

LETTER

Agenda Control and Electoral Success in the US House

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‘Party records often can be changed in ways that affect the vast majority of members’ reelection probabilities in the same way (either helping or hurting all)’.

– Cox and McCubbins 1993, 112.

‘The key resource that majority parties delegate to their senior partners is the power to set the legislative agenda; the majority party forms a procedural cartel that collectively monopolizes agenda-setting power’.

– Cox and McCubbins 2005, 24.

Cartel Theory (Cox and McCubbins 1993; Cox and McCubbins 2005) – and much of the subsequent scholarship on parties in the US House – is founded on the assumption that there is an essential link between how a party performs in government and how it performs in elections. In Cox and McCubbins’ (1993) initial formulation, quoted above, the electoral value of the party record – or ‘party brand’ – provides the incentive needed to solve the coordination and collective action problems inherent in a policy-making body composed of members with diverse preferences beholden to constituencies with diverse interests.

In subsequent work, Cox and McCubbins (2005) further clarified (as demonstrated in the second quote above) that the essential mechanism used to manage the party record is agenda control. In other words, they assume that what does (and does not) get on the legislative agenda determines some portion of the electoral reward or punishment shared by all majority-party members. This supposition has become commonplace in the congressional literature (for example, Grynaviski 2010; Kim and LeVeck 2013), yet an essential implication has never been tested: does successful legislative agenda control improve the electoral fortunes of majority-party members (and vice versa)?

In this letter, we test the hypothesis that, as the majority party in the US House is more successful at managing the legislative agenda, majority-party members’ individual electoral performances improve. We do not find support for the hypothesis. As we discuss in the conclusion, we cannot say definitively that the null result disproves this key model assumption, and it certainly does not disprove Cartel Theory. Still, this surprising null finding at a minimum raises questions about the particular mechanisms that incentivize congressional agenda control, which we believe to be of increasing importance as the theory continues to be exported to countries around the world such as Brazil (Neto et al. 2003), Japan (Cox et al. 2000), Israel (Akirav et al. 2010), Italy (Cox et al. 2008) and others.

Congressional Performance and Electoral Outcomes

Scholars have regularly grappled with the general question of party performance and electoral outcomes. For example, while there is mixed evidence regarding the relationship between the popularity of Congress (Durr et al. 1997), or the popularity of a particular party (McDermott and Jones 2003), and the electoral success of incumbent legislators, it is well known that at least some congressional behavior – such as particularly rewarding (Bovitz and Carson 2006) or damaging (Carson et al. 2010) roll-call votes – can affect individual members’ re-election chances. Similarly, there is evidence to suggest that, especially for incumbents in competitive districts, excessive party loyalty on salient issues can lead to re-election trouble (Carson et al. 2010), and that a strong party brand can make it harder for those incumbents to court pivotal moderate voters (Kim and Leveck 2013).

Yet previous studies have not directly examined the link between agenda-setting success and members’ electoral outcomes. The closest is Richman’s (2015) recent look at macro-level electoral responses to ‘blockout zones’, where he provides evidence that, contrary to the expectations of Cox and McCubbins (2005), the majority’s propensity to block bills that may produce rolls has negative electoral implications under certain circumstances.¹ But Cartel Theory is fundamentally about members’ individual incentives conflicting with the need for collective action.

According to Cartel Theory, the majority party is afforded procedural advantages that give its leaders greater control over the agenda. Rank-and-file members delegate management of the agenda to the party leadership, which is in charge of maintaining the party brand. Using these powers, the majority party is able to carefully manage which bills members vote on, and ultimately which policies are adopted on their watch. Members support these efforts (explicitly and tacitly), occasionally to the detriment of their personal policy goals or district-specific electoral goals, because the reputation that the party creates – the party brand – is thought to be electorally beneficial to individual members. That is, the stronger the party brand, the better all party members should fare in their bids for re-election.

