.

In our sample, the *ideological range* variable has a minimum value of 0 (the same as in Freudenreich's original data)—i.e., an ideologically homogeneous cabinet—and a maximum

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[9]In her study, Freudenreich (2016) considers all potential coalitions that could be formed. As these alternative coalitions (i.e., coalitions that were not formed) were not of interest for our empirical analysis, we dropped these cases. Therefore, we only consider coalitions that were formed (the coalitions that the author identifies as *empirical governments*) from Freudenreich's data.

value of 3 (instead of the original maximum value of 4)—i.e., an ideologically heterogeneous cabinet. The mean value of this variable is 0.706, with a standard deviation of 1.112.[10]

4.2.2   Retrospective Evaluations

Respondents' retrospective evaluations of the economy were obtained from the following survey question: "Do you consider your economic situation and that of your family to be much better, a little better, about the same, a little worse or much worse than 12 months ago?" Higher values of this variable indicate poorer economic evaluations.

According to our theoretical expectation, the ability of respondents to punish the president under worse economic evaluations should be conditional on the mean ideological composition of the cabinet. In particular, when the cabinet has a higher level of ideological heterogeneity (i.e., is composed of more ideologically distant parties), respondents should be less likely to punish the president. In other words, if our theory is supported by our empirical test, then we should find that the interaction between both variables (economic evaluation and ideological heterogeneity) has a negative and significant effect. Therefore, we interact the *mean ideological composition* of the cabinet with respondents' *retrospective evaluations of the economy*.

4.2.3   Control Variables

Based on our theoretical argument, we also include a series of control variables that could affect our dependent and main independent variables. At the aggregate-level, we control for three economic indicators obtained from the World Bank (2014): *GDP growth*, *inflation*, and *unemployment*. *GDP growth* is the annual percentage growth rate of GDP based on constant local currency in the year previous to the survey, while *inflation* is the annual percentage

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[10]With the exception of Brazil, all countries had only one cabinet for survey year. In the case of Brazil, that in 2005 and 2007 had more than one cabinet formed, we decided to calculate the mean of the cabinet ideological distance in the same survey year before merging this data with *Latinobarometer* survey data. As removing these Brazilian cabinets from our sample does not change our results, we decided to keep them in our analyses. A list with the number of cabinets in each country and survey year can be found in Table A.3 in the Appendix A.

change in the costs to the average consumer of acquiring a basket of goods and services, measured in the year previous to the survey. Similarly to unemployment. *Unemployment* is the percentage of the labor force that is unemployed in the year previous to the survey. We expect that GDP growths have a negative impact on presidential punishment but that unemployment and inflation have a positive impact, as when the economy is doing good, voters should be less likely to penalize incumbent governments, but when it is doing bad, they should be more likely to do so.

We also include Negretto's (2009; 2013) *Comparative Index of Legislative Powers of the President* (ILP)[11] which measures the concentration of policy-making power of the executive, as this can affect both our independent and dependent variables. On the one hand, presidents with more extensive constitutional powers will likely build more homogeneous cabinets ((Amorim Neto, 2006a,b). On the other hand, as previous studies revealed (Carlin and Singh, 2015), Latin American presidents with broad legislative powers for policymaking are likely to face higher levels of accountability for economic outcomes.

Since presidents lose popularity over time during their administrations (Carlin, Martínez-Gallardo, and Hartlyn, 2012; Gronke and Newman, 2009), we also control for president's time in office in months at the time of the survey(*cycle*). Singer and Carlin (2013) also found that prospective voting predominates early in the president's election cycle, but that retrospective voting becomes more important as the incumbent's term develops. Following this finding, we expect that the presidents who have been in office for a longer period of time at the time of the survey will face higher levels of economic accountability.

We also account for whether the president has a majority in the legislature, as this could affect our dependent and main independent variables. On the one hand, presidents who have a majority in the legislature have fewer incentives to hand out cabinet posts to other

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[11]Negretto's index has a theoretical range of 1 to 100, with higher values indicating greater legislative powers of the president. To create this index the author takes into account the president's veto powers, president's line-item and budget veto powers, the president's exclusive initiative, the existence and nature of urgency bills, whether the president has power to call a referendum, his budgetary powers, and decree authority. It is also important to mention that Negretto's index captures each country's constitutional changes.

parties (Amorim Neto, 2006b; Martínez-Gallardo, 2012), and therefore the mean ideological composition of the cabinet might be smaller. On the other hand, having a majority legislative support corresponds to a high clarity of responsibility situation (Powell and Whitten, 1993) that could bias the direction of our findings.

