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Dear Editors and Reviewers,

We would like to thank you all for another opportunity to revise our manuscript, “An Empirical Evaluation of Explanations for State Repression” (Ms. No. APSR-D-13-00597). We have made several changes to the paper to reflect the comments we received from the editors and reviewers. We believe these changes have helped us better frame the contribution of the paper as well as remove some text that distracted from our main points, and that the manuscript has improved as a result. We hope the editors and reviewers agree.

These changes include added discussion of the connection between our results and the existing (theoretical and empirical) literature on state repression, added discussion of the general theoretical orientation of most research on state repression, a more full discussion of our results regarding specific (disaggregated) indicators of repression, and toned-down language in our discussion of the measurement issues with civil war and Polity. In our responses to the editors’ comments below we briefly discuss each of these changes, and discuss them in greater detail in our responses to individual reviewers.

We thank the editors again for the opportunity to revise the manuscript, and the reviewers for their extremely helpful comments.

Sincerely, The Authors

## Editor Comments and Responses

1) The editor writes: Although the reviewers agree that you have done this to a good degree, they would like you to go even further in explaining the broader relevance of the paper. Please refer specifically to point 2 of reviewer 1’s comments, although reviewer 2 also mentions this.

### **Response:**

We have better connected our results to the existing literature on repression, most notably the results with respect to civil war and democracy, and we discuss the broader literature (both theoretical and empirical) on dissent/repression (pp. 6-7). We also more clearly connect our findings about international factors to previous research, and further contrast these results with those for domestic factors (pp. 31-33, 40). We think these changes help to connect our paper to the literature more broadly, and clarify how our results speak to previous empirical

and theoretical work on repression.

2) The editor writes: Show a bit more clearly the theoretical orientation of the work cited to better position the paper within the relevant literature. Reviewer 1 asks for this explicitly, but reviewers 2's comments point in the same direction...

**Response:**

We have added a fair amount of text explaining the theoretical orientation of existing work (pp. 6-7), which we think will help readers see the connections between the covariates chosen for our analysis and better connects our results to past research. We have also added, at several points throughout the text (pp. 6-9, 15, 27, 30), short descriptions of some of the theoretical arguments that led researchers to include different variables in their models, which also helps connect our results to previous work.

3) The editor writes: Explain a bit further the tradeoffs between, and implications of, research that focuses on specific (sub)types of repression. See especially point 3 of reviewer 1 regarding this.

**Response:**

We have also added some more discussion of our results for specific sub-types of repression. We discuss the fact that disaggregating different types of repression is fairly uncommon (pp. 5-6), and discuss at-length the differences in our results across different indicators of repression (pp. 29-30, 40) and the implications this has for past and future research (pp. 40).

4) The editor writes: Both reviewer 1 and 2 ask that you make your conclusions somewhat more nuanced.

**Response:**

We have toned down/removed some of the language used in our discussion of the measurement issues with regard to Polity and civil war (pp. 3-4, 7, 22, 26-27, 36-38), which we think addresses this point.

## **Reviewer 1 Comments and Responses**

1) Reviewer 1 writes: provide some summary statement of the guiding theoretical orientation for most of the work identified (e.g., rationalist, structuralist, etc.), work through what the basic argument is and how this relates the variables, hypotheses and models selected. This

is not addressed at all in the current version and thus we are left without an understanding of why prior researchers did what they did.

**Response:**

We agree with Reviewer 1 that the previous manuscript lacked any indication of a theoretical framework that connects the various pieces of the literature we discuss. We have added a substantial amount of text to the literature review describing the theoretical framework (pp. 6-7) adopted by most of the literature. We characterize this approach as an informal decision-theoretic/expected utility approach, and connect this especially to theoretical/empirical work on domestic influences on repression, primarily dissent and democracy. We also integrate this into our discussion of international factors, noting how the theoretical motivations for this work relate to those for research on dissent/democracy, and explicitly walk through one existing argument about the relationship between international economic standing and repression (p. 9). We have also mentioned at several points past researchers' theoretical motivations for including in models of repression measures of youth bulges (pp. 8, 15, 27), oil rents (p. 8), and a lagged dependent variable (p. 30). These were mentioned in the previous draft, but we thought it was important to remind readers of the theoretical reasons these measures are being examined as they perform quite well in the analysis and are mentioned at several points throughout the text.

We believe all of these changes will help readers understand why past researchers included measures of these concepts in their models, and also help readers to see the connections between the various covariates we chose to include in our analysis. This also partially addresses Reviewer 1's second point about the theoretical implications of the variables included and associated results.

2) Reviewer 1 writes: related to this, what are the implications of the compilation of variables and the theories associated with them. there is a comment buried on page 23 that there are important policy implications involved but this is part of the picture. the dominant theories in political science are involved in the discussion of state repression and the operationalizations/concepts in this community are drawn from the larger literature. the piece still reads as isolated. good references/acknowledgement to weingast et al.

