**LaGina Gause**  
**Protest in Congress**  
**Fluxx Request ID: R-2211-40714**  
  
**Reviewer 1:**   
  
This project has several useful features, perhaps most notable would be an expanded database of protest activity among U.S. legislators. I see this project as being described effectively and that if completed it would make some useful incremental contributions to the legislative behavior literature. What is missing most in this LOI, that would need to be present in a full proposal, is a much clearer statement about the breadth of the contribution and the theoretical insights that would be garnered through completion of the project. The project focuses on a rather rare event. It strikes me that step one really is figuring out whether there are effects of these protests. So if the project could be narrowed to that question first, then the rest of the project might be an easier sell.  
  
**Investigator Response**  
We appreciate your confidence in this project and the advice to broaden the theoretical and empirical contributions of the project in the proposal. Indeed, our project seeks to contribute to the literature on legislative behavior and social movement with a comprehensive database of legislators’ protest activity while also providing theoretical and empirical insights into the nature of this understudied phenomenon. However, as you stated in your feedback, the breadth of the theoretical and empirical implications of the project was not fully articulated in our brief LOI. The full proposal makes several improvements towards those ends.

First, the proposal now theoretically stresses the imperative of protesting for legislators, particularly those who find traditional legislative behavior insufficient for achieving their reelection, policymaking, representation, or personal goals. That is, this project on legislators’ protest underscores an understudied legislative behavior that is crucial for many legislators’ survival. The proposal emphasizes that protest is especially useful for legislators of color, intersectionally marginalized legislators (along dimensions of race and gender), and those who are otherwise unable to legislate in an institution that was not built for them or their constituents. For example, the proposal notes that many women of color legislators are discouraged by their colleagues from taking credit for their legislative behaviors, so they practice tactical invisibility to get legislation passed (Hawkesworth 2003). They remove their names as sponsors or cosponsors of bills even though they played a major role in writing and progressing the legislation through Congress. For other legislators, protesting helps them advocate for policy issues that their colleagues deem to be unimportant. In these cases and many others, legislators’ choice to protest is a rational decision that enables them to credit claim, advertise, and increase their brand name among their constituents in hopes of securing their reelection goal (e.g., Mayhew 1974). Thus, while protest may be relatively rare compared to other legislative behaviors, we expect there to be substantive implications of protests for the ability of legislators to pass legislation, appeal to their constituents, or increase their personal or party’s brand name.

Next, the proposal now describes in greater detail the empirical research designs that will help uncover the implications, if any, of legislators’ protests. That is, we believe that the project is important if we are able to understand how women, racial and ethnic minorities, and intersectionally marginalized groups navigate the institution. But we also believe that protest can lead to positive outcomes for legislators who rely on it when traditional institutional behaviors fail them.

We begin by exploring Google searches and news stories on legislators before and after their protests (and compared to legislators who do not protest). That is, if a legislator’s protest makes constituents more aware of them, then constituents should be more likely to seek information about that legislator after a protest. Nevertheless, public attention does not necessarily translate into more constituent support or legislative capacity. So, we use survey data to assess how constituents respond to protests by legislators of different racial, ethnic, gender, and partisan affiliations compared to legislators with similar demographics who do not protest. The survey will help better assess the potential electoral benefits legislators may receive from protesting.

Similarly, if legislators protest to demonstrate their commitment to issues constituents care about, then after protesting, legislators should see more campaign contributions, even from constituents who are not in a legislator’s district. However, we specify in the proposal that we do not expect protest to correlate with positive outcomes for all legislators and among all constituents. Campaign contributions are an especially useful tool for constituents who are newly descriptively represented in Congress to demonstrate their support of a protesting legislator but cannot cast their vote in favor of the legislator.

Moreover, a legislators’ protest can be a means to increase their brand name within Congress by showing their colleagues they are the person to rely on to push for a specific legislation. Protest can therefore help legislators increase their ability to sponsor or pass legislation or land on committees that advance their goals.

We recognize that legislators’ protests could work at cross purposes, where they are able to increase support among constituents but do not have a similar effect on constituents’ voting behavior or legislative effectiveness. However, for legislators who are already incapable of passing legislation, receiving recognition for their labor, or attaining a leadership position within Congress, protesting leaves them no worse off within Congress. Accordingly, this project on legislators’ protests is an important theoretical and empirical contribution to studies of protests and legislative behavior primarily for its ability to understand the fundamental decision-making of elected officials, particularly those from groups marginalized within Congress. Further, the project’s benefits are in understanding the implications of legislators’ protests and how those implications influence future protest behavior.