At the individual-level, we control for the wealth of the respondent, as it could affect both economic perceptions as well as the likelihood that respondents will punish the president for economic outcomes. Wealth is a categorical variable between 1 and 4, where higher values indicate higher income conditions.[12] We also control for education, age, gender, and ideology of the respondent [13] as all of these variables could affect the respondent's attribution of responsibility to governments and the respondent's vote choice (Anderson, 2000; Nadeau, Niemi, and Yoshinaka, 2002; Marsh and Tilley, 2010). Education is a categorical variable from 0 to 3, where higher values indicate higher education levels.[14] Age is a categorical variable dividing respondents' age into quartiles. Ideology is a categorical variable from 0 to 10, where higher values indicate that respondents are located more to the right of the ideological spectrum.[15] Gender is a dummy variable, where 1 indicates that the respondent is a woman, and 0 if the respondent is a man.

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[12]The survey question used to create this variable is: "Does the salary you receive and your total family income allow you to cover your needs in a satisfactory manner? Which of the following statements describes your situation?" Respondents can choose between 4 options: "It's sufficient and we can save", "It's just sufficient and we don't have major problems", "It's not sufficient and we have problems" and "It's not sufficient and we have major problems."

[13]Another relevant variable to control for is party identification. Whether respondents identify with the party of the incumbent president or another party could impact both their economic perceptions and their probability of punishing her for these. However, unfortunately the surveys do not ask respondents the specific party with which they identify.

[14]In particular, 0= no education, 1= primary education, 2= secondary education, and 3= higher education.

[15]The question used to construct this variable is the following: "In politics, people normally speak of "left" and "right." On a scale where 0 is left and 10 is right, where would you place yourself?"

4.3 Methodological Approach: Multilevel Models for Binary Responses

The integration of aggregate-level with individual-level data is of particular interest in empirical tests of the economic voting theory. On the one hand, classic studies on cross-national variations in the economic vote were based on aggregate-level data (Lewis-Beck, 1990; Paldam, 1991; Powell and Whitten, 1993). On the other, individual-level models have been identified as better specified models to test the economic vote (Lewis-Beck and Stegmaier, 2000; Duch and Stevenson, 2005).

Yet, the combined use of aggregate-level data (on cabinet composition and economic indicators) and survey data in our empirical analysis could lead us to biases in both our parameter estimates and their standard errors if we overlook the hierarchical structure of the data (Gelman and Holl, 2007). Our time-series cross-sectional (TSCS) data have a clear multilevel structure, where level-1 units are individuals (i.e., the respondents in each of the surveys) and level-2 units are the countries in which these surveys were conducted (i.e., countries in which the respondents are grouped). Moreover, since each survey was conducted in a particular country in a particular time point (i.e., the date the survey was administered), our data also allows for variation in the individual economic perceptions and president punishment across countries and over time (i.e., years).

To test whether the impact of economic evaluations on president punishment is conditioned by cabinet composition, we estimate multilevel logistic models to capture how our main variables of interest (and the interaction between them), which are measured at various levels of a hierarchical structure (i.e., *egotropic retrospective* among survey respondents across countries and over time, and *ideological range* across countries and over time) affect our outcome of interest, *president punishment.* In addition, we include a random intercept for country and a fixed effect for year in our models to allow for unmeasured variables that might make all respondents in a given country or for a given country-survey more or less likely to punish the president The general structure of our models using indexes i, t, and j

(for respondent, year, and country, respectively) can be represented as,

$$Pr\big(\text{president\_punishment}_{itj} = 1\big) = \text{logit}^{-1}\big(\beta_0 + \beta_1 \text{egotropic\_retrospective}_{itj}$$
$$+\beta_2 \text{ideological\_range}_{itj} + \beta_3 (\text{egotropic\_retrospective} \times \text{ideological\_range})_{itj}$$
$$+\beta_{(\text{controls})[itj]} + u_j + e_{itj}\big),$$
$$\text{for } i = 1, \ldots, N; \; t = 1, \ldots, T; \text{ and, } j = 1, \ldots, J.$$

Where e is the error term, and each country has a random intercept u_j,

$$u_j \sim N(0, \sigma_u^2), \text{ and } e_{ij} \sim N(0, \sigma_e^2)$$

We have also explored other model specifications with random intercepts for both year and country, and random intercept for country-year. The main results of our empirical analysis remain robust to these different specifications.

5 Results and Discussion

Tables 1 and 2 present the results of the analysis about the president's accountability for bad economic outcomes in the Latin American countries included in our study. The models presented in Table 1 include our main independent variables (egotropic evaluations, the ideological range of the cabinet, and the interaction between these two variables) one at a time without any control.

