

FW: Manuscript ID JPR-15-0143 - Journal of Peace Research - Decision

From: Prof Michael Ward Ph.D. <michael.d.ward@duke.edu>

To: Shahryar Minhas <hermes829@gmail.com>

Sheesh.

On 7/25/15, 3:03 PM, "jpr@prio.no" <jpr@prio.no> wrote:

>25-Jul-2015

>

>Dear Mike:

>

>We have now completed the review of your manuscript entitled "Relax,
>Tensors Are Here: Dependencies in International Processes" which you
>submitted to the Journal of Peace Research for the special issue on
>'Networked International Politics'. Three reviewers have evaluated your
>manuscript and their comments are included at the bottom of this letter.
>We hope that with the present decision, and hopefully your resubmission
>and a fast second round of review, we will be able to get the special
>issue out in print sooner rather than later next year.

>

>The reviewers have generally recommended publication, but also suggest
>some minor--and in case of one reviewer (R1) major--revisions to your
>manuscript. Therefore, I invite you to respond to the reviewers'
>comments and revise your manuscript.

>

>You will notice that there is general agreement on how the ms. could be
>strengthened, but that the reviewers differ markedly on how fundamental
>these changes are perceived to be. First of all, the reviewers are
>concerned about the statistical properties of the introduced statistical
>(tensor) model. Reviewer 1 asks for these statistical properties to be
>either stated explicitly or for a reference to a 'technical' article
>where these properties are derived. Reviewer 3 questions what
>assumptions about the error term (σ in 3) are made. I agree that
>these are crucial points to address in the revisions.

>

>The second concern that comes up repeatedly is the comparison with
>alternative approaches. Reviewer 1 points towards Multilayer models

>(Mucha) while reviewer 3 refers to MRQAP models. In my opinion, this also
>relates to the main concern of the second reviewer: what is the main
>problem that this paper addresses? Clearly, the main audience of JPR will
>be mainly interested to know when tensor models are most appropriate (and
>when it is better to rely on alternative models). It could even be
>helpful to compare with 'incorrect' models. How serious are the problems
>that the tensor models avoid.

>

>Finally, Reviewer 2 specifically asks for a concrete set of examples /
>questions; even referring to an earlier version of the paper that was
>made available on-line. Re-reading the introduction I noticed several
>examples, but admittedly they are somewhat hidden. Restructuring the
>introduction and/or using a specific (but preferably published) example
>could strengthen the intro.

>

>Your ms. stands at 7,691 words. Our upper limit is 10,000 words,
>including notes, references and all other elements. The revised article
>should not exceed 10,000 words.

>

>With regard to style and technical matters, please refer to our 'Notes
>for Authors', available at
><http://file.prio.org/journals/JPR/JPR-Notes-for-Authors.pdf>. In
>particular, note section 18, which explains our data replication policy.

>

>When submitting your revised manuscript, attach an anonymous revision
>memo explaining in detail how you have dealt with the various comments.
>Please use this memo to document any changes you make to the original
>manuscript. You are not, of course, obliged to follow every suggestion,
>but please identify where and why you disagree. In order to expedite the
>processing of the revised manuscript, please be as specific as possible
>in your response to the reviewers. I will probably send the manuscript
>back to all of the original reviewers together with the revision memo,
>and possibly to a new one without. So please make both the revised
>article and the revision memo anonymous.

>

>The Journal of Peace Research is obviously concerned about the
>publication date of the special issue, and it would be extremely helpful
>if you were able to submit your revised ms. as soon as possible. Ideally,
>you would submit your revised ms. by September 1st (even better if you
>could submit before that date).

>

>Once again, thank you for submitting your manuscript to the Journal of
>Peace Research and I look forward to receiving your revision.

>
>Sincerely,
>
>Han Dorussen
>Associate Editor and Special Issue Guest Editor, Journal of Peace Research
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>***
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>To revise your manuscript, log into
><https://mc.manuscriptcentral.com/jpres> and enter your Author Center,
>where you will find your manuscript title listed under "Manuscripts with
>Decisions." Under "Actions," click on "Create a Revision." Your
>manuscript number has been appended to denote a revision.
>
>You will be unable to make your revisions on the originally submitted
>version of the manuscript. Instead, revise your manuscript using a word
>processing program and save it on your computer.
>
>Once the revised manuscript is prepared, you can upload it and submit it
>through your Author Center.
>
>IMPORTANT: Your original files are available to you when you upload your
>revised manuscript. Please delete any redundant files before completing
>the submission.
>
>Because we are trying to facilitate timely publication of manuscripts
>submitted to the Journal of Peace Research, your revised manuscript
>should be uploaded as soon as possible. If it is not possible for you to
>submit your revision in a reasonable amount of time, we may have to
>consider your paper as a new submission.
>
>
>via Bertrand Lescher-Nuland
>Managing Editor, Journal of Peace Research
>jpr@prio.no
>
>*****
>Reviewers' Comments to Author:
>
>Reviewer: 1
>
>Comments to the Author
>Summary of Review

>By all rights, this should be a rejection but the technique introduced by
>the authors is interesting enough that I think it warrants a second
>chance. I have two problems with this manuscript. The first is that it
>seems to willfully ignore several large literatures that are closely
>related to the topic area. The second is that, though the basic framework
>of the statistical model is explained, none of its properties are derived
>and the statistics part of what is clearly a methods paper is curiously
>absent.