**Response:**

We have added some text to the last paragraph of the introduction (pp. 3-4), the literature review (pp. 6-7), and the conclusion (p. 35-36), specifically connecting our results with respect to civil war and democracy to what scholars typically

regard as the primary findings in the repression literature (i.e. that dissent and democracy affect repression). We note that the importance of these results must be tempered by the measurement issues we discuss, but are careful not to disregard these findings generally since it is possible to examine these relationships in a meaningful way. In this regard we discuss (pp. 6, 35-36) the sub-literature on dissent/repression which typically uses more micro-level data on dissent that does not suffer from the problems associated with measures of civil war. We also mention (p. 7) work that disaggregates democracy into its constituent parts and discuss (p. 37) the result we obtain using the executive constraints component of Polity, which is not subject to the measurement problems that affect other parts of Polity. We also mention in a footnote on p. 37 that the executive constraints component is sometimes treated as an indicator of judicial independence, which further supports the importance of that concept. This also addresses Reviewer 2's concern about downplaying these results (more on this below).

Additionally, we speak more (pp. 31, 33) to the divide between studies of domestic/international influences, highlighting in particular the inconsistent findings with respect to international influences which has led some scholars to view this work as less important than work on domestic behavior/institutions and repression. We discuss more fully the connection between our findings and previous research, emphasizing (p. 40) that some of this inconsistency is due to the fact that research examining international influences does not allow for the more complex (i.e. interactive/nonlinear) relationships between these influences and repression that our analysis examines. We also note (pp. 33) that our result with respect to trade openness in fact lends support one of these inconsistent findings, and stress that work in this area should examine/allow for complicated relationships between repression and covariates of interest.

Finally, we have added a paragraph to the conclusion (pp. 40-41) that summarizes what we hope to accomplish with this paper with respect to the literature on state repression: to determine which hypotheses receive the most support, and to use these results to sort through existing theoretical/empirical work and offer useful advice about how this body of research can progress. Importantly, we emphasize that we hope our results will help to *inform future work* rather than be treated as the last word about the “true” causes of repression, which could discourage further work. In our opinion scholars too often treat their results as definitive when they should regard them as tentatively confirming a belief that must be subjected to many, many more empirical tests.

3) Reviewer 1 writes: i feel that the disaggregated stuff is just tossed in at the moment. the literatures has people that study indices like pts and cirri (e.g., poe/tate and cingranelli and richards), there are people that study single repressive actions (e.g., hathaway), there are people that study distinct combinations (e.g., personal integrity as well as civil liberties restrictions [davenport]) and some work that looks at a bunch of different types (e.g., cingranelli and richards). there is no discussion of the different approaches or the implications of the different approaches. this should be done in order to better contextualize your contribution.

**Response:**

We agree that this warrants more discussion than was contained in the previous manuscript, and we thank the reviewer for pushing us in this direction. In our view, there is very little work that disaggregates repression into different types (we highlight work on torture (pp. 5-6) as the exception), though our results indicate that this should be more common. In the introduction (p. 4) we present the unevenness of our indicators' performances across the different measures as one of our main findings, and use this finding throughout to suggest that disaggregation should be more common. We note the general lack of this approach in the literature review on pp. 5-6. We have also added a footnote (p. 6) citing some of the sociological literature on variation in repressive tactics. We note that the typologies used in this literature are distinct from those that inform the PTS/CIRI data, thus there are many hypotheses about variation in state tactics that we are unable to examine, though these hypotheses developed in a different literature than the one we speak to. We have also added a footnote on page 18 citing a study by Cingranelli and Richards that examines the CIRI scale using formal measurement techniques. We note that we do not challenge their findings concerning the scalability of the CIRI index, but rather wish to stress that there is no reason to assume *a priori* that the determinants of repression will affect each component of the scale identically.

We also discuss the implications of this finding in much more detail, first on pp. 29-30, where we note that it speaks to a need for greater theoretical refinement, and again in the conclusion (p. 40), where we make this point in more detail. Specifically, our results suggest that the covariates examined by researchers influence different types of repression to different degrees, but are generally assumed *a priori* to affect all repressive practices in the same manner. We also cite some work that examines whether the practices measured by repression indicators are generally substitutes or complements.

We would like to thank Reviewer 1 for his/her helpful comments and suggestions.

## Reviewer 2 Comments and Responses

1) Reviewer 2 writes: I take issue with the discussion of the conceptualization/measurement problems of civil wars and democracy. The empirical estimates suggest that civil war, political competition, youth bulges, judicial independence, and natural resources have the most predictive value in terms of increasing accurate prediction over basic models. Of these five measures that are shown to have predictive value, the authors take particular issue with the coding of civil war and political competition, arguing that the measurement of these concepts include the very concept of repression and therefore should not be used as control variables in models predicting repression. In other words, they do a post hoc analysis in an attempt to undermine the estimated results that these variables have predictive value. While I do not disagree that there are serious issues with using measures with such overlap, I believe that the authors are overstepping the bounds of their analysis and the claims they can make for future research.

