"A General Empirical Law of Public Budgets: A Comparative Analysis"
(MS # AJPS–33702: Reviewer 1)

The authors attempt to descriptive characterize government expenditures/budget outlays for a variety of different governments and budgetary subunits. They provide a fresh departure from the existing literature rooted in preference–based models of institutional actors cited in this study and how they interact with institutional rules to determine budgetary outcomes (e.g., Krehbiel; Shepsle and Weingast; Tsebelis). I commend the authors for trying to really take a different approach to understanding public budgeting as "frictional" process – defined as the extent to which budgetary expenditure/outlay distributions are leptokurtic ("fat–tail" and/or "heavy–shoulder" statistical distributions). Unfortunately, this work suffers from a lack of theoretical microfoundations; omission of the short–run institutional dynamics associated with preference–based models of budgetary politics; and a failure to clearly demarcate among the various stochastic processes that the authors are interested in advancing to further our understanding of public budgets. My comments below are intended to assist the authors in providing a more satisfying approach that will make a meaningful contribution to our scholarly knowledge on this topic.

THEORETICAL

(1) After reading this paper twice, I am still uncertain what is the mechanism that is causing these distributions to be non–Gaussian. Although they frequently discuss institutional friction, but there is no clear mechanism linking this concept to policy outputs. The authors note "signals" and their transmission to the political environment, this discussion is theoretically vapid since there is no true causal understanding of the social processes generating these signals. It is true that the authors provide a descriptive–general explanation concerning friction in organizational systems (which I find vague to say the least), but what is really going on here? Is it based on the response to explicit shocks (unanticipated events)? Is it connected to institutional preferences within each governmental system (see **(3)** below for more details)? What we are left here is with a description, but not explanation, of these stochastic processes. That is, we are left with no clear identifiable theory of policy punctuations and how it causally relates to overcoming friction that results in a deviation from equilibrium behavior. The works of John Padgett (1980: *APSR*, 1981: *American Journal of Sociology*) and Dan Carpenter (1996: *APSR*) provided a tangible causal structure on their stochastic processes (rooted ***explicitly*** *in* information processing and adaptive adjustment), and hence, had considerable explanatory power. The present study sorely lacks in this regard. A clear and logically precise either verbal or formal theoretical model of how cognitive and organizational limitations drive such budgetary punctuations is necessary for this work to go beyond mere description of univariate data of a dubious nature.

(2) The authors discussion implies that due to the Central Limit Theorem (CLT), convergence to a Gaussian distribution happens to "kick–in" as the number of independent processes resulting from these signals from the policymaking environment. However, in asymptotic theory statistics, it is widely understood that convergence to a Gaussian distribution may be difficult unless the sample size is incredibly large (e.g., the normality and regularity conditions of many maximum likelihood estimators do not "kick–in" until the sample size becomes at least a few hundred observations – if not more). This convergence difficulty is exacerbated by deviations from the maintained distribution. The authors failure to **explicitly** link these independent processes to policy outputs further obfuscates any claims that cane be made regarding when one can expect to observe a Gaussian distribution for budgetary outputs.

(3) The authors do a fine job of analyzing cross–government (between) variation in formal institutional structures to account for *institutional friction*. Regrettably, the authors omit short–run dynamics of institutional politics (i.e., <u>within–variation</u> involving institutional friction), derived from preferences and institutional rules, as a feature that may explain the expenditure/ outlay distributions that they obtain. This omission has several serious implications. First, their substantive "so what?" conclusion to political scientists is that these distributions can be explain by formal institutional structures seems strikingly obvious at first blush. Second, yet I am not sure that I entirely buy this particular claim given that considerable variation occurs within each formal institutional structure – e.g., divided versus unified government in SOP and presidential systems; the width of the gridlock interval; the size and nature of governing coalition [e.g., number of minority parties in and/or percentage of seats held by the governing coalition]. One would expect that institutional friction would vary systematically with these aforementioned factors so clearly documented in the new institutionalism literature. For example, the capacity for growth punctuations vis–a–vis cutback punctuations – noted on the top of page 13 – will be related to government gridlock (in one form or another described above). Is it possible that cutback (growth) punctuations can be explained by a fiscally–austere right–of–center government? Is it possible that growth punctuations can be explained by gridlock? Beyond the authors narrative, the reader has no clear sense as to what is causing these distributions to have fat tails and/or heavy shoulders – let alone the substance underlying these descriptive distributional characteristics.

Put simply, the dynamics associated with such within–country or government variations inherent to institutional politics is completely absent from this study. If the authors are going to provide a convincing punctuated equilibrium account of budgetary outcomes, they must account for the **independent** effects attributable to over–time variations involving institutional preferences. Otherwise, the authors model is underspecified, and they are grossly overstating the effects of these formal institutional structures which can only account for between–variation among budgetary institutions. Moreover, it does not add to the cumulative scholarly knowledge of such processes since it is completely divorced from insights derived from scientific theories and evidence to date on the topic of budgetary politics.

