AMERICAN POLITICAL SCIENCE REVIEW
DEPARTMENT OF POLITICAL SCIENCE
THE GEORGE WASHINGTON UNIVERSITY
WASHINGTON, DC 20052
APSR@GWU.EDU

February 9, 2007

Professor Bryan D. Jones
Department of Political Science
University of Washington
Box 353530
Seattle WA 98195

Dear Bryan:

RE: Manuscript #13928, "Are Political Systems Poised between the 'Order' of Friction and the 'Chaos' of Urgency? Public Budgeting in Comparative Perspective"

We have completed our review of "Are Political Systems Poised Between the 'Order' of Friction and the 'Chaos' of Urgency? Public Budgeting in Comparative Perspective," which arrived here on October 16, 2006.

This paper has evoked a variety of compliments, criticisms, comments, and overall evaluations from the reviewers, each of whom is very well qualified to review it. Two of the three reviewers think that it has very good long-term potential but see a substantial amount of work as necessary before it will begin to fulfill its potential. Having taken some time to work my way through the paper and these two reviews, I must say that their openness to a "revise and resubmit" invitation, given the breadth and depth of the required changes, exceeds my own. As I think you know (and as I think you favor, at least in the abstract, though not necessarily in this particular instance), I take a very conservative approach to issuing such invitations, which I reserve for papers about which the reviewers are genuinely enthusiastic and for which the required revisions would be, if I may be so bold as to use budgeting terminology, relatively incremental. As I read these two reviewers, the reviewers' enthusiasm centers more on what the paper might possibly become than on what it currently is, and, as noted above, I think the gap between the "currently is" and "might possibly become" is very sizeable. Reviewer 3, as you will also see, isn't at all favorably disposed toward the paper. S/He is put off by the ruling metaphors and jargon, and that leads to some sarcastic comments on his/her part. I think you should get past the sarcasm, once your irritation has cooled down, and face up to the substance of his/her dissatisfactions along these lines. More substantively, his/her eighth point goes much deeper than metaphor and jargon.

A couple of other passing comments prompted by my own reading of the paper: (1) The table or figure to text ratio here is very high. A picture may worth a thousand words, but in this instance I think clearer and in some places more elaborated verbal pictures, rationales, and explanations would have helped

more than yet another figure. (2) The paper reads as though it had been written by a committee. That is hardly surprising, given the number of co-authors, but knowing *why* it reads that way didn't make it more pleasurable to read.

All in all, I'm afraid that this set of reviews, buttressed by my own reading of the paper, haven't given me enough ammunition to hold our review process open by inviting a revision and resubmission. That being the case, I must now close the process by declining the opportunity to publish your paper. Thanks for letting us consider it. I'm confident that you will find these reviews helpful as you rework the paper for submission elsewhere. If you have any comments concerning this review process, I would be pleased to receive them.

Very truly yours,

Lee Sigelman
Editor

Referee Report
Are Political Systems Poised Between the "Order" of Friction and the "Chaos" of Urgency?
Public Budgeting in Comparative Perspective (13928)
Reviewer 1

Summary

Overall, I like this paper. I appreciate the novelty of the problem being considered. I respect the attempt to construct an explanation from the ground up. Finally, I admire the paper's scope, the amount of effort that went into gathering and analyzing the data. Impressive. Nevertheless, the paper does not, at present hang together well enough to merit publication in the APSR as currently written. It needs stronger links to micro level models, to history, and to the literature on bureaucracy. I would be VERY willing to consider a revised version along lines that I will suggest in my review.

Report

This paper looks at the distribution of budget changes over time in a comparative perspective. This begs three questions. First, what does that distribution look like? Second, what explains its shape? And, finally, why do we care? The third question, of course, is the most important, but we can only answer that after we've answered the first two. For example, if budget changes are normally distributed because GDP changes are normally distributed then we care because we now know that budgets tend to track GDP. We could test that further by seeing if the changes in the two are positively or negatively correlated: if the former, economic growth and government services are complements. If the latter, they're substitutes.

This paper finds, quite surprisingly I think to some, that budget changes follow a power law. This is true not just in the US, but in other countries as well. Now, this causes us to ponder where that power law came from. They propose a stark model that includes two forces: friction and error accumulation. I think that this is a reasonable starting assumption, but I think that the model needs more theoretical justification. There is no mention, for

example, of the literature on hazard rates of public bureaucracies. One cause of drops in the budget would be the dismantling of existing bureaucracies. Nor, is there any mention of work on cost overruns, such as Seth Lloyd's fascinating model of Boston's Big Dig, which generates a power law distribution of project sizes.