>

>Detailed Comments

>The first big problem with this manuscript is that there are vast swaths
>of literature that the authors ignore (almost entirely!) in order to
>claim to be the "first" to address this problem. They most certainly
>are not and need to beef up the front sections of the paper considerably
>in order to engage this literature and explain how their approach is
>different (which it is). Then, the backend of the paper needs to contrast
>their approach to the existing state of the art and show that their
>approach is better (which it very well may be, but one has no clue about
>this from reading the manuscript).

>

>The first body of literature omitted from consideration is the growing
>literature on multiplex/multilayer networks. See, for example

>

>- Pretty much anything Peter Mucha has done since 2011

>

>- Multilayer networks M. Kivela, A. Arenas, M. Barthélemy, J.P. Gleeson,
>Y. Moreno and M. Porter, Journal of Complex Networks, Vol. 2, No. 3:
>203-271 (2014)

>

>- Mathematical formulation of multi-layer networks M. De Domenico, A.
>Sole-Ribalta, E. Cozzo, M. Kivela, Y. Moreno, M. A. Porter, S. Gomez and
>A. Arenas, Physical Review X, 3, 041022 (2013)

>

>- Diffusion dynamics on multiplex networks S. Gomez, A. Diaz-Guilera, J.
>Gomez-Gardenes, C.J. Perez-Vicente, Y. Moreno and A. Arenas, Physical
>Review Letters, 110, 028701 (2013)

>

>

>The second technique / approach / body of literature not engaged by this
>study is the stochastic actor oriented model by Snijders and Co. This is
>a dynamic model designed for the same purposes as the author's tensor
>model. This technique is widely used in the social sciences. The authors
>mention this in one sentence and then proceed as if it does not exist.

>Tensors may be better, but that case is not even made let alone
>justified.

>

>The second major problem is that section 3 is highly dissatisfying. The
>authors explain the approach in very broad strokes but the technique
>remains mostly black-boxed. No properties of this technique are examined
>or proven. My intuition is that the technique probably works, but that
>hardly cuts muster in the statistical world. I would have thought maybe
>that there was a technical paper already published or in the works and
>that this is just an introduction/application paper, but no such
>technical paper is referred to. Thus, without some sort of evidence that
>the technique works, as a responsible referee, I can only assume it does
>not. This must be rectified prior to this manuscript's consideration as a
>resubmission.

>

>The application is kind of interesting, but, since it does not contrast
>the proposed technique to any of the other approaches to the same
>data/problem, one cannot really conclude anything from it. And one is
>left asking what it is doing there?

>

>The use of a new empirical dataset is really nice and refreshing though.

>

>As a methods paper, why is this going to a special issue of JPR and not
>to political analysis or something like that?

>

>

>Reviewer: 2

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>Comments to the Author

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>Summary:

>The article summarizes some of the issues with the analysis of
>time-series dyadic data in IR with a particular focus on the heroic
>independence assumptions often employed. They illustrate an approach
>based on Hoff (2014)'s multilinear regression framework and provide an
>analysis using the recently released ICEWS data.

>

>Assessment:

>This paper summarizes an exciting area that spans a number of fields
>(there are parallel developments in statistics, biostats, econometrics,
>cs- see e.g. Stewart "Latent Factor Regressions for the Social
>Sciences"). The authors do an excellent job of highlighting the
>applicability of these developments to political science and I think

>publishing this work in JPR would be an excellent way to increase the
>visibility of this approach to modeling.
>I have a few small suggestions which are mostly focused on ensuring that
>the paper is readable by as many political scientists as possible. They
>are organized below into major and minor comments although none of these
>points are dealbreakers.

>

>Major Point 1: Readability

>This is an important piece but necessarily a bit more technical than the
>median political scientists will be comfortable with. A couple of areas
>where things might be improved a bit. For equation 4, you may want to
>write out the equation for a single observation in addition to this
>version in terms of multilinear operators. Seeing the equation in this
>way might be helpful to people less comfortable with linear algebra. In
>general pg 6 is a bit dense for non-math people. Obviously some of this
>is inevitable but perhaps signaling the example a bit earlier and using
>that for illustration would help.