**Reviewer 2:**   
  
The project proposed is very interesting, and the preliminary analyses point to potentially important (albeit perhaps not surprising) racial/ethnic disparities in protest by legislators. Figure 1 should be presented in percentages rather than counts, since as the LOI makes clear, what is at stake is the type of legislator who protests, not the total number of acts of protests. Also, how does party relate to protest (as shown in Fig 1a)? Do legislators protest more when their party is in power or not, do Democrats simply protest more than Republicans, or does particular legislation lead non-white legislators to protest more on average?

Extending the data to be studied beyond official misconduct and “words taken down” makes absolute sense. Is there a reason to believe that newspaper coverage will not be similarly biased, though? The PIs are right to note potential biases by ideology, race, and gender, but I also worry about biases in what will be considered 1) newsworthy/ sanctionable, and 2) an act of protest. In general, how will the PIs address the question of what counts as protest when it comes to Congressional speeches. Does “commentary on other protest events” count as protest? How can “protest-via-speeches” be distinguished from impassioned speech, speech by more effective orators, different styles of speech that can be correlated with personalities, occupations, race/ethnicity, and geography?

What I was most excited and frustrated by was the short paragraph about “consequences.” In the same way that Fig 1 was helpful, more preliminary information from the CMP would have been useful. How did it measure “how constituents perceive legislators who protest”? Were they given information about real legislators and their actual protest, or were they given something more like the proposed survey experiment? If the latter, how do the PIs plan to distinguish attitudes about the legislators from attitudes about the protest, or is the interaction of vital importance to their research questions? And, how exactly will campaign contributions be studied to learn how constituents react to legislators’ protests — will the PIs look at average donations before and after a protest? Will it be restricted to the district, or is this potentially an example when more national or symbolic constituencies may be relevant? Finally, how exactly will the PIs study whether protests make legislators more or less effective at policymaking?  
  
**Investigator Response**  
Thank you very much for your thoughtful feedback. We address each point below.

**First**, we appreciate and heed your suggestion to present Figure 1 in percentages rather than counts. Nevertheless, Figure 1A plots the raw counts of misconduct allegations against legislators by party and chamber from 2000-2020. We plotted this graph as a raw count rather than proportion because it shows the prevalence of these behaviors across chambers, which can serve as a baseline by which the subsequent graphs can be interpreted. Based on this, Democrats (across the House and Senate) were accused of misconduct 90 times over this period, while Republicans were accused 105 times. Please note that we changed the language around Figure 1 and the *Legislator Misconduct Dataset*. In the LOI we represented the figures in terms of “protest.” In this iteration, we are more conservative in our language, as we would like to complete further analysis/inter-coder reliability checks before claiming what instances of misconduct constitute protest.

Following your suggestion, Figure 1B plots the percentage of (unique) legislators with misconduct allegations for each party in the House of Representatives. The shaded regions indicate which party was in the majority. Generally, we see that fewer than 6% of legislators have misconduct allegations against them in each year, aside from 2021 where 14 % of Republicans were accused of violating security and mask-related mandates, which we discuss further in the proposal. This figure also shows that allegations are leveled against legislators in the minority and majority parties. Finally, Figure 1C plots the percentage of (unique) legislators with misconduct allegations for each party in the Senate, showing again that allegations are leveled against legislators in both the majority and minority parties. Misconduct allegations are overall less prevalent in this chamber compared to the House and occur more sporadically.

Now, given the caveat that misconduct allegation does not necessarily equate with protest, these preliminary findings cannot tell us exactly which parties engage in protest more than the other. But they do give indications of the relative size of legislators’ protests across Chambers and parties. With RSF funding, we can gain more accurate data on legislators’ protests by legislators’ party affiliations and assess how shifts in party leadership, heterogeneity within parties, or the size of the partisan advantage influence protest activity (Schmoll & Leung Ting 2023).

