A General Empirical Law of Public Budgets: A Comparative Analysis
(Manuscript AJPS–33702R1: Reviewer 1)

This revised manuscript has several commendable aspects. First of all, the revised manuscript has a much tighter and focused presentation style relative to the initial submission version. Second, I feel that the author(s) are pursuing an interesting puzzle – and trying to go about it in an original way with the use of non–Gaussian distributions as a means to characterizing univariate budgetary outlay data, and trying to make cross–national comparisons. Third, the author(s) raw data is ambitious and careful in both its measurement and capacity to make comparable measurements across time and nation. Finally, the execution of the distributional tests and plots are competently done in terms of a pure statistical exercise.

Nonetheless, the revised manuscript suffers from the same conceptual problems that plagued the initial submission version discussed in my previous review. What is perhaps most alarming is the author(s) failure to seriously address these shortcomings in either their manuscript or in their response/revisions document. This is problematic since several statements are explicitly or inferred by the author(s) in the revised manuscript which cannot be substantiated by their data analysis and resulting inferences. In my professional judgment, this manuscript falls far short of the standards of conceptual and empirical rigor typically found in AJPS articles. I both fully accept and respect the fact that my evaluation of this manuscript is merely advisory to the Editor. Nonetheless, I cannot in good faith provide the current manuscript a positive recommendation in light of my previous concerns which also exist in the current version of this manuscript. I do not mean to sound harsh in my assessment of this manuscript, but the concerns that I possess are of a fundamental nature dealing with basic social science concepts pertaining to (1) causality, (2) spurious relationships/observational equivalence, and (3) cumulative knowledge building. Irrespective of the outcome of the editorial process, I wish the author(s) the very best in their research program.

Rather than reiterate my concerns from my initial review of this manuscript (which have not been addressed by the author(s) in the revisions), I will state my concerns as I read the "Responses to Reviewers Comments" document and the revised version of this manuscript as noted below. Please note, that much of my "marked–up" comments on the manuscript are redundant (e.g., many of the points I raised in the interpretation of the *Empirical Analysis* through *Conclusion* sections of the revised manuscript appeared earlier in my reading of the manuscript). Thus, I limit my type–written comments to "unique" points for purposes of parsimony.

Page 1: Responses Document:

The "earth science" logic is beyond "complex and elusive" as noted in the author(s) "Responses" document – it is merely relegated to serving as a **metaphor** that has neither an identifiable causal mechanism nor considers rival explanations which may be driving the "power function" law results (points noted in explicit detail in my previous review). This is highly problematic if one views social science as being concerned about establishing causation as well as placing a premium on avoidance of spurious associations.

Page 2: Responses Document:

The author(s) discuss Guttenberg–Richter law as an empirical law which takes the form of a power function. They go on to add "Policy studies is more like this than a top–down deductive process that is sometimes viewed as desirable, but in this case it is not currently possible." This defense of their logic is not persuasive given that *within*–nation institutional politics and political–economic conditions are omitted altogether in their univariate analyses of budgetary data. Political scientists have known for some time that short–run institutional politics can determine budgetary outcomes via pivotal politics, partisan fragmentation, and veto players (e.g., Alt and Lowry 1994, 2000; Krehbiel 1998; Kiewiet and McCubbins 1991; Primo 2007; Tsebelis and Chang 2004).[1] For instance, "institutional friction" can be caused by institutional gridlock, while punctuations are thought to correspond to a lack (or sizeable reduction) of gridlock coupled perhaps with major partisan/ideological change (e.g., a realigning election cycle). Once one properly and rigorously accounts for such existing explanations, it is quite possible that the estimates for the power function exponents will become attenuated. If they cannot become differentiated from the normal/Gaussian distirbution, then the author(s) are merely engaged in "selling old wine in new bottles". If, however, the Paretian distribution assumption effectively holds after a more rigorous scrutiny of the data, then the author(s) can make claims about information processing which are not observationally equivalent to economic or political conditions. Put simply, the author(s) univariate budgetary data would have to either control or "purge" such institutional and political–economic effects before they can make any claims about detecting a "General Empirical Law of Public Budgets" that embodies uniformity and non–Gaussian distributions. Until they account for such issues, I remain unpersuaded by their empirical findings and claims – and I surmise many other scholars will share my assessment. To persuade me that this study provides "value–added" to our knowledge of budgetary/fiscal politics and policy processes, the author(s) must establish an explicit causal story that shows what causes frictions and nonlinearities – and that these are distinct from existing social scientific explanations of found in the literature on budgetary/fiscal politics and policymaking.

Page 1: Revised Manuscript:

I fail to see the connection between the present study and the EITM approach which is about linking causal theories with explicit tests of these theories, that rigorously account for competing explanations in the process of hypothesis testing (e.g., Krehbiel's "Pivotal Politics" book where he tests his theory, while accounting for Cox and McCubbins "Party Cartel" theory as a rival explanation).

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[1] While the author(s) do explicitly consider macro–institutional factors to explain "institutional friction" (see **Pages 17–18, Figure 9**), these are time–invariant within country – i.e., they omit *within–unit* variation [accounted for by budgetary/fiscal politics and policymaking studies cited above] that corresponds to changing political and economic conditions that occur through time within a give nation.

Page 1: Revised Manuscript:

What the author(s) uncover in their study is an "empirical regularity" – albeit one that it not rigorously examined. An empirical generalization presupposes a causal logic/mechanism is identified – this is not the case here for reasons noted in both my previous review and above in the current review.

Page 2: Revised Manuscript:

The author(s) claim that they utilize the full distribution (as opposed to 1st or 2nd moments of the distribution via the mean and variance) and by doing so they provide an "elusive empirical generalization upon which sound theory can be built.". Regarding the first point, an aggregate probability distribution is comprised of its various individual moments (mean, variance, skewness, kurtosis). If the individual moments are unidentified or misspecified, then the aggregation process will result in a inaccurate characterization of the aggregate probability distribution. Second, for empirical generalizations to be of any use of building new theory, it must account for "old" theory since all we may be doing here is reinventing the wheel by ignoring past theoretical and empirical insights on budgetary politics.

Page 5: Revised Manuscript:

The author(s) use of e-mail communications following a Paretian distribution is intriguing. However, establishing this distributional form without an identifiable causal mechanism is problematic – especially given that several persuasive causal mechanisms have been established in the literature on budgetary/fiscal politics and policymaking (some of these important works are noted earlier in this review).

Page 13: Revised Manuscript:

The author(s) acknowledge comparative–static explanations of budgetary/fiscal politics and note its limitations with respect to adaptive/dynamic information processing. This is a valid criticism raised by the author(s). Unfortunately, the present study fails to provide leverage whether this approach is indeed not borne out by the data – i.e., falsify the claims from the comparative–static equilibrium camp by accounting for measures/variables that they use to test the veracity of their theories. The author(s) lack of willingness to address past research makes one cast strong doubt on their empirical validity of their argument. The empirical implications of comparative–static models can be tested alongside information processing models (e.g., Dan Carpenter's 1996 <u>APSR</u> article [noted in my previous review] provides one template for how this can be implemented via gamma probability signaling functions applied to budgetary control of administrative agencies). At present, the author(s) empirical execution does not get any purchase on the validity of their substantive argument and empirical approach since existing rival explanations are omitted from their empirical investigation.