October 28, 2008

Marianne Stuart, Editor
American Journal of Political Science

Dear Marianne:

My coauthors and I wish to take advantage of your offer to revise and resubmit our manuscript, "A General Empirical Law of Public Budgets", and include the revised version here. In this letter I will detail how wee have responded to the reviewers.

The most important revision we made was in response to Reviewer 3's general objection that 'the paper starts to promise more than it delivers'. We think this was a fair criticism, and indeed the approach we used in developing the paper implied exactly what the reviewer suggested. We believe that the data analysis is convincing on the robustness of the power law for budgets—it survives in a variety of situations, including a number of national budgets and sub-national budgets. Second, the power exponents for the distributions differ from one another in what are likely to be important ways (although there is no available statistical test for differences among exponents; indeed we may be the first research team to raise the possibility of detecting differences in systems. We believe, with the reviewer, that the data we use cannot establish that the differences are due to friction, although the results are intriguing.

As a consequence, in this version we have emphasized the fundamental nature of the budget law, and its consequences. Then we develop in more detail the friction notion (using the particular stick-slip friction dynamics of earthquakes as a metaphor but not a model). This approach more reasonably reflects the accumulating state of knowledge in this area than in the first draft. Moreover, earthquake dynamics, like budgetary dynamics, are so complex that to date no convincing formal models have been established, although many have been proposed. That is about all we can do at this state of knowledge—indicate some directions, make some simple tests, and keep an open mind about what the final explanatory factors may look like. We think that is how scientific enterprise proceeds, and we do not think that the first version represented that stance.

We have added some language about the informal meta-analysis we have performed (and which must be informal, because the Paretian distribution has complications with its variance], and the 'convenience' nature of our samples, early in the manuscript. It is not true, however, that these datasets have all been published elsewhere in regard to their probability distributions. The power function has been demonstrated only in the American national case.

Reviewer 2 had a few comments that he/she characterized as mostly discretionary. But these were helpful, and we addressed all of them. We dropped the term 'chaos' that indeed was misleading (we did not mean it in the formal sencs, but of couse this invited confusion). We included a brief discussion of error correction and non-linear error correction models. We noted the findings of Soroka and Wlezien. We dropped the Texas school district data, a suggestion also made by another reviewer, because it is far afield of the more independent policy producing units of government we studied in the rest of the paper.

Sincerely

Bryan D. Jones