As can be seen in Table 1, the results support our two main hypotheses. Egotropic retrospective evaluations have a positive and significant effect across all models, indicating that as citizens' economic evaluations worsen, they are more likely to punish the president. To test whether the impact of economic evaluations on punishment is conditioned by cabinet composition, in model 3 of Table 1 we interact our measure of ideological range with economic retrospective evaluations. As expected, the main interaction variable is negatively signed and significant at level 0.01, indicating that as economic evaluations worsen and cabinets are

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Table 1: Benchmark Full Sample Models:
Models with Main Independent Variables (Without Controls)

	(Model 1)	(Model 2)	(Model 3)
Egotropic Retrospective	0.434***	0.436***	0.474***
	(0.012)	(0.012)	(0.015)
Ideological Range		-0.138***	0.001
		(0.023)	(0.037)
Egotropic Retrospective × Ideological Range			-0.049***
			(0.010)
Constant	-1.791***	-1.720***	-1.834***
	(0.163)	(0.177)	(0.180)
Random-effects Parameters for Country:			
Constant	-0.602***	-0.518**	-0.510**
	(0.210)	(0.210)	(0.210)
N	38419	38419	38419
Log-Likelihood	-24807.792	-24789.366	-24778.591

Dependent Variable: President Punishment (binary variable).

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

composed of more ideologically heterogeneous parties (i.e., clarity of responsibility decreases), the likelihood that voters will act on those evaluations and punish the president decreases.

Table 2: Cabinet Composition and Attribution of Responsibility

	(Model 4) Controls	(Model 5) Interaction	(Model 6) Interaction Year FE
Egotropic Retrospective	0.425***	0.481***	0.470***
	(0.016)	(0.020)	(0.020)
Ideological Range	-0.233***	-0.045	-0.179***
	(0.030)	(0.047)	(0.050)
Egotropic Retrospective × Ideological Range		-0.066***	-0.059***
		(0.013)	(0.013)
Unemployment (log)$_{t-1}$	0.141*	0.139	-0.598***
	(0.085)	(0.085)	(0.200)
Inflation (log)$_{t-1}$	-0.103***	-0.101***	-0.306***
	(0.018)	(0.019)	(0.026)
GDP Growth$_{t-1}$	-0.035***	-0.034***	-0.043***
	(0.004)	(0.005)	(0.009)
Legislative Majority	-0.279***	-0.287***	-0.100*
	(0.042)	(0.042)	(0.057)
Cycle in Months (log)	0.489***	0.485***	0.286***
	(0.021)	(0.021)	(0.033)
President's Legislative Powers (ILP)	0.117***	0.116***	0.127***
	(0.016)	(0.016)	(0.018)
Wealth	-0.011	-0.012	-0.016
	(0.017)	(0.017)	(0.017)
Education	0.107***	0.109***	0.114***
	(0.019)	(0.019)	(0.019)
Age	-0.080***	-0.079***	-0.075***
	(0.015)	(0.015)	(0.015)
Ideology	-0.065***	-0.066***	-0.069***
	(0.005)	(0.005)	(0.006)
Gender	0.044	0.043	0.049*
	(0.027)	(0.027)	(0.027)
Constant	-8.754***	-8.838***	-6.785***
	(1.226)	(1.223)	(1.434)
Random-effects Parameters for Country:			
Constant	1.014***	1.001***	0.997***
	(0.248)	(0.249)	(0.260)
N	25665	25665	25665
Log-Likelihood	-15796.373	-15783.613	-15605.479

Dependent Variable: President Punishment (binary variable).
Note: Unemployment, inflation, and *cycle in months* were log transformed to reduce the right-skewness of their distribution. Model 3 conducted with Year Fixed-Effects (FE). Standard errors in parentheses. Two-tailed test.
* *p* < 0.1, ** *p* < 0.05, *** *p* < 0.01

Models presented in Table 2 indicate that, once control variables are accounted for, the main results remain unchanged. Model 4 includes the main independent variables together with the controls, Model 5 includes the main interaction variables together with the controls, and Model 6 controls for unmeasured factors common to all countries in the same survey year through year-fixed effects.[16] As Model 4 shows, egotropic evaluations have a positive and significant effect on our main dependent variable, indicating that - as the literature suggests - economic voting exists across the region. In addition, ideological range has a negative and significant effect, indicating that more heterogeneous cabinets decrease presidential punishment. The results (particularly Models 5 and 6) further suggest that the impact of citizens retrospective evaluations on presidential punishment is conditional on the composition of the cabinet, as the main interaction variable is negatively signed and statistically significant.[17]

The results in Table 2 also show that the effect of some of our control variables remains consistent in all models. At the individual level, education is positively related to presidential punishment, while age and ideology have a negative relationship to it. Wealth and gender seem to have no significant effect on presidential punishment (except for Model 6 that indicates a significant relationship between being a woman and presidential punishment). At the country-level, we find similar findings to previous studies (Singer and Carlin, 2013): voters are less likely to punish presidents with a majority of seats in the legislature, and they are less prone to approve of presidents who have held office longer as well as those who have strong legislative powers. As expected, GDP growth decreases the likelihood that citizens punish presidents across all model specifications, and unemployment increases it (although

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[16]We also estimated models including random intercept for year along with random intercept for country (see Table C.3 in the Appendix) and the main results remain unchanged, as our main interaction variable is still negative and statistically significant.