>The second readability piece concerns the opening. I read an earlier
>draft of this paper when it was first posted on arxiv
>(<http://arxiv.org/pdf/1504.08218.pdf>). For what its worth, I prefer the
>opening in that draft because it is cast in terms of a concrete set of
>examples/questions rather than the more abstract formulation in the
>submission here. Obviously its a stylistic preference but I think others
>will likely find it a more engaging opening.

>

>Major Point 2: Defining the Problem

>The problem is posed primarily in terms of bias in the intro. I think
>there is a tension here between bias and accurate estimates of the
>parameter uncertainty (via standard errors, posterior variance etc.).
>Discussing for example the Erikson et al (2014) critique is really more
>about standard errors than about biased parameter estimates. Obviously
>without some very strong independence assumptions between blocks of
>parameters the estimator will have both sets of problems (biased
>parameter estimates and bad SEs) but the problems could be a bit more
>clearly articulated.

>

>Minor Comments:

>- Great figures throughout!

>- " Though this design has remained standard for decades, this approach
>has been repeatedly argued to produce biased statistical results" This
>could probably use a list of citations. Goodness knows there are plenty.

>- Citation issue "Hoff (201)". (although I like the idea of Hoff writing
>about this in 201AD)

>- when introducing the data format on pg 5 you may want to explicitly say
 >that variables = "relational measures" as you switch between the two and
 >folks without a networks background may be confused.
 >- you probably need to cite Schrodts paper for Cameo. YOu also may want
 >to attribute the quad counts to him as well since I think Yonamine is
 >just summarizing prior work here?
 >- " the model performance noticeably declines" in what sense? Speed?
 >Predictive Accuracy? RMSE?
 >- in summarizing the results I'd give the wall clock time so people at
 >least have some sense of what it takes to fit these models.
 >- the performance analysis in Figure 8 seems a bit silly in the context
 >of Ward's previous work on the importance of out-of-sample forecasting.
 >I can definitely understand why its set up this way but it does seem
 >worth acknowledging the tension here. In some sense the things being
 >diagnosed here would perhaps be better set up with posterior predictive
 >checks.
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 >
 >Reviewer: 3
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 >Comments to the Author
 >Review of manuscript JPR-15-0143, ³Relax, Tensors Are Here: Dependencies
 >in International Processes² for Journal of Peace Research
 >
 >This paper presents a linear regression model that permits plausible and
 >interpretable linear models for longitudinal relational data, allowing
 >the representation of reciprocity and transitivity in an autoregressive
 >approach. It seems a good step forward, but not enough information is
 >given about the model specification. The major question that is still
 >open after reading the paper, is the assumption with respect to the error
 >terms in the regression; or, equivalently, the assumption on Sigma in
 >(3). Without any information about this, the model cannot be assessed,
 >and section 5.3 falls flat.
 >
 >Since this is a linear model, it is for numerical dyadic data, and the
 >main alternative approach seems to be MRQAP. A comparison with MRQAP
 >seems called for.
 >
 >Detailed remarks.
 >
 >The title is funny and not convincing.
 >
 >p. 3. In the stochastic actor-oriented model of Snijders (2001), the

>utility of actors can depend on their covariates. Therefore, it is not
>true that the model assumes that all actors have the same utility.
>

>p. 4. Please specify the type of heterogeneity between the nodes that is
>represented in the multilinear tensor model of Hoff. For example, do the
>nodes have different regression coefficients?
>

>p. 4 Writing that Hoff (2014) provides an approach that ³combines both of
>these approaches in terms of their strengths² suggests too much. The
>approach by Hoff (2014) is different from both the latent space and the
>actor-oriented models. It does represent reciprocity and transitivity and
>unobserved heterogeneity between the nodes, but by going into a different
>direction than these two earlier models. More importantly, it is a model
>for numerical dyadic data, whereas the actor-oriented model and the
>latent space model are for binary data. Therefore, a better comparison is
>MRQAP, which also is a method for a linear model.
>

>p. 5. The paper writes about ³a set of v relational covariates². In my
>understanding, a covariate is an exogenous variable. I think that here
>the author really means a set of v interdependent dependent variables.
>Please be quite specific in the terminology here.
>

>p. 6. Please explain Tucker product.
>

>p. 9. Is the quantile transformation applied to all variables
>simultaneously, or were quantiles computed per variable?
>

>Small points.
>

>p. 3. Jackson-Wolinsky should be spelled correctly, and given with a
>reference.
>

>p. 6. If possible, it would be nice to avoid the index l , for confusion
>with the number 1. (On my screen in this font I can't see the difference.)
>

>p. 11. ³identifiable role²: I guess the author meant to write
>³identifiable², but this invites confusion with model identifiability,
>which cannot have been intended.