And, yet, I want to be really clear about this, if the budget consists of a huge number of bureaucracies and projects with however strangely shaped distributions, it's change distribution would not be immune from the Central Limit Theorem. We'd still expect to get a normal distribution. Even so, this point needs to be made and justified. For, if the size distribution of bureaucracies or projects is long tailed, then cost overruns could create a long tailed distribution - whenever a big project went awry, there could not be enough small budget changes to fix it. I'll come back to this.

The primary contributions of this paper are two fold - first to show the power law distribution exists and second to say that one model of friction and accumulation won't produce that, so therefore, the long tails must be created by a sense of urgency. I don't necessarily believe that argument. If you look at the time series for the United States you see that the big increases in expenditures arise during wars: the war of 1812, the Civil War, WWI, and WWII. The big decreases, also not a big surprise, come following those same wars. The conservation created by budget balancing (or more accurately limits on debt) makes this unavoidable: what goes up, must come down.

Enough rambling. Here's what needs to be done.

Test the Urgency Hypothesis: The friction and accumulation model that the authors propose seems perfectly suited to periods of peacetime and periods free of natural disasters. During what I'll call **periods of regularity** these frictions and error accumulations imply that we should not see the double Paretian distribution. It seems only natural then that the authors should try to isolate these periods - across their six countries --- and see if in fact, the theoretical distribution holds when there is no urgency. If so, they've proven their point.

This test would require cutting off the far right tail of the distribution and then asking whether or not you have a

Pareto distribution from the midpoint up to that tail. If so, then the friction model works, except for cases of urgency, which is what I think the authors claim.

Another test of the urgency hypothesis would be to look at the micro level, to see if large budget increases occurred in bills for single purposes or if it was just a general rise in expenditures across the board.

I'll be harsh for a second. The paper constructs a model. And then says the model doesn't fit the data. Then it says "okay, it must be urgency" but then it never fleshes out the concept of urgency in a model. My suggestion, that perhaps it's Paretian + Wars, would show that the model is correct - it just leaves out big random events - wars. Why then is war spending a power law, which leads to …

Analyze the Power Law's Far Right Tail: Some physicist, either Barabasi or Newman has a paper on the many causes of power laws. It would be of great benefit to at least point people toward that paper and even greater benefit to explain which if any might explain these data. Morever, Seth Lloyd's cost overrun model, Neil Fergusen's battle models, or Richardson's war data all of which produce power laws might well explain why the ends of the tail fit a power law. After all, if the size of wars satisfies a power law, then so might the cost of wars. If so, then we have a causal explanation.

Follow the Rat through the Snake: Once the war happens, the government is in debt. It has to pay off that debt. Let's suppose that I have a budget that follows a normal distribution. Now I introduce a big event, like a war, to pay for that war, I have to run deficits in future years. Write down a simple macro model in which I pay off that war loan like a mortgage in some fixed number of years. What does that do to the distribution of budget changes? Does it give it fat tails?

Pushing further: Point 2 on page 20, the authors claim that "orgies of spending are not fully offset by less exuberant cutting." Wouldn't the fact that they can pay off the loan over time mean that the cuts can be spread out? And thus explain why the left tail is not as long as the right one.
Unpack the Reductions: Reductions in the budget come from ending programs, departments, initiatives, etc… Therefore, it makes sense to ask whether the hazard rate models of

Carpenter and others would create a distribution like that for the left tail. Let me be more specific. I can tell a Herb Simon like story that gives me a skewed distribution of agency sizes in government, a power law in fact. If those die with a constant hazard rate, then I'm going to get a power law distribution in budget reductions. Of course that's not quite accurate because the analysis is on percentage change and not actual change, but it's close.

Minor Comments

Page 6, I'd stay away from mentioning punctuated equilibrium. Though I agree with the author's use, the whole Gould/Dawkins, punctuated/gradualism debate requires a much more nuanced discussion. Given it's not central, I see no reason to introduce confusion.

Page 7,11: Cite John Miller, etal's new book *Complex Adaptive Systems: An Introduction to Computational Models of Social Life* as a place to find a clear discussion of Bak's model's relevance to social science with adaptive actors.

Page 8: The claim that "friction is greater in politics than in markets" is too broad. Friction enters in lots of places. Lock-in effects in markets are often hard to over come. Markets have less friction with respect to incentive based actions but they may have more friction in coordinated actions. Governments can unilaterally change.

Page 8: Here you note that inputs are Gaussian. My reaction was, "whoa, strong assumption" Later on page 9, you motivate it convincingly. Tell us that's going to happen.

Page 10: I'd drop the Barabasi argument. It's true that he has urgencies (in the form of delay) and gets a power law, but it's a bad analogy. It's easy to respond to email quickly, but very hard to allocate billions of dollars.

Page 12: it's be nice to see some runs of the model under different scenarios. Not AVERAGE runs, but individual runs with commentary so that readers can get a feel for how the budget increases unfold.