This, according to Cartel Theory, is the linchpin that holds the cartel together: the shared electoral benefit of a strong party brand overcomes the collective action problem presented by diverse constituencies – and thus diverse electoral demands – within the caucus. Therefore, if agenda-setting power is the key to maintaining the party brand, and if a strong party brand is an electoral asset, then:

HYPOTHESIS 1: The more successful the majority party is at managing the legislative agenda, the better individual party members will fare electorally, all else equal.

Measurement, Design and Estimation

Cox and McCubbins, and most other scholars, have focused predominately on negative agenda control and examined it almost exclusively as a dependent variable. Appropriately, then, party rolls have been the predominant measure of agenda control. Rolls – when a majority of the majority party votes against a measure that passes nonetheless – are failures of negative agenda-setting power; thus fewer rolls indicates more effective negative agenda control. Since the Cartel Agenda Model makes specific, measurable predictions about negative agenda control failure, but assumes that negative agenda control success occurs prior to the floor stage (and thus in a manner that is hard to systematically measure), few studies have examined agenda control success and failure.

¹Though our focus is different from Richman’s – his hypotheses are not derived directly from Cartel Theory and his empirical focus is the aggregate seat share of the majority – we note that our results do contradict his in spirit. However, if we constrain Richman’s sample to match our own sample of the modern era 1972–2008 and replicate his analysis, his model outputs agree with our own.

Jenkins and Monroe (2016), however, articulate a more comprehensive typology of agenda control outcomes that captures both positive and negative agenda control successes and failures. While this measurement typology does not solve the pre-floor-agenda-control measurement problem mentioned above, it does give us a broader set of measures with which to gauge agenda management at the floor stage. Jenkins and Monroe discuss the following four categories of party-level agenda control outcomes: successes (a majority of the majority-party members vote *for* a measure and it *passes*), disappointments (a majority of the majority-party members vote *for* a measure but it *fails*), rolls (a majority of the majority-party members vote *against* a measure but it *passes*), and blocks (a majority of the majority party votes *against* a measure and it *fails*). Successes and blocks are indicators of effective positive and negative agenda control, respectively, while disappointments and rolls imply ineffective positive and negative agenda control, respectively.² In the following section we use both this four-part typology and a simplified two-part typology – aggregating successes and blocks as ‘wins’ and disappointments and rolls as ‘losses’ – to measure agenda control effectiveness in the House over time by sorting all final passage votes into one of these categorizations.³ For the four-part measure, we expect the marginal effect of successes, blocks, disappointments, and rolls on legislators’ electoral fortunes to have the following relationships: success > blocks > disappointments > rolls, and that all parameters will be positive relative to rolls, which will function as the base category in our models.⁴

This rank ordering begins with the assumption that allowing the party to be ‘rolled’ on the floor is the worst of leadership sins. Indeed, there is an informally held principle of majority leadership in the House – sometimes called the ‘Hastert Rule’ – that ‘no speaker can survive if he or she brings up a series of bills opposed by a majority of his or her party’ (Gingrich 2015). Intuitively, rolls also seem to be the worst outcome because they are the only category for which policy is moving away from the preference of more than half the party. Logically, the next-worse outcome of the four must then be a disappointment. Here, the party is no worse off – in that policy remains at the status quo and does not move further away from its preference – but there is perhaps a missed opportunity for policy gain. And, at minimum, a disappointment represents an instance in which the leadership has used up valuable plenary time with no new policy change to show for it.

Blocks are similar to disappointments in that sense, and thus one might quibble about whether blocks are really better. We assume that the party perceives blocks as a sort of successful backstop, catching things that fall through the cracks of their pre-floor negative agenda control, whereas disappointments are (in most cases) seen as wasted time and very public failures of positive agenda control. Finally, we assume that successes are the best outcome because they represent fruitful investments of agenda time that move policy in the direction of at least a majority of party members’ preferences. The intuition underlying our rank ordering is therefore that winning is preferable to losing, and that passing a new policy should bear more electoral fruit than maintaining the status quo. For the two-part measure, our expectation is a positive parameter estimate on the proportion of ‘wins’.⁵

²One might argue that the blocks – especially from the perspective of Cartel Theory – should not be viewed as successes, since they are in a sense evidence that pre-floor negative agenda control has failed. We are sympathetic to this claim, and we have empirically estimated ‘successes’ without blocks included, and the results are null. Still, we think there are good arguments in favor of keeping blocks as ‘successes’. Most notably, there may be cases when the majority party would want to make a public display of their opposition to some policy by publicly defeating it on the floor rather than quietly snuffing it out in committee.

