

February 5, 2009

Marianne Stewart, Editor  
American Journal of Political Science

Dear Marianne:

Much thanks for allowing us to respond to the second round of reviews on our manuscript, A General Empirical Law of Public Budgets. In this memo we will first note a couple of relevant points in Reviewer 3's comments, and then concentrate on Reviewer 1's remarks, as you requested. [Reviewer 2 is satisfied with the manuscript.]

Reviewer 3 has some difficulties with the 'wind up'—basically that it is too distracting and long-winded. We agree; that was added in response to other comments and it can be shortened. He/she is quite correct that models of policy change should be brought up front in the discussion. The second point is the fuller discussion of the measurement of institutional friction. We appreciate the willingness of the reviewer to be flexible on this, because as the reviewer notes this is an on-going theoretical and empirical project and cannot be done simply in a single article that also has another important empirical point. We think that the first step is, as the reviewer notes, the establishment of generalization. We will take one more cut at clarifying this specific role of the paper, should it be accepted.

Reviewer 1 is a superb and thorough reviewer whose opinion we respect. Nevertheless, we have some fundamental disagreements about how to proceed in the kinds of situations such as the one we are trying to develop in this paper.

First, we appreciate the first paragraph of the review, where Reviewer 1 applauds our general research direction and our technical capacity regarding what we set out to do. Second, we really were seriously heartened by the last phrase of the second paragraph, in which the reviewer wishes us the very best in our research program.

Here is where we believe we differ. Our model of an 'empirical law' is the establishment of a firm general finding that is amenable to causal analysis. Now the reviewer may well be right that the term 'law' is either misleading or maybe even incorrect, and the correct term would be 'empirical regularity' (although that seems to underestimate the contribution), but we do not feel strongly about that language.

However we will note that the Guttenberg-Richter Law of earthquake magnitudes (note that it is always referred to as a 'law') was established long before the causal mechanisms were traced. Indeed, many aspects of the complex causal dynamics underlying the G-R Law are still not understood. Yet it was an incredibly important generalization that has stood the test of time and formed one of the central 'facts' that must be explained by any model of the dynamics of earthquakes. Any model that cannot generate the G-R pattern must be eliminated from consideration.

Is our analogy (not 'metaphor') between earthquakes and institutional dynamics insightful? Or unscientific? We think the former. The dynamics of earthquakes are classic for complex systems, and complex systems often display dynamics whose underlying causal patterns are ill-understood. We think the operation of political institutions should be similarly viewed. We are much more firmly in the complexity science (as NSF now calls it) camp than is Reviewer 1, which we think accounts for at least some of our disagreements.

We wish we had never had the idea of the EITM remark. It was ill-advised and distracting, and it will disappear in future versions.

However, we have a more fundamental disagreement over the manner in which this law or generalization is established. Reviewer 1 thinks we should control for all internal political and institutional dynamics in these countries. His/her argument is that our conceptualization is not valid unless 1) we subtract out all possible confounding variables within countries; 2) re-calculate the residuals from this analysis; and 3) check to see if the residuals are Gaussian, which the reviewer claims would vitiate our institutional friction notion.

We disagree on both practical and more fundamental grounds. On practical grounds, this approach is not possible, due to lack of much of the relevant data and a strategy for adjusting the model to incorporate this approach. On fundamental grounds, we think it more important to establish a valid 'law' or generalization than to rush out and try to establish causal patterns prematurely.

We have, however, offered what we would suggest is a firm foundation for more work. We show that the pattern holds generally across various institutional contexts. We also show that it differs meaningfully across countries. The differences correspond to differences in institutional friction, and not as we would predict based on the effects of party control. For example, in our sample of countries, non-normality is not greatest in majoritarian, parliamentary systems, where the effects of party control of government should be particularly pronounced.

The extent to which this is true of course requires (much) more empirical research; this is suggestive rather than definitive. Is this 'unsupported' and speculative? Or is it one step toward an understanding of a very complex process? We think the latter. Reviewer 1 tends toward the former.

In addition, there are a number of interesting within-nation institutional and political-economic conditions that might be interesting for further analysis. It might be worth exploring whether punctuations in spending are systematically related to partisanship, elections, and so on. But we cannot do this systematically, or at least in sufficiently similar ways, across such a wide range of countries. The fact that we find similar distributions in countries with such great differences in institutional and political-economic conditions makes less likely the possibility that these

distributions would change dramatically, and in similar ways, by taking into account these very different conditions.

In sum, we have firmly established a general law (in the same sense as the Gutenberg-Richter Law) and have begun to point to a direction of research to explain it. That is what we wanted to accomplish in this paper, and after reading the second round of reviews, we think we have done just that.

In addition, we believe that a major way that science progresses is through re-formulation of old understandings. The reviewer is clearly a skeptic on this, as noted in the remarks on 'observational equivalence'. Our notion of friction (and stick-slip dynamics) doesn't refute static institutional analysis so much as it incorporates this into a broader frame. This clearly sacrifices some things, but it most definitely illuminates others. Most importantly, it offers a comparative frame of reference that may (or, in all honesty, may not) advance comparative policy studies.

In the end, we may have an irreconcilable difference with the reviewer on how to proceed. This does not mean that Reviewer 1 is wrong; neither does it mean that we are. Indeed, the reviewer dispensed of any sins of incorrect analysis in the first paragraph. We just disagree on how to proceed.

We agree most fundamentally with Reviewer 3 who describes his final and revised understanding of the niche that this article will fill. It does not end the discussion; on the contrary we expect that it will begin or continue one. We have established a widespread empirical regularity here, and one that we suspect will generate considerable response in the literature. For one paper we think our contribution is quite substantial here already and we are pleased that the reviewers do as well, even understanding the substantial reservations of Reviewer 1.

Finally, it is possible that this article will prove controversial. We would argue that that is one of the roles of scientific journals (an argument we strongly suspect the editor has some sympathy with, given both the original opportunity to revise this, and the second opportunity to provide this memorandum).

Sincerely

Bryan Jones (on behalf of the collective enterprise)