We also applied your suggestion to Figure 2, which explores the relationship between party, majority/minority status, and legislator race within Alexander’s (2021) dataset of Rule XVII (decorum) violations. We are confident that all such behaviors captured in this dataset are protests. Thus, Figure 2A plots the proportion of legislators in each party who engage in such protest across time and majority/minority party status. We find that Democrats had 159 decorum violations in this time period, while Republicans had 95. These violations, however, occur when a legislator’s party is in the majority *and* minority, indicating that such are methods by both conflict winners and losers. While we cannot yet speak to your question on if there are issue areas that prompt non-white legislators to protest more on average (our newspaper data collection will help with this), Figure 2B shows that the rate of protest among Black and Latinx legislators is higher on average compared to white legislators.

Theoretically, however, we expect that legislators will engage in protest over certain issues more than others. For instance, issues that are more ideologically extreme or fall outside of the political agenda will likely receive more protest because of legislators’ inability to make progress on these issues by using conventional means. Likewise, given that policy issues that impact stigmatized subgroups are often disregarded and require more efforts on part of their benefactors, we expect that such issues and policy areas will receive more protest action, particularly by legislators under race-gender subjugation (e.g., Rocca & Sanchez 2008).

**Second**, we agree with the reviewer that newspaper coverage is likely biased in a variety of ways. In fact, our theory supposes that legislators are aware of these biases and that the presumed biases enter legislators’ strategic calculations when deciding whether or not and how to protest. That is, whether legislators believe they will get favorable or unfavorable coverage, or any coverage at all, from a protest likely factors into if and how they protest. In a similar way, we argue that protest occurs in Congress *because* of inequalities experienced throughout the legislative and electoral process. Protest is a way for legislators to circumvent, expose, and overcome such inequalities to achieve their political goals. In this way, we want to collect the newspaper data to achieve a more comprehensive dataset of legislator protests than what is currently available while also respecting legislators’ awareness of the institutional and structural constraints they face both within Congress and the media.

However, we take seriously the variety of ways that news articles may be biased in their coverage of legislator protests. What newspapers deem worthy of coverage can be shaped by a host of factors, including societal events, demographic characteristics of the legislator(s) involved, and how disruptive or unique the act is. In an effort to overcome these biases, and in relation to your **third** point, we turn to the Congressional Record to collect statements made by legislators on the floor of the House and the Senate.

To begin, statements made by legislators on the House and Senate floor can count as acts of protest if they meet our definition of protest: (1) publicly observable and (2) disrupt regular day-to-day institutional functioning or violates institutional norms for the purpose of (3) communicating or achieving a political goal, as opposed to a strictly personal one. So, regardless of how impassioned a speech is or the different styles of speech legislators use, statements made on the floor of the House and Senate are only considered an act of protest if they meet our definition.

Additionally, we have laid out in greater detail in the proposal how we will analyze the Congressional Record to make our dataset on legislators’ protests more complete. One way we will do this is by identifying legislator protests that take place on the House and Senate floors. We will do this by conducting a keyword and phrase search. Often certain words and phrases, like “decorum,” “violate the rule,” and “preserving order,” are uttered by legislators in opposition to the protest. By looking at the prior statement, we can identify potential protests that go unrecognized in our other datasets because they include acts of protest that are not met with formal allegations (in contrast to the *Legislator Misconduct Database*) or sanctions (in contrast to the *Legislators’ Decorum Violations* data) and are not picked up by news media.

The other way we analyze the Congressional Record is by conducting a computer-assisted content analysis with RTextTools. Using the newspaper articles we identified as discussing legislator protests, we rely on RTextTools to predict whether or not a floor statement discusses a protest. Floor statements that discuss legislator protest are not necessarily acts of protest in and of themselves. Rather, they have the potential to expand our universe of protests by (a) associating new, additional legislators with a protest we were already aware of or (b) identifying a related protest that went uncaptured in our existing data.

**Fourth**, we appreciate the space and opportunity to fully discuss the consequences of legislators’ protests. Figures 3A-C identify the three survey questions we placed on the CMPS. Each respondent was asked one of three questions concerning their opinions on a legislative behavior (i.e., how respondents would feel if their member of Congress sponsored legislation, violated congressional rules, or were arrested for protesting in order to represent their interests). Respondents were given a five-point scale ranging from strongly approve to strongly disapprove on which to respond. Figures 3A-C depict preliminary findings from regression analyses. Here, we see that some constituents, primarily Democrats and Black Americans, approve of legislators who violate institutional norms and are arrested while protesting for their interests. However, the reviewer is correct that we are unable to distinguish attitudes respondents have toward their legislator and their approval/disapproval of their legislative act. Moreover, our initial survey is unable to assess how legislator demographics, as well as the respondents’ demographics, influence their approval/disapproval of a legislator’s protest. With the support of RSF, we will conduct a survey experiment that oversamples Black and Latinx participants and allows more variation in the legislators’ characteristics (e.g., race, party, gender, protest tactic, issue) to assess our expectations for how constituents respond to legislators’ protests.