³If we consider all votes, rather than only final passage votes, the substance of the presented results below does not change.

⁴Of course, this four-part measure is a compositional variable where the four parts must sum to 1. As such, one of the category’s parameters must be constrained to 0 (omitted from the model) in order to recover proper estimates.

⁵We also estimate models comparing rolls to all other categories combined, and the results are equivalent to our other specifications. We have included them in the appendix.

We marry these agenda control measures to Jacobson and Carson's (2016) House election data, which span all elections from 1972 through 2008.⁶ We use these data to adapt a canonical model of incumbent performance (Jacobson 1989) to our purposes: estimating the effect of majority-party agenda control on majority-party electoral performance. Our dependent variable is the share of the two-party vote won by the House majority-party candidate in a given district. We regress this variable on our measures of the majority's agenda control and their individual-level complements from the most recent Congress. That is, for each member of the majority party, we calculate their personal record of successes, blocks, disappointments and rolls – and the simple two-part descriptor – and include them in the model to account for individual deviations from the party as a whole. We also include the following vector of covariates in the model: the majority–minority spending gap (majority expenditures minus minority expenditures in the district), indicators for whether the district is currently represented by the majority or minority party and for whether the majority and minority candidates are 'quality candidates' (have previously held elected office), a dummy indicating that the majority candidate is unopposed by the minority (districts uncontested by the majority are omitted; such districts are rare), the performance of the majority in the previous contest (lagged dependent variable), and an interaction of the performance of the majority party's presidential candidate in the district (either in the concurrent election or the most recent contest in mid-term years) with an indicator for a presidential election year and a dummy indicating whether the sitting president is a member of the majority party. Finally, to account for correlations across rows of data due to unmeasured factors, we estimate random intercepts at the level of the election year and district. We present results from models using the two-part and four-part measures of agenda control in Table 1.

It is important to note that several of the variables are explicit interactions – and must therefore be interpreted appropriately – and several others are implied interactions. That is, some indicators may only take on a value of 1 if other indicators are also turned on. For example, whenever majority incumbent equals 1, majority-controlled district and quality majority candidate must also be equal to 1, as all incumbents are quality candidates and all incumbents run in districts controlled by their party.

These models bear no support for our hypothesis. In the simple specification, the party wins estimate is in the predicted direction, but far from robust. In the more precise four-part measure, the party-level agenda covariates fail to produce the predicted rank ordering and none are robust. Contrast these estimates with the covariates in the model that are unrelated to the legislative agenda: all other covariates are in the expected direction, and all save one are statistically robust. The model results suggest, for example, that the majority fairs better when it spends more, has a quality candidate, is running with a popular presidential candidate, etc. Indeed, the strength and sensibility of the control variable estimates suggest that the model is properly specified, yet evidence for the hypothesis is absent.

Model Sensitivity

We believe the model in Table 1 is an appropriate test of our hypothesis. However, in constructing this model we made a series of choices with which others could reasonably disagree. Further, because this model is just one of many that could be analyzed, and because we are claiming a null result, we want to explore just how sensitive this null result is to model specification and, if there are models that *do* support the hypothesis, we want to find them and evaluate their sensibility.

⁶This is as much data as is available to bring to bear on the question. It is possible that the results may change somewhat by adding data from 2010 to the present, however, given that congressional elections seem to be transitioning into extensions of presidential politics (e.g., Smidt 2017), we believe this is unlikely.