[17]It might be the case that citizens' attribution of responsibility for economic outcomes and therefore the probability that they punish presidents heading different types of cabinets is a function of their political knowledge about functional responsibility. In other words, it might be the case that only those citizens who have information about how government work and who is responsible for specific policy outcomes (i.e., cabinet composition) (Fortunato and Stevenson, 2013) are less likely to punish presidents heading ideologically heterogeneous cabinets. To examine if this is the case we run our main models with a three way interaction between political attention (as a proxy for political knowledge), mean cabinet composition, and egotropic evaluations. However, the effect of this variable was not statistically significant, indicating that our results are not conditioned by the level of political knowledge of citizens.

the positive effect of this variable is not robust to the inclusion of year fixed effects). Regarding inflation, we find an inconsistent negative effect: it decreases the probability that citizens disapprove of the president. However, this might be related to the fact that most of the effect of this variable is being captured by another one of our economic indicators, unemployment.

Figure 2 graphs how the marginal effect of egotropic evaluations changes with the ideological range of the cabinet. 95% confidence intervals around the line allow us to determine the conditions under which retrospective evaluations have a statistically significant effect on presidential punishment.

Figure 2: Conditional Marginal Effects of *Egotropic Retrospective | Ideological Range* on *President Punishment (with 95% confidence intervals)*
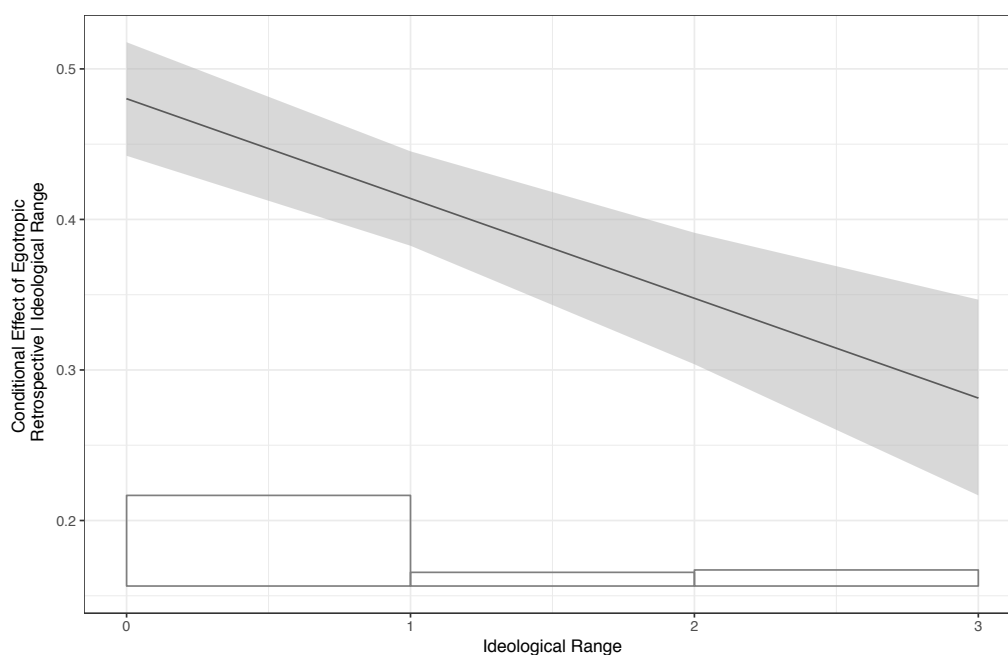
As can be seen in Figure 2, as the ideological composition of the cabinet gets more heterogeneous, the negative impact of having bad evaluations of the economy on presidential punishment becomes moderated. In other words, cabinet composition mediates the relationship between retrospective economic evaluations and voter behavior so that citizens with

bad evaluations are less likely to punish the president. Given the symmetry of the interaction, in Figure C.1 in the Appendix we depict the marginal effect of the ideological range of the cabinet on presidential punishment, conditional on economic evaluations. As expected, heterogeneous cabinets strongly decrease presidential punishment, and this reductive effect gets higher as economic evaluations worsen.