**Fifth**, we view campaign contributions as one way to gain greater understanding of the electoral consequences of legislator protests. We do not intend this to be a causal analysis since we only have correlational data and cannot confidently say whether changes in campaign contributions were due to a legislator’s protest or some other factor. However, we are able to get a better understanding of whether the legislators’ protest corresponds to (a) an increase or decrease in campaign contributions and (b) a change in who is contributing to their campaign. As stated in the proposal, we will use the Database on Ideology, Money in Politics, and Elections (Bonica 2016) to evaluate campaign contributions both from within and outside of their congressional districts. We will look to see if there is a correlation between a legislator’s protest and an increase or decrease in the (a) number of constituent contributions and (b) average amount contributed. Based on geographic location, we can probabilistically estimate the contributor’s race or ethnicity since neighborhoods remain racially segregated in the United States. We expect that legislators from historically marginalized groups will see decreases in campaign contributions from the average campaign contributor but will see increases in contributions from groups they descriptively represent. We expect these differences in contribution changes to be even stronger from constituents not in a legislators’ district since they may be most wanting of representation, given their general lack of descriptive representation, even in the absence of substantive representation. Moreover, since the DIME data includes a variable for ideology of the contributor, we can see if a similar pattern exists for legislators with extreme ideological positions.

**Sixth**, again, we do not intend this to be a causal analysis, as we only have correlational data and cannot definitively say whether changes in effectiveness in lawmaking are due to legislators’ protests. However, we are able to get a better understanding of whether legislators’ protests correspond to changes in different measures of legislative effectiveness. There are two primary ways we try to capture legislators’ effectiveness in lawmaking. The first relies on the Legislative Effectiveness Scores developed by Volden and Wiseman (2014). These scores take into account the number of bills a legislator sponsors, the number that make it to committee, the number that gets through committee, the number that gets passed, and the number that gets enacted into law. The more effective a lawmaker, the greater their score. By looking at their effectiveness score before and after their protest, we can see if engaging in protest corresponds with changes in their effectiveness as a lawmaker. However, considering the growing reliance on omnibus legislation (Casas et al. 2020; Krutz 2005; Sinclair 2016), along with the tactics that marginalized legislators use, like women of color’s use of “tactical invisibility” (Hawkesworth 2003), the Legislative Effectiveness Scores miss important ways that legislators effectively engage in policy making. As a result, we also consider the Lawmaking Productivity Measure created by Eatough and Preece (n.b.). which relies on a text reuse measure that determines if the language used by legislators in their sponsored bills and amendments is included in omnibus legislation. We look to see if more or less of their legislative language can be found in omnibus legislation before or after protesting. This allows us to capture more unorthodox (Sinclair 2016) ways in which legislators engage in lawmaking that LES overlooks.

**Reviewer 3:**   
  
This is a creative and timely proposal for work on legislative protest. AUs are well-qualified to carry out the research. The idea of studying legislator protest is one of the most compelling proposals I have read in a while given how often something that could be categorized as legislator protest is covered in the news with little social science context with which to understand the event.

I have one suggestion, which is that AUs seem to have created their definition of protest without a lot of reference to the social movements literature and that seems like a missed opportunity both intellectually and for the purposes of building their audience. Would the work of McAdam, Tilly, or even Hirschman (Exit, Voice, Loyalty) not enrich our understanding of what individuals or groups of protesting legislators are doing when they protest within their party or caucus? I can only assume based on AUs' expertise that they are very familiar with these literatures and perhaps just omitted those reference for brevity. I think using some standard theories of organizing and protest techniques would be useful for highlighting the originality of their contribution.  
  