Table 1. Majority candidate performance

	Dependent variable: Majority Vote share	
	Simple	Disaggregated
Party Wins	0.362 (0.350)	
Party Successes		0.433 (0.378)
Party Blocks		0.290 (1.205)
Party Disappointments		1.553 (1.068)
Candidate Wins	-0.052*** (0.020)	
Candidate Successes		-0.045** (0.021)
Candidate Blocks		-0.062 (0.368)
Candidate Disappointments		-0.275 (0.357)
Majority–Minority Spending Gap	0.006*** (0.001)	0.006*** (0.001)
Majority-Controlled District	-0.060 (0.046)	-0.060 (0.046)
Majority Incumbent	0.038*** (0.010)	0.038*** (0.010)
Minority-Controlled District	-0.040 (0.046)	-0.039 (0.046)
Minority Incumbent	-0.067*** (0.013)	-0.067*** (0.013)
Majority Quality Candidate	0.028** (0.011)	0.028** (0.011)
Minority Quality Candidate	-0.030** (0.012)	-0.030** (0.012)
Majority Unopposed	0.088*** (0.008)	0.087*** (0.008)
District Presidential Vote (DPV)	0.140*** (0.030)	0.144*** (0.030)
Presidential Election Year (PEY)	-0.085*** (0.032)	-0.085*** (0.032)
Majority is Presidential Incumbent Party (MPI)	-0.063* (0.038)	-0.068* (0.039)
DPV × PEY	0.120*** (0.036)	0.119*** (0.036)
DPV × MPI	-0.008 (0.042)	-0.010 (0.042)
PEY × MPI	0.056 (0.055)	0.042 (0.057)
DPV × PEY × MPI	-0.011 (0.061)	-0.012 (0.061)
Lagged Dependent Variable	0.387*** (0.021)	0.386*** (0.021)
Constant	0.028 (0.335)	-0.062 (0.368)
District Variance	0.000	0.000
Year Variance	0.003	0.003
Observations	7,289	7,289
Log Likelihood	3,801.712	3,804.447
Akaike Inf. Crit.	-7,559.423	-7,556.894

*p < 0.1; **p < 0.05; ***p < 0.01

In order to assess the sensitivity of the null result to the specification and seek out models that support the hypothesis, we take the brute force approach of estimating every possible specification our data allow. That is, we construct a list of variables (each variable in Table 1, where each permutation of the presidential support–presidential election year–presidential co-partisanship interactions are estimated separately, plus a dummy indicating that a district was redrawn),⁷ and build a program that cycles through each and every possible combination of variables, estimates the model and logs the results. In all, there are 163,840 unique variable combinations to be evaluated. Through these exercises, however, we remind our readers of the principal constraint on the unique combinations of included variables: the data we have gathered. Thus, we can only discuss the sensitivity of our null result with respect to the data we have on hand. Nonetheless, our dataset represents the culmination of a fairly exhaustive consideration of the extant literature to identify which factors are (and are not) salient to the outcome of US House elections. If there are important omitted variables, they are not readily apparent to us and we presume they are not readily apparent to the majority of readers.

The results of this exercise are plotted in Figure 1. In the interest of space, we focus our discussion on the simple two-part measure of agenda control where successes and blocks (‘wins’) are combined and evaluated against the combination of disappointments and rolls (‘losses’). We discuss the four-part measure results in the appendix, but note here that the iterative modeling process finds no support for the hypothesis using that measure.⁸ We plot both the raw coefficient estimate for party win rate and its t-value for every specification possible with our data in the left panes of Figure 1. For the sake of comparison, we plot the same values for a relationship that the extant empirical literature is confident does exist – the positive relationship between performance and the spending gap – in the right panes of Figure 1. Beginning with the coefficient estimates on party win rates, the distribution is centered just off 0, with nearly all estimates signed in the predicted direction (positive), which is encouraging. However, examining the distribution of t-values shows that less than 1 per cent of estimates are even marginally statistically significant – fewer than we would expect by chance alone. Further, all of the model estimates that turn up a robust positive effect omit the lagged dependent variable and the majority-candidate characteristics such as incumbency, quality candidate, etc. These models are of course misspecified and would almost certainly never clear the peer review process. In other words, the only support for the hypothesis is buried in models that are plainly invalid.