In summary, we find evidence that cabinet composition is a relevant institutional variable to be taken into account in order to understand presidential accountability for economic outcomes. Presidents that head more ideologically heterogeneous cabinets are held less accountable for their bad economic performance, and this finding is reinforced when examining the impact of our main interaction variable. Relating this back to our main theory, voters hold presidents to account for economic outcomes to a less extent when they are able to diffuse their responsibility for these outcomes through a cabinet that includes actors with diverse preferences.

6 Conclusion

In this study, we set out to test whether cabinet composition affects the clarity of responsibility for policy outcomes and, in turn, the probability that citizens hold presidents accountable for bad outcomes. The motivation to pursue this question originated from the fact that much of the existing literature on president's accountability for policy outcomes has focused on a single exogenous variable: the legislative support for the president's party. As a result, our understanding of how voters attribute responsibility for policy outcomes to presidents that head different types of cabinets - i.e., a different type of institutional context - remains limited.

In order to test the effect that the composition of the president's cabinet has on the likelihood that voters attribute responsibility to the executive and punish her for bad outcomes we used existent survey data from the *Latinobarometer* together with data on cabinet

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composition for a set of presidential democracies. Our main expectation was that in these countries voters should be more likely to punish presidents heading homogeneous cabinets. Our findings supported our main expectation: in presidential systems voters are less likely to act on their retrospective economic evaluations when presidential cabinets are more ideologically diverse. This finding is important because it allows us to begin building knowledge about, on the one hand, the extent to which voters are able to hold presidents accountable for economic outcomes in multiparty presidential democracies, and on the other, the types of institutional variables that are relevant for explaining variations in clarity of responsibility and, in turn, variations in economic accountability across the region.

Our study suggests that presidents in Latin America face a trade-off when building their cabinets. On the one hand, presidents might prefer to fill their ministries with cronies, technocrats and party allies, as this gives them more leeway to implement their policy packages. On the other, this will facilitate vertical accountability, and they therefore need to be careful with the economic consequences of their policies if they don't want to be ousted in the next presidential elections. In addition, our research shows that voters in Latin America are able to attribute responsibility when the institutional context makes it easier for them to recognize who to blame.

The results from the empirical analyses provide avenues of further research on this topic. Specifically, further work should explore if other factors affecting cabinet composition, such as the amount and saliency of ministries controlled by the parties in the cabinet, have an effect on government accountability. It could be the case that a president that heads a cabinet that is ideologically heterogeneous but where her party controls the most important posts (such as the ministry of economy) is held accountable to the same or greater extent than presidents heading ideologically homogeneous cabinets. In a future draft of the paper we wish to control for these factors. In addition, we wish to explore if our main results remain robust to a different codification of the main independent variable: the ideological distance of governing parties from the ideological position of the party of the president. We

believe this operationalization of the independent variable will allow us to get results that are more aligned with our theory.

Furthermore, we wish to test our hypothesis using a different specification of the dependent variable and a different dataset. In a future draft of the paper we plan to use *LAPOP* data together with *Latinobarometer* data to see if the results of our analysis hold across the respondents of both surveys (and disregard any *house* effect). If our results hold across respondents from the two barometers, this will give us more confidence in our results. In addition, we would like to operationalize our main dependent variable (presidential punishment) using vote choice (i.e., would respondents vote for the incumbent president if elections were held that day?) instead of presidential approval to check if our results holds with this variable as well.

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Appendix

A Countries, Years, Respondents, and Cabinets

Table A.1: Countries and years included in the study.

Country	Survey Year
Argentina	2003, 2005, 2009, 2011
Bolivia	2002, 2006, 2010
Brazil	2002, 2003, 2004, 2005, 2011
Chile	2002, 2006, 2010
Colombia	2002, 2006, 2010
Costa Rica	2002, 2006, 2010
Dominican Republic	2004, 2006, 2008
El Salvador	2003, 2004, 2006, 2009
Honduras	2002, 2006, 2010
Nicaragua	2002
Panama	2004, 2009, 2011
Uruguay	2002, 2005, 2010

*Source:*Latinobarometer.

Table A.2: Number of Respondents (Frequency) by Country

Country	Frequency	Percent	Cumulative
Brazil	5,817	13.79	13.79
Argentina	4,806	11.39	25.18
El Salvador	4,034	9.56	34.74
Bolivia	3,649	8.65	43.39
Colombia	3,607	8.55	51.94
Uruguay	3,607	8.55	60.49
Chile	3,603	8.54	69.03
Costa Rica	3,013	7.14	76.18
Honduras	3,011	7.14	83.31
Dominican Republic	3,007	7.13	90.44
Panama	3,007	7.13	97.57
Nicaragua	1,025	2.43	100
Total	**42,186**	**100**	

Table A.3: Countries, survey years, and number of cabinets included in the analysis.