EMPIRICAL

(4) The authors need to clearly demarcate between Gaussian distribution and the variant non–Gaussian distributions in mathematical form prior to the analysis of their data. This is especially critical to the point of the manuscript since the authors purport to advance a general law of budgets that can be accounted for by variations in a power function. For example, how does the power function differ from the Laplace (double–exponential) distribution in both intuitive and mathematical terms? How do each of these non–Gaussian distributions depart from the Gaussian distribution along the same dimensions? The authors should provide inferential tests for these data according to each of these stochastic (probability) distributions, where the null/maintained hypothesis is that a given distribution is valid. Rejection (or failure to reject) of the null/maintained hypothesis could give us inferential leverage into discriminating among these various distributions. One could also complement the use of distributional tests with other criteria to discriminate between these stochastic processes – e.g., How well do the various data fit a particular distribution according to various information criteria?

(5) The authors analyze budgetary expenditures/outlays which are **much more** subject to random shocks than compared to budgetary appropriations. This is because the former may vary in relation to varying demands for public goods/services (i.e., government spends less than appropriated when demand is less than anticipated; while the converse is true – usually requiring a mid–year supplemental appropriation); budgetary carry–over provisions from one fiscal year to another, but to name a few. These budgetary concepts are only equivalent when all appropriations are spent within a given fiscal year without additional expenditures. As a result, I am curious whether the authors' evidence of non–Gaussian budgetary distributions would be attenuated if they replicated their analysis with appropriations. Further, appropriations are the direct result of the institutional friction (i.e., formal institutional structures) that they are attempting to use to explain budgetary processes. Such structures may have little, if any, bearing on how much is spent relative to the amount appropriated if this gap is accounted for by variations involving budgetary carry–over provisions and/or unanticipated surges or decline in the demand for public goods/services.

(6) The statement on page 18 "Economies are less volatile today than in the past as economic management in the developing world improves, so that the volatility of the budget series has dampened over time." strikes me as not being necessarily true – especially for governments that permit deficit spending (e.g., most, if not all, national governments). Unless the authors can establish a clear empirical link in their data, I think that they should refrain from such a conjecture.

(7) The authors should use a *Spearman rank rho* correlation coefficient (and test) since their institutional friction variable is an ordinal measure. *Pearson's r* may overstate the true correlation between institutional friction and degree of leptokurtosis in the budgetary expenditure distribution (page 17).

(8) I do not know how to make sense of the major conclusions of this study. *Regarding 1)*: we have no real empirical leverage on understanding how institutional friction affects the distribution of budgets for reasons noted in **(1)** & **(3)**; *Regarding 2)*: the authors do not present an identifiable causal mechanism about the sources of these bursts – rather they are inferring such theoretical patterns from the observed data in a post–hoc manner; *Regarding 3)*: I am unsure of the substantive political meaning/importance associated with the claim that "Public budgets in modern democracies are invariably characterized by change distributions that follow power laws."; *Regarding 4)*: This insight regarding the asymmetric pro–spending bias from divided government is already known from existing preference–based models of budgetary politics (e.g., McCubbins book chapter in *Politics and Economics in the Eighties*, ed by Alesina and Carliner).

In closing, the authors possess some innovative ideas – but the motivation and execution are frankly a mess. It is my opinion that the mere description of data distributions is of limited utility since it lacks a true causal, explanatory story. In my opinion, a productive enterprise that yields <u>cumulative</u> knowledge on budgetary outputs from a political science perspective would involve advancing a novel, explicit causal mechanism. For instance, one route to pursue along these lines would be to model (in a regression framework) the between and within institutional friction as an information processing problem ala Carpenter (1996), whereby institutional friction would result in budgetary outputs that deviate from what "equilibrium, preference–based" theories (e.g., Shepsle & Weingast; Krehbiel; Tsebelis) grounded in complete information would predict. Another potentially promising route is to model these budgetary output distributions using alternative stochastic distributional assumptions involving the data generating process for the various budgetary output dependent variable(s) of interest – and re–examine both between and within sources of institutional variation in budgetary arrangements impact budgetary outcomes. This alternative approach would be in the vein of Gary King's classic maximum likelihood methods project from nearly two decades ago insofar that the authors could show us whether distributional/stochastic empirical modeling assumptions really make a difference for analyzing budgetary outputs. If the authors' skepticism of Gaussian distributions used to analyze budgetary outputs is empirically valid, then it should manifest itself in the relationships estimated/obtained from non–Gaussian regression–models – as well as yielding better fitting models whose parametric assumptions are satisfied. Failure to demonstrate such empirical characteristics would undermines the authors' arguments about the utility of power functions and non–Gaussian distributions more generally. The mere univariate characterization of the authors dependent variable/series of interest – budgetary expenditures/outlays – is of VERY limited theoretical utility or explanatory power to further the type of research that typically appears in leading political science journals such as *AJPS*.