If we take the ‘negligible effects’ approach to null testing (Rainey 2014), in which we consider a meaningful change in majority vote share given a first-difference change in win rate to be any value of 2 per cent or greater (a generous criterion),⁹ then the preferred model (Table 1) concludes that majority win rate has 0 probability of exerting a meaningful effect. Assessing all results from the universe of estimable models, that probability is 0.03. This is a conclusive null result.

We contrast these results with those in the right panes of Figure 1, which repeat this exercise for a relationship for which previous research has found robust support – the spending gap (for example, Green and Krasno 1988) – to demonstrate that the procedure above is not simply an exercise in misspecification, but rather a valid (though brute force) technique to demonstrate the model dependence (or lack thereof) of recovered correlations. Here, the predicted relationship is

⁷We also include either all candidate-level agenda variables or none in the four-part agenda model. This choice substantially reduces the number of possible models from 655,360 to 163,840. More importantly, though there are three covariates, they are conceptually a single compositional variable, as we discussed above.

⁸We recover the predicted rank ordering of coefficients in less than 0.1 per cent of the iterations, and we never recover the predicted rank ordering in which all parameters are positive relative to rolls.

⁹Victory margins are 2 per cent or less in just under 4 per cent of House elections between 1946 and 2010 (Eggers et al. 2015).