Page 15: Data "are" unreliable. (Yes!!! I finally caught someone else making this mistake - I've done it myself 743 times.)

Page 16: "Budget changes have dampened over time" this is because governments have grown as a percentage of GDP. When the government is 4% of GDP it's easy to have government increase by 30%. When it's 40% of GDP, then it's a LOT harder.

Page 19: Making comparisons "among" governments should be "across" governments.

Review of Ms # 013928

Apart from a horrible title, this textually short paper has a fair number of things going for it: (a) new data from a wide range of national and local governmental contexts, (b) a parsimonious indeed minimalist theory capable of explaining almost universal budget distributions, (c) which nonetheless is not inconsistent with a wide range of much richer theories in political science, devoted toward explaining more fine-grained data. The theory of the paper – which basically is a threshold mixture of sluggish institutional "friction" and occasional bursts of political attention, which break the dam of institutional rigidity – is perhaps too minimalist for many tastes. But stochastic-process models, devoted to deriving distributional forms, are a genre typically characterized by minimalism, so that feature per se should not be held against the authors, in my opinion. The question is more whether the model robustly captures a wide and robust range of empirical cases. Certainly enough distinct data sets are presented to establish at least a plausible claim in this direction. So I do not recommend rejection.

Now for my criticisms, which I hope the authors can alleviate in future drafts:

1. Forget the cutsie title. The self-organized criticality work of Per Bak and his predecessors Chris Langton and Stuart Kauffman refers to a dynamic or evolutionary progress driving systems to "poisedness." It is true that power laws, like the Pareto, are produced in such models, but the authors here have theorized no such evolutionary or even sandpile dynamic pushing budget systems into an alleged state of poisedness, as they admit in footnote 5. Please come up with a more serious title that does not invoke strained analogies with S.O.C. (whose applications in biology, by the way, have been heavily criticized, although that is not the point here).

2. The verbal derivation of observed Double Paretos has two steps: (a) the translation of Gaussian inputs into Double Exponential budget outputs through normal incremental budget dynamics, and (b) the additional generation of "fat tailed" Double Paretos through the occasional/punctuated collective attention of political actors (presumably the President and the Congress although the authors are vague about concrete agency). I am supportive of the threshold mechanism that underlies 2a, recommending only the tightening up of the simulation that backs this up. I am more critical of 2b, believing that an entirely different approach is just as consistent with their data as is their alleged "collective attention" mechanism.

3. Re 2a: Contra the misleading statement on page 8, Gaussian distributions have exponential-type tails, as in apparent in the Normal-distribution formula itself. [Misleading because Normals have fatter tails than Exponential only because the "shoulders" of Exponentials are much thinner, thereby forcing offsetting mass into their tails. Functionally, however, both distributions have similar mathematical structure in their tails.] Hence any threshold mechanism which ignores Normally distributed inputs that are small, and translates inputs into outputs only beyond that threshold will generate

Exponential outputs. The higher the threshold, the more this statement becomes true. Therefore all that is really necessary to generate the core simulation finding is this minimalist threshold device. All the other wrinkles added to the simulation verbally (and only loosely) described in page 12-13 are superfluous to demonstrating the core point.

Further, adding cumulative errors does not push the model into fat-tailed Paretos because convolutions of Normals remain Normal, and convolutions of Exponentials are Gamma. No summation process is going to move you out of the exponential-type tail family. It certainly does not hurt to mention this negative 'finding' but I'm not sure a simulation was required to demonstrate this. All in all, I recommend paring the simulation down to its minimalist bare essentials to demonstrate the core "Normal inputs into Exponential outputs (because of thresholds)" point, and then simple alluding to how robust this result is to adding various wrinkles, banishing details into footnotes or references to other papers.

4. There are two major problems and one minor problem with the all important Double-Pareto 2b:

4a. There never is, actually, any model pro-offered to move from failed Double-Exponential predictions to observed Double-Pareto data. Rather there is simply the observation of Paretian tails, and the subsequent invocation of "collective attention" (or urgency) to post-hoc account for such tails. [The cumulative buildup of pressure, underlying institutional friction alone, was rightly shown not to produce Paretos, as I just agreed with.] .Deus ex machine invocations like this are not even verbal models. Much more precision is necessary about the structure of "collective attention" before something as specific as Pareto distributions can be derived from the not implausible claim.

There are many models in the contagion family that can generate power laws like the Pareto. Hence there is a rich field of mathematical frameworks to choose from. In other words, I believe a properly specified "collective attention" model is within the authors' reach. But they do have to reach.