The purpose of this article is to evaluate the ability of \*specific measures\* to add predictive value to baseline models predicting repression. While the authors do spend a bit of time discussing the concepts that the measures approximate in the literature review, all these models do is assess whether these particular measures add predictive value to general models. The analyses here do not: (1) assess the value of \*concepts\* for predicting repression (which no empirical model can do definitively, of course), nor do they (2) evaluate \*explanations\* or theories of state repression (which the title explicitly claims).

This would seem to be a nitpicky point, but the language used throughout the paper is slippery with these distinctions. If the authors choose to go into why these MEASURES are not appropriate, they should take great care not to suggest that the CONCEPTS are not appropriate. For instance, there are ways to demonstrate that violent dissent leads to repression that is not reliant on using civil wars as the variable (Davenport 2007 book, Conrad and Moore 2010, Danneman and Ritter Forthcoming). And while political competition is problematic as coded by Polity, that's not to say that democracy (or more likely, its component parts), is not useful as a concept, as the authors point out with the DeMeritt & Young example. The authors make very strong claims in the introduction, literature review, and the discussion of the empirics that suggest they have results that overturn these concepts, when \*the findings are that they predictive power\*.

Furthermore, the focus on CW and polcomp, while not unwarranted, seems a bit ad hoc. What led the authors to focus on these variables that demonstrated high predictive value,

but not on youth bulges or natural resources? The purpose of the paper is not to assess all of the 30-odd measures that we use in terms of their coding or conceptual appropriateness, and the authors do not do so systematically. Yet the language used in the lit review ("we argue?" as if it is a critical point of the paper) and elsewhere suggest the authors are putting meaningful importance on these arguments that are neither fully fleshed out nor a systematic study across the measures used. For instance, on page 6, the authors write, "This claim is strongly supported by our analysis, and we discuss the implications of this problem in more detail below"?the phrasing of which implies that the authors have empirical evidence that repression affects political competition, which they do not. They \*infer\* that the strong results are because of the correlation, and they point out the problems in the coding.

I want to stress that I don't disagree with their points, but I question the appropriateness of the post hoc theorizing and the strength of these claims FOR THIS PAPER, given the actual goals and analyses therein. It seems to me that this analysis would be better shortened - serve as a warning - and reserved for more careful analysis in another paper when the authors can consider the concepts at work and alternative ways to measure them. Additionally, the authors should take care regarding language slippage between concepts and measures regarding the claims they make on these two particular variables of concern. I also think that the title should be something like "An Empirical Evaluation of Predictors of State Repression" (or similar), since the authors are not evaluating theories/explanations and make it explicit that this is not their goal.

Finally, as an aside, I think the point about international factors having more complex relationships than scholars tend to propose when including them in their models is rock-solid and so very cool. Way to be.

**Response:**

We agree with Reviewer 2 that our analysis cannot speak directly to the conceptual issues we discussed in the previous draft. We have changed the language when discussing this issue to reflect concerns about measurement/operationalization issues with Polity rather than concerns about the concepts themselves. We also discuss the problems with civil war in terms of measurement, and note (p. 36, Footnote 48) that studies which measure violent dissent itself (rather than civil war, which is a combination of gov't dissident violence) are generally sound, and we cite some of this work in the literature review (p. 6).

With respect to democracy, we go a bit further in discussing the results pertaining to democracy so as to not downplay its importance (pp. 27, 37), but we do note that the results with the Polity scale have to be viewed in light of the measurement issues. Our points in the conclusion about civil war and Polity are

largely unchanged but, again, everything is discussed in terms of measurement, and we limit our conclusions about how to conduct analyses to Polity and its subcomponents rather than democracy as a concept. We think our conclusions are strongly supported by the analysis we present as well as the passage quoted from the Polity codebook.

We also agree with Reviewer 2 that the way these points were presented in the previous draft made them appear ad-hoc and so distracted from our main points. We thank the reviewer for pointing this out. We have removed language like “we argue,” “our claim is strongly supported,” “this result is consistent with our argument,” etc when discussing this point.

The one point we disagree with concerns the title of the paper. We would argue we are evaluating the veracity of existing theories by seeing how empirically accurate their implications are. We have therefore chosen not to change the title of the paper, though we have changed language at the end of the literature review (p. 11) to say that we are evaluating *empirical implications* of theoretical arguments/explanations. This reflects R2’s concern that we are not evaluating the arguments themselves. This is true; we don’t evaluate the logic of theoretical arguments, and some of these theoretical arguments are not based on explicit deductive reasoning which one could formally evaluate (though we do cite some formal work on pp. 6, 36). But they are the theories/arguments that exist in the literature, and their empirical accuracy (which we are examining) is usually assessed with a test of statistical significance. Our point is that this is inadequate, and we wish to evaluate these theoretically motivated hypotheses in a better way.

We would like to thank Reviewer 2 for his/her helpful comments and suggestions.