**Investigator Response**  
Thank you for your thoughtful and encouraging feedback. We, too, think this project is compelling and are surprised by the dearth of theoretical inquiry thus far on legislators’ protests. We appreciate your recommendation to rely on the social movements literature to ground our theory of legislative protest. We definitely did not intend to imply that our definition of protest was created without consulting and building upon existing social movements literature, and we had been contemplating ways to incorporate it more since, as you mention, not doing so is a missed opportunity for highlighting the originality of this project’s contribution. We believe the social movements literature, like the legislative behavior literature, provides insights into legislators’ protests but is insufficient in explaining fully why legislators protest and the consequences of their protests. In the proposal, we take care to recognize the strengths and weaknesses of the literature in understanding legislators’ protests. Most of the discussion is in the first six pages of the proposal, but we respond directly to your suggestions below.

McAdam, Tilly, and many other social movement scholars have made tremendous strides in our understanding of citizens' protests. Generally, there are four broad reasons why citizens protest: grievance theories, resource mobilization theories, political opportunities, and issue framing. McAdam (1982) argues that political opportunities only mobilize individuals when subjective meanings are attached to political shifts. In focusing on citizens’ incentives to protest, much of this literature has only implications for legislators’ protests. Indeed one reason the protest literature is limited in fully explaining legislators’ protests is that legislators’ protest is elite behavior that is not necessarily motivated by instrumental gains, often has elements of public posturing aimed at appealing to broader audiences, and is responsible for much of the political opportunities and resources that motivate citizens participation.

Tilly’s (1978) work on repertories of contention has, perhaps, the most direct implications for legislators’ protests. His work suggests that protest tactics are constrained by time, space, and group characteristics and that activists borrow tactics they deem successful. It implies that legislators from a tradition of protesting are more likely to see protest as a valuable tool for achieving their goals. The proposal recognizes repertories of contention as a likely motivation for some legislators’ protests.

Relatedly, Hirschman's (1970) exit, voice, loyalty model broadly attempts to model how members of an institution respond when they feel their needs are not being met. They can voice their concerns in hopes that the institution will be responsive to them. Or they can exit the institution if they do not think the institution will be responsive. They may choose to do neither if the costs of exiting or voicing their concerns are too high or if they have a sufficient amount of loyalty to the institution. In the case of legislators’ protests, this model could be useful in explaining legislators’ decision to voice (protest) or remain loyal to the institution. However, voice is a violation of institutional norms that most legislators attempt to avoid (Alexander 2021), and exit, an essential component of the EVL model, is rarely a consideration for legislators. Nevertheless, we appreciate the suggestions to incorporate these theories since they help us to develop a comprehensive theory on legislators’ protests that helped us enhance our proposal and will enrich any of our future publications on legislators’ protests.

Building on the legislative behavior, social movements, and comparative politics’ parliamentary brawls literature, in the proposal we perceive legislators’ protest as a political lever that legislators pull when traditional, institutional tools fail in helping them achieve their reelection, personal, or political goals. Legislators are rational actors who protest to 1) appeal to a broader constituency and 2) achieve tactical or legislative goals. Legislators weigh the costs and benefits of protesting relative to other legislative behaviors when seeking to achieve their goals. Nevertheless, we perceive parliamentary brawls and resorting to violence to be a more costly and extreme action than the overwhelming majority of legislators’ protests we analyze.

**Reviewer 4:**   
  
This is a well-conceived proposal that addresses a pressing issue in society. It is a novel idea. And I have not seen other’s working on this issue. There is a clear need for a new data set—which the authors propose to collect—and their novel data will allow them to answer multiple research questions. The proposal stands out as both proposing a novel empirical approach and being able to make exciting theoretical advancements.

The intersectional focus of the proposal is particularly promising. I would consider engaging other work that theorizes that members of marginalized groups engage in different types of behavior to overcome their minority status in the chamber. Work on collaboration, for instance makes arguments about why women and minorities are more likely to collaborate owing to their marginalized status. This seems relevant to the theory being developed herein.

I encourage the authors to also consider comparative politics research on legislative behavior, particularly work on parliamentary brawls, which seems to be consistent with the definition here.

Obviously, the page limits prohibit the authors from engaging this work here. I look forward to seeing the next stage of the research where the authors can engage a broader range of literature.

The data is extremely promising and will results in a number of projects beyond the ones proposed herein. The data will be widely used and well cited.

The proposal fits well with Russell-Sage mission.

The PI is extremely well positioned to execute the research.  
  