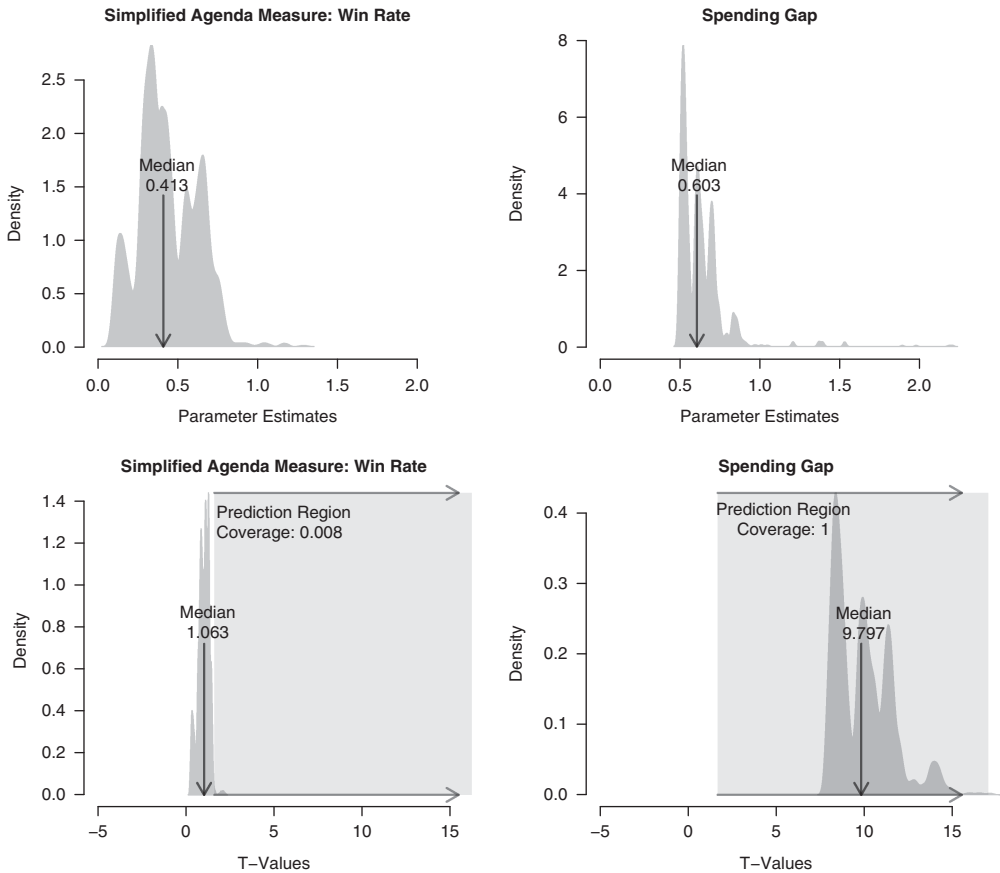


Figure 1. Iterative sensitivity probes of the null result

that majority candidates will win more votes as their spending relative to their minority competition increases: a positive relationship. As is clear from the figure, a positive, substantively meaningful and statistically significant relationship is recovered in every possible model specification. This relationship is so strong, so clearly present in the data and so model independent that no amount of misspecification can beat it out. However, agenda control is centered just off 0, only reaches traditional level of significance in the presence of gross misspecification, and has a 0.03 probability of exerting a meaningful effect.

Comments and Alternatives

In this section we briefly discuss how to interpret these null results, think clearly through the processes being modeled and consider alternative measurements that may provide a better fit to the data-generating process. First, we urge the reader to bear in the mind that the null results reported above do not indicate that the majority's policy record is irrelevant to its electoral performance, nor do they mean that the majority's collective roll-call behavior does not shape electoral outcomes. However, we *can* conclude from these null results that the majority's *aggregate* record of agenda control has no statistically discernible impact on the majority's *aggregate* electoral performance, contrary to the arguments of Cartel Theory.

We have come to believe (with the help of our anonymous reviewers and the editorial team) that thinking through the substantive process we are modeling may help uncover alternative

measures of the focal concept (agenda control) that are more appropriate to the question. Specifically, the implicit assumption of the tests above, following the explicit argument of Cartel Theory, is that voters *care* about the majority’s ability to control the legislative agenda. Therefore limiting rolls will disguise any potential cracks in the majority’s ideological foundation, and highlighting successes will signal the majority’s policy priorities and demonstrate its competence. Because the model predictions require that voters be attune to these outcomes, we want to think critically about the possible cognitive limitations of voters as well as the signal-to-noise ratio in the roll-call record. Since it is possible that voters only remember what the government has done recently, we may want to give priority to more recent votes over more distant votes. It is also possible that some votes receive more attention in the political discourse than others, and so we want find a way assign weights accordingly. Note that all analyses we discuss here are given in the appendix.

Our first set of alternative measures weights the roll-call votes according to their position in the electoral calendar following research by Lindstädt and Vander Wielen (2011, 2014) and several others. As there is no ‘natural’ or ‘built-in’ weighting scheme for factoring in proximity to elections, we calculate several variations and replicate our models with them. These weighting schemes also produce null results. We also use Lindstädt and Vander Wielen’s (2014) findings on individual members’ willingness to take a party-line vote as a guide, and cut off the agenda at the point when this willingness begins to drop off steeply in anticipation of an election, about 180 days prior. The intuition is that, because this is when individual members are most conscientious about the appearance of their behavior, this is also the period in which voters are most attune to congressional outcomes. This cut-off agenda estimate also produces null results.

One potential explanation for why these calendar-weighted results still fail to produce evidence for our empirical expectations could be that House members become increasingly consumed by district pressures in their voting behavior, subverting the will of their party in favor of the will of their district when the two are discordant, thus making it more difficult for the party to enforce discipline on the floor or reliably predict the outcome of various votes. This may reconcile previous findings on the importance of the legislative calendar in individual behaviors (for example, Lindstädt and Vander Wielen 2011) with our results that indicate the insignificance of the calendar in determining group performance while also explaining why the legislative agenda grows so sparse in this period.

The second potential explanation stems from a consideration of vote salience. Simply put, we do not believe all votes are equally important in determining the party’s brand: some votes should count more heavily than others. To explore whether failing to consider vote salience could be causing a Type II error, we have replicated the main models using a competitiveness-weighted agenda measure:

$$1 - \frac{win - 0.5}{0.5},$$

where *win* is the proportion of votes on the winning side and the potential *win* values (which must be greater than 0.5 and less than or equal to 1) constrain the measure $\in [0,1)$. This weighting assigns a value of 0 to unanimous votes and a value of nearly 1 to votes winning only a bare majority. Models employing this measure also produce null results.