Country	Survey Year	Number of Cabinets
Argentina	1995	1
Argentina	2005	1
Argentina	2007	1
Argentina	2009	1
Argentina	2011	1
Bolivia	2002	1
Bolivia	2006	1
Bolivia	2010	1
Brazil	2002	1
Brazil	2003	1
Brazil	2004	1
Brazil	2005	3
Brazil	2007	2
Brazil	2011	1
Chile	2002	1
Chile	2006	1
Chile	2010	1
Colombia	2002	1
Colombia	2006	1
Colombia	2010	1
Costa Rica	2002	1
Costa Rica	2006	1
Costa Rica	2010	1
Dom Republic	2002	1
Dom. Republic	2004	1
Dom. Republic	2006	1
Honduras	2002	1
Honduras	2006	1
Honduras	2010	1
Panama	2004	1
El Salvador	2003	1
El Salvador	2004	1
El Salvador	2006	1
El Salvador	2009	1
Uruguay	2002	1
Uruguay	2005	1
Uruguay	2010	1
Nicaragua	2002	1
Nicaragua	2007	1

B   Descriptive Statistics

Table B.1: Descriptive Statistics and Variables

Variables:	Mean	Std. Dev.	Min.	Max.	N
Dependent Variable:					
President Punishment	0.401	0.49	0	1	38752
Main Independent Variables:					
Egotropic Retrospective	2.912	0.903	1	5	41703
Ideological Range	0.706	1.112	0	3	42108
Control Variables:					
GDP Growth (%)	4.033	3.889	-10.894	11.984	42186
Unemployment (%)	9.618	3.973	2.7	19.6	42186
Inflation (%)	7.199	5.618	-1.067	51.461	42186
Legislative Majority	0.395	0.489	0	1	42109
President's Cycle (in Months)	27.233	21.824	3	88	41111
Legislative Powers of the President (ILP)	55.929	24.557	22.06	99.94	38703
Respondent's Wealth	2.379	0.856	1	4	41097
Respondent's Education	1.724	0.817	0	3	37216
Respondent's Age	2.32	0.992	1	4	42104
Respondent's Ideology	4.452	2.932	0	10	34449
Respondent's Gender	0.518	0.5	0	1	42104

C Robustness Checks

Table C.1: Benchmark Models: Full Sample (Without Controls)

	(Model 1)	(Model 2)	(Model 3)	(Model 4) Year FE	(Model 5) Year FE	(Model 6) Year FE
Egotropic Retrospective	0.434***	0.436***	0.474***	0.402***	0.405***	0.442***
	(0.012)	(0.012)	(0.015)	(0.013)	(0.013)	(0.015)
Ideological Range		-0.138***	0.001		-0.184***	-0.045
		(0.023)	(0.037)		(0.026)	(0.040)
Egotropic Retrospective × Ideological Range			-0.049***			-0.049***
			(0.010)			(0.011)
2003				-1.572***	-1.464***	-1.455***
				(0.063)	(0.065)	(0.065)
2004				-0.228***	-0.095	-0.097
				(0.056)	(0.059)	(0.059)
2005				-1.242***	-1.154***	-1.148***
				(0.057)	(0.058)	(0.058)
2006				-0.779***	-0.765***	-0.768***
				(0.038)	(0.038)	(0.038)
2008				-0.905***	-0.836***	-0.844***
				(0.090)	(0.090)	(0.091)
2009				-1.099***	-1.031***	-1.033***
				(0.064)	(0.065)	(0.065)
2010				-0.661***	-0.732***	-0.735***
				(0.037)	(0.038)	(0.038)
2011				-1.233***	-1.138***	-1.143***
				(0.063)	(0.064)	(0.064)
Constant	-1.791***	-1.720***	-1.834***	-1.073***	-1.007***	-1.116***
	(0.163)	(0.177)	(0.180)	(0.220)	(0.230)	(0.233)
Random-effects Parameters: for Country:						
Constant	-0.602***	-0.518**	-0.510**	-0.294	-0.249	-0.243
	(0.210)	(0.210)	(0.210)	(0.208)	(0.208)	(0.208)
N	38419	38419	38419	38419	38419	38419
Log-Likelihood	-24807.792	-24789.366	-24778.591	-24099.724	-24075.159	-24065.037

Dependent Variable: President Punishment (binary variable). *Notes:* Models 4, 5, and 6 were conducted with Year Fixed-Effects (FE). Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Models with Controls and Interactions
(with and without Year Fixed-Effects)