**Investigator Response**  
We appreciate your confidence in the project and your advice to broaden the literature with which we should engage. We have taken these suggestions seriously and they have greatly informed our new proposal. For instance, on pages 4-6, we discuss in greater detail what previous work has found on how structurally disadvantaged (i.e., by race, gender, race-gender) legislators navigate and overcome barriers to their full participation in Congress. As you said, previous work has noted that legislators who experience intersectional marginalization within the institution must engage in different types of behaviors, compared to relatively privileged members, to overcome their minority status in the chamber. This minority status stems from two sources, primarily: 1) Legislators who are intersectionally marginalized tend to represent interests that tend not to be “perceived as important by the dominant population,” thus influential actors in Congress have little incentive to address the policy priorities of such legislators (McClain 1993, p. 2) Intersectionally marginalized legislators may lack influence in Congress because it is a racialized and gendered institution, reinforced by racial and gendered cleavages that prevent the consolidation of significant coalitions and downplay the value of racialized and female members’ perspectives (e.g., Preuhs 2006; Hawkesworth 2003; Dittmar, et al. 2022). Thus, in the face of this minority status, intersectionally marginalized legislators must engage in legislative activities often beyond the requirements of relatively privileged members.

In response to these structural barriers, legislators 1) deploy different procedural tactics (e.g., tactical invisibility; Hawkesworth 2003) and 2) do *more* than their colleagues across a variety of legislative behaviors–from giving speeches to collaborating at higher rates to releasing more newsletters (e.g., Pearson & Dancey 2011; Swift & VanderMolen 2021; Blum, et al. 2022). For instance, previous work has revealed that women in Congress, given their perception that they must work harder than their male colleagues to establish credibility with press, colleagues, and constituents, speak on the House floor at greater rates than congressmen (Pearson & Dancey 2011). Women also collaborate more than men (Swift & VanderMolen 2021), and legislators who share minority status in terms of race, ethnicity, or gender cosponsor bills with each other at relatively high rates (Bratton & Rouse 2011). These trends are likely to due to such legislators’ exclusion from other forms of power and gendered expectations of women to collaborate (Bratton & Rouse 2011; Holman, Mahoney & Hurler 2021)

Because racism and sexism prevent such legislators from using conventional legislative actions to advance their policies *and* because such legislators are likely to have a deep commitment and “sense of responsibility” to constituents who are disadvantageously situated outside of Congress (Dittmer et al. 2022), we expect that intersectionally subjugated legislators will engage in more protests. However, our theory does not claim that *only* intersectionally marginalized legislators will engage in protests. For instance, on page 6, we argue that extreme idealogues, legislators seeking advancement in their party, and parties generally may engage in protest. This development is thanks to your suggestion to explore comparative politics literature on parliamentary brawls, which then pointed us to an additional CP literature on “legislative disruptions.”

We include citations such as Phadnis (2021), Batto & Beaulieu (2020), and Schmoll & Leung Ting (2023) to discuss why ideologically extreme legislators may be more likely to engage in protest, borrowing their insights from parliamentary brawls/disruptions. For instance, legislators may protest to pressure party leaders and key veto players toward their policy preference (ibid.). Similarly, this literature prompted us to consider that legislators may protest at the behest or encouragement of parties, such as in the case of India’s parliament, explored by Phadnis (2021). Indeed, Phadnis (2021) argues that parties may better reap the benefits and deflect the costs of an affiliated member’s protest than an individual protester without party coordination. As a reward, protesting legislators can gain party resources. Gandrud (2016) finds that brawls are more likely when parties hold insecure majorities or when there is increased party fragmentation (i.e., less cohesion). This implies that both winners and losers can use protest. Minority parties may protest to stall legislation or change procedures they dislike, while majority parties may protest to prevent lowers from doing things that constrain their power to control the agenda and policy outcomes. The likelihood of protest is thus higher when there is the credible chance for party control to switch at the next election (Lee 2016).

We are appreciative that your review suggested further consideration of this literature. We believe that our theoretical and conceptual contribution of “legislators’ protests” will allow us to synthesize across these literature – considering racialized-gendered norms and institutional dynamics, conditions of majority/minority party status, and legislators’ individual incentives all as explanations of this legislative behavior. Ultimately, we theorize that legislator protest is a behavior of those who, for various reasons stated above, cannot use institutional levers to achieve their electoral or policy goals. This argument allows us to explore this legislative behavior primarily in light of intersectionally marginalized legislators while also explaining why “winners” and the relatively advantaged in Congress may protest.