4b. Even were such a contagion-style model to be developed, there is an entire different approach to the transformation of Exponentials into Paretos available in both the mathematical and the political science literatures, which appears to be inconsistent with any contagion approach. Namely heterogeneity in parameters: a mixed distribution of Exponentials with Gamma parameters is a Pareto distribution. In the political science literature on budgets, this has already been demonstrated in Padgett (1980) and Padgett (1981), who was the first person in the field to discover that budgets were distributed as Double Pareto. Parameter heterogeneity of many sorts is sufficient to transform Exponentials into fat tailed Paretos. "Collective attention" is only one of a wide array of candidates.

4c. There is only weak rooting of this paper into previous research. Padgett (1981) is not cited, but it classified parameter heterogeneity into three classes -- macroeconomic/macropolitical parameter variation across periods and years, institutional

parameter variation across organizational subunits, and technical parameter variation across substantive programs within agencies. Depending on the details of better specification, perhaps "collective attention" is consistent with the first such class of heterogeneity, but a closer engagement with (indeed recognition of) this previous research might yield more cumulative science.

Similarly, there is no reference to the old Davis, Dempster, Wildavsky work on incrementalism, even though verbally they specified a process not so unlike what the authors here allege – namely a disjunction between "normal" incremental budgeting dominated by institutional friction, and the occasional abnormal shift points wherein dramatic punctuated changes are contained. Being wedded to linear regression, Pareto distributions never entered the heads of DDW. But the point is that the authors in this paper can easily be read as returning to that old dichotomy between "normal incremental" and "abnormal punctuated" view that DDW held. At very least clarification of how the present authors differ from DDW incrementalism needs to be made, beyond just the invocation of Normal versus Pareto.

5. Finally I have an important data-analysis point. Their verbal threshold model is temporal in character (pressure builds up, etc.), but their distributional data is cross-sectional. This is not the end of the world, but the point is that their arguments seem to expect temporal, not cross-sectional, Pareto distributions. In other words, plot this distribution of percent change data for one program across multiple years. This should be either Double Exponential (2a) or Double Pareto (2b). How mixtures of such distributions should behave cross-sectionally is not entirely clear. Perhaps there should be consistency between temporal and cross-sectional distributions, but perhaps not, depending upon the particular distributions involved.

The operative implication of such an observation is simply to plot and examine a few temporal distributions. Judging from the appendix, only the French and the US data have extensive enough time-series to make this viable. But for those two cases at least my recommendation is possible. [I know the authors warned about accounting changes and the like. But just find some programs where this problem is less severe.]

Some of these challenges are major, but I believe the authors can meet them. I recommend giving them a chance to do so. The work is valuable enough that it should be supported.

MS 013928 "Are Political Systems Poised between "Order" and "Chaos"

1. this is a multi-authored text much in need of a good central editorial hand. Too much repetition.

2. too much casual use of the "friction" metaphor. There are different kinds of friction. The existence of friction by itself does not imply anything about system stability or equilibrium or lack of change. Stability is not the same as lack of change. P. 4: "this force [what force???] resembles the friction that occurs in the physical world. . . "

p. 5. "friction would cause slender peaks. . ." This is just wrong. The implication of the existence of friction is knowable only in relationship to the other forces at work in the system. And there is friction on ice vs. Velcro. Both are friction. A system might have varying degrees of friction over time in different circumstances.

2.1. the "friction" metaphor is mixed with "signaling" metaphors, e.g., p 12 ff. This strikes me as very odd and discordant, but perhaps I just don't know enough about the literature on signaling.

3. p 4. "sense of urgency." I hope that this will be given some more precise definition or form later in the paper, but I think it's just a way of saying "big change happens but I don't really know why."

4. p 3. In my recollections of the work, neither Shepsle & Weingast nor Krehbeil write about change being associated with "governing parties." However, of great interest to this paper, both of them do have some fairly specific notions about the mechanisms by which political changes are created. And neither of them imagines that changes is continuous. This paper has no mechanisms, just category labels.

5. Authors in love with their jargon: leptokurtic ("fat tailed"); Gaussian ("normally distributed")

6. p 6. "punctuated equilibrium theory." Theory? It may be appropriate to say that the discussions that have been so ubiquitous in studies of biological evolutionary change—which were provoked initially by Gould and Eldredge (none of this is cited here, by the way), deserve the label "theory"—since the debates have been intensely focused on competing concepts of the mechanisms of change (e.g., see the exchange in Science, 18 April 1997). Some research has discovered patterns of biological change that look just like punctuated equilibria in systems in which there is no external change at all (Science 21 June 1996). In this paper, however, "punctuated equilibrium theory" means nothing more than a data distribution with a particular shape—overwhelmingly small changes with "more than normal" large changes.

6.1. p 18. here's an experiment for you to perform with this manuscript. Go through and substitute "intermittent change" (or even "realignment") for occurrences of the phrase "punctuated equilibrium." E.g from p 18: "we conclude that national governments shift