	(Model 1) Controls	(Model 2) Controls Year FE	(Model 3) Interaction	(Model 4) Interaction Year FE
Egotropic Retrospective	0.425***	0.421***	0.481***	0.470***
	(0.016)	(0.016)	(0.020)	(0.020)
Ideological Range	-0.233***	-0.344***	-0.045	-0.179***
	(0.030)	(0.033)	(0.047)	(0.050)
Egotropic Retrospective × Ideological Range			-0.066***	-0.059***
			(0.013)	(0.013)
Unemployment $(\log)_{t-1}$	0.141*	-0.633***	0.139	-0.598***
	(0.085)	(0.199)	(0.085)	(0.200)
Inflation $(\log)_{t-1}$	-0.103***	-0.310***	-0.101***	-0.306***
	(0.018)	(0.026)	(0.019)	(0.026)
GDP Growth$_{t-1}$	-0.035***	-0.046***	-0.034***	-0.043***
	(0.004)	(0.009)	(0.005)	(0.009)
Legislative Majority	-0.279***	-0.092	-0.287***	-0.100*
	(0.042)	(0.057)	(0.042)	(0.057)
Cycle in Months (log)	0.489***	0.284***	0.485***	0.286***
	(0.021)	(0.033)	(0.021)	(0.033)
President's Legislative Powers (ILP)	0.117***	0.129***	0.116***	0.127***
	(0.016)	(0.018)	(0.016)	(0.018)
Wealth	-0.011	-0.016	-0.012	-0.016
	(0.017)	(0.017)	(0.017)	(0.017)
Education	0.107***	0.112***	0.109***	0.114***
	(0.019)	(0.019)	(0.019)	(0.019)
Age	-0.080***	-0.075***	-0.079***	-0.075***
	(0.015)	(0.015)	(0.015)	(0.015)
Ideology	-0.065***	-0.068***	-0.066***	-0.069***
	(0.005)	(0.006)	(0.005)	(0.006)
Gender	0.044	0.049*	0.043	0.049*
	(0.027)	(0.027)	(0.027)	(0.027)
Constant	-8.754***	-6.620***	-8.838***	-6.785***
	(1.226)	(1.433)	(1.223)	(1.434)
Random-effects Parameters for Country:				
Constant	1.014***	1.008***	1.001***	0.997***
	(0.248)	(0.259)	(0.249)	(0.260)
N	25665	25665	25665	25665
Log-Likelihood	-15796.373	-15615.146	-15783.613	-15605.479

Dependent Variable: President Punishment (binary variable).
Note: Model 2 and Model 4 with Year Fixed-Effects (FE). *Inflation* and *cycle in Months* were log transformed to reduce the right-skewness of their distribution.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table C.3: Cabinet Composition and Attribution of Responsibility:
Using Random Intercepts for Country and Year

	(Model 1)	(Model 2) Interaction
Egotropic Retrospective	0.421***	0.470***
	(0.016)	(0.020)
Ideological Range	-0.340***	-0.174***
	(0.033)	(0.050)
Egotropic Retrospective × Ideological Range		-0.059***
		(0.013)
Unemployment $(\log)_{t-1}$	-0.590***	-0.558***
	(0.193)	(0.194)
Inflation $(\log)_{t-1}$	-0.303***	-0.299***
	(0.026)	(0.026)
GDP Growth$_{t-1}$	-0.044***	-0.041***
	(0.009)	(0.009)
Legislative Majority	-0.097*	-0.104*
	(0.056)	(0.056)
Cycle in Months (log)	0.291***	0.293***
	(0.033)	(0.033)
President's Legislative Powers (ILP)	0.129***	0.127***
	(0.018)	(0.018)
Wealth	-0.016	-0.016
	(0.017)	(0.017)
Education	0.112***	0.114***
	(0.019)	(0.019)
Age	-0.075***	-0.075***
	(0.015)	(0.015)
Ideology	-0.068***	-0.069***
	(0.006)	(0.006)
Gender	0.049*	0.048*
	(0.027)	(0.027)
Constant	-6.950***	-7.104***
	(1.405)	(1.435)
N	25665	25665
Log-Likelihood	-15630.560	-15620.820

Dependent Variable: President Punishment (binary variable).
Notes: Both models conducted using random intercept for *country* and *year*.
Unemployment, *Inflation*, and *cycle in months* were log transformed to reduce
the right-skewness of their distribution. Standard errors in parentheses.
Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Random-effects parameters for country:
Model 1: Constant = 7.6430; Std.Dev = 2.7646.
Model 2: Constant = 7.4736; Std.Dev = 2.7338.
Random-effects parameters for year:
Model 1: Constant = 0.1795; Std.Dev = 0.4236.
Model 2: Constant = 0.1758; Std.Dev = 0.4193.