Models in the appendix also consider rolls relative to all other outcomes as well as successes relative to all other outcomes and find null results. Finally, we note that models that constrain the sample to incumbent members of the majority party also produce null results on the covariates of interest.

The distribution of agenda outcomes is dominated by majority successes, so much so that it may be tempting to conclude that the distribution of non-success outcomes is simply noise and is not substantively meaningful. Indeed, the smallest proportion of successes in our sample is 0.917

(we have plotted these distributions in the appendix). However, drawing this conclusion disregards the significant amount of theoretically and empirically rigorous research demonstrating the importance of the variability in these outcomes, and that the distribution of agenda outcomes is significantly shaped by characteristics of the body – both its rules and its members. Stated differently, even if our analysis suggests that the distribution of the majority’s agenda outcomes is insignificant to its electoral performance, this does not mean that the distribution of outcomes itself is insignificant.

Discussion

In this letter we conducted an explicit test of the electoral implications of Cartel Theory and presented what we believe is strong evidence that the effectiveness of the majority party’s agenda control in the House has no direct effect on its electoral fortunes in the period of 1972–2008. Our findings comport with recent efforts to identify majority-party advantage in American state legislatures (Feigenbaum et al. 2017), but contradict the theoretical predictions of Cartel Theory. Though this does not suggest that the theory as a whole is without value – quite the contrary, the theory’s implications for legislative behavior have been empirically supported in several chambers, in several countries, dozens of times – our finding does present a challenge to one of the theory’s central premises.¹⁰ Assuming one believes our results, how should we reconsider the purpose of agenda control in Cartel Theory?

One possibility is that a party’s brand serves ‘defensive’ rather than ‘offensive’ purposes, setting a floor for the majority’s electoral performance by perhaps providing coverage for poor candidates, but never pushing its ceiling upward. Of course, these effects may be harder to detect in observational research. It may also be the case that the party brand is not a valuable electoral good, and that the shared benefits of agenda control are not electoral, but perhaps strictly policy-oriented. If this is the case, members may maintain the cartel to pursue shared interests while compensating so-called policy losers with other electoral resources, distributive benefits, and high-status positions within the party organization, as has been suggested in previous research (Carroll and Kim 2010; Jenkins and Monroe 2012).

Alternatively, it may be that agenda control does, in fact, determine the party brand, but the brand is not merely an indication of quality or competence. Rather, it is a signal that allows voters to infer the likely behaviors of candidates prospectively as well as retrospectively where information on legislative behavior is scarce (for example, Fortunato and Stevenson 2016). In this case, agenda control would have a mediated impact on electoral returns by clarifying ideological expectations. This, too, has been discussed in previous research, though evidence of the effects of the clarity of candidates’ or parties’ ideological position is mixed (Woon and Pope 2008; Tomz and Van Houweling 2009; Kim and LeVeck 2013).

Finally, our results are limited by our measurement strategy (as any results are, of course). Party brand may be made or lost based on just a few key legislative successes or failures each session. If that – or something close to that – is the case, then the variation across our measure will have largely been a noisy signal of the link between agenda control and party brand. If this is the case, then our null results may simply be a function of imprecise measurement. However, our measures are the standard ones used to assess Cartel Theory’s implications for legislative behavior and outcomes. Even with these caveats in mind, we are surprised, interested and motivated by these findings. At minimum, there is more work to be done in this area.

Supplementary material. Data replication sets are available at <https://doi.org/10.7910/DVN/KTGFRE> and online appendices at: <https://doi.org/10.1017/S0007123418000418>

¹⁰For a challenge to the more general body of statistical evidence in support of Cartel Theory, see Wiseman and Wright’s (2008) argument that some of this evidence is a statistical artifact stemming from the nature of their dependent variable.

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