Table C.4: Model with Country-Survey Random Intercept

|	Dependent Variable: President Punishment
Egotropic Retrospective	0.410***
	(0.021)
Mean Ideological Range	0.271
	(0.204)
Egotropic Retrospective × Mean Ideological Range	-0.048***
	(0.014)
GDP Growth$_{t-1}$	-0.021
	(0.043)
Unemployment (log)$_{t-1}$	0.011
	(0.044)
Inflation (log)$_{t-1}$	-0.098
	(0.142)
Legislative Majority	-0.220
	(0.320)
Cycle in Months (log)	0.380**
	(0.152)
President's Legislative Powers (ILP)	-0.007
	(0.009)
Wealth	-0.041**
	(0.018)
Education	0.148***
	(0.020)
Age	-0.070***
	(0.016)
Ideology	-0.069***
	(0.006)
Gender	0.052*
	(0.029)
Constant	-1.991***
	(0.706)
N	25665
Log-Likelihood	-14602.883

Notes: Unemployment, Inflation, and cycle in months were log transformed to reduce the right-skewness of their distribution. Standard errors in parentheses.

Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Conditional Marginal Effects of *Egotropic Retrospective* and *Ideological Range* on *President Punishment*
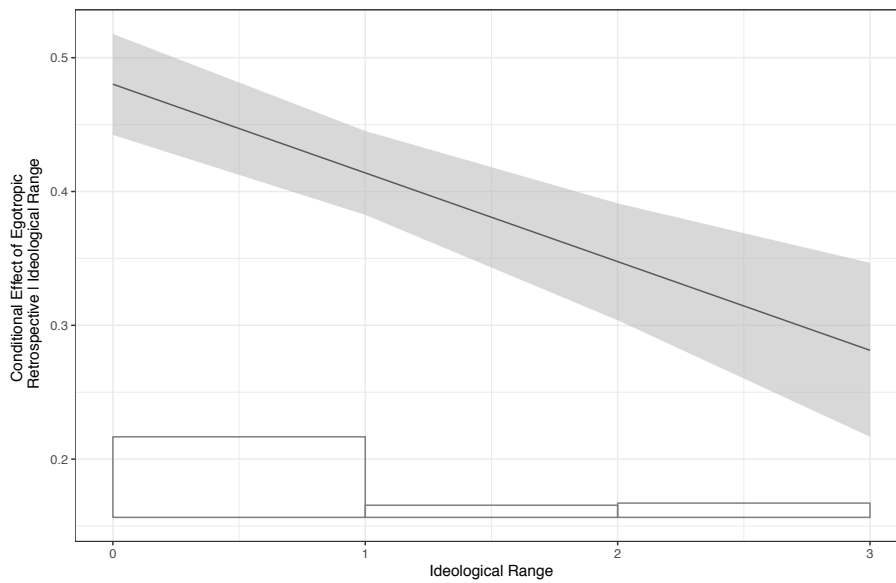
a. Egotropic Retrospective | Ideological Range
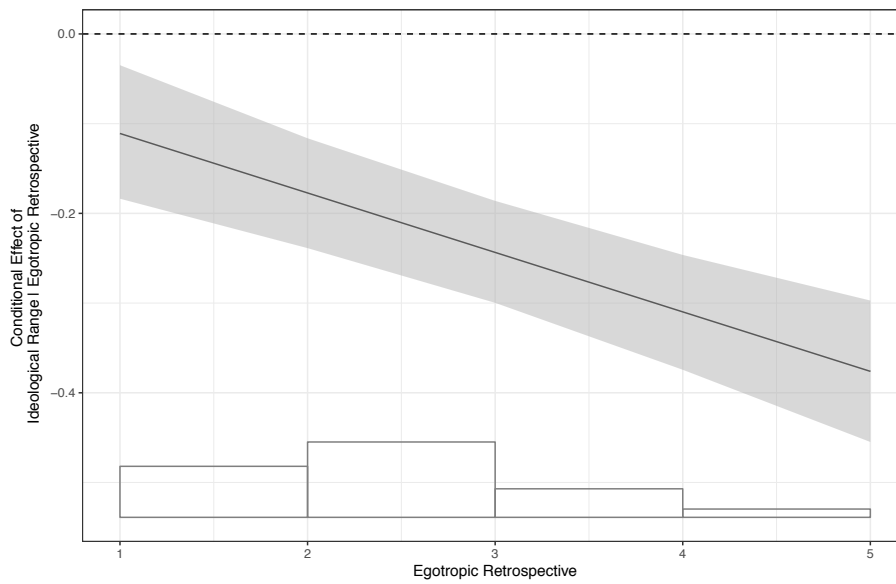
b. Ideological Range | Egotropic Retrospective

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