

**JPR-15-0143 RETITLED “RELAX, TENSORS ARE HERE
COEVOLVING LONGITUDINAL NETWORKS”**

Dear Professor Dorussen,

We first would like to thank you for the opportunity to revise and resubmit our manuscript. We believe the manuscript has greatly benefitted from the Reviewers’ helpful and thoughtful comments. We have revised the manuscript, taking seriously each individual point raised by the Reviewers. The revision memo is organized by first responding to your comments and then addressing the reviewers’ points. Our comments and responses are shown in *BLUE* below each point.

We hope you agree that the manuscript has improved through this helpful process and we are looking forward to your response.

Sincerely,

The Authors.

1. EDITOR

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 (sigma in 3) are made. I agree that these are crucial points to address in the revisions.

We have more clearly added in references to the “technical” article that this paper applies to the study of coevolving longitudinal networks in IR. There is a technical paper forthcoming in the Annals of Applied Statistics that discusses the technical properties of the model we introduce here in much greater depth. This current JPR paper is meant to provide an introduction and application of this model for scholars working in international relations. However, we have clarified a number of the methodological points raised. Specifically, we have provided a version of the multilinear tensor regression (MLTR) model for a single observation, provided more detail on the Tucker product, discussed how the MLTR framework accounts for nodal heterogeneity, and further clarifies how this framework differs from extant

Date: August 24, 2015.

approaches.

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.

We have added in a longer discussion comparing the existing approaches used for modeling networks with the model we illustrate here. The main problem that this paper addresses is the question of how to employ a regression based approach to study the coevolution of longitudinal networks in international relations. As we state in the “Modeling Approach” section, ERGM based approaches such as SAOM and TERGM have been extremely useful in studying binary, longitudinal networks while still permitting an understanding of network level dependencies. The MRQAP approach is also a valuable tool but its primary limitation is that it is a model whose goal is not to study network level dependencies but control for them through permutations in the rows and columns of the data (Krackhardt, 1988; Dekker et al., 2007). The resurgence in studying multilayer/multiplex networks in the field is a crucial development and we have added in a discussion about it to the paper. Many of the tools that have been developed in this literature are at the core of the model that we illustrate, particularly, with regards to work that has been done on the use of the Tucker Product as a tool to conduct high dimensional singular value decomposition (e.g., Dunlavy et al., 2001; Kolda & Bader, 2009.). However, we are not aware of any method in this literature that provides a comparable regression based approach to what we illustrate here. Specifically, the regression based approach we provide allows for the analysis of coevolving networks in a longitudinal context using a tensor based representation of the vector autoregression framework.

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.

We have tried to restructure the introduction through introducing a set of concrete questions towards the beginning of the manuscript that the model we introduce here can help to study. We have chosen not to focus on a single, published article in the introduction because we want to highlight the fact that the issues we raise with the dyadic framework are not just limited to one or two articles but are very much emblematic of how research into relational data structures is done across the field.

2. REVIEWER: 1

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, non of its properties are derived and the statistics part of what is clearly a methods paper is curiously absent.

2.1. Major 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. 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.

We agree that there are important pieces of the literature that we had not spent enough time discussing. We have added a substantially longer discussion to the second section of our paper that discusses the existing approaches and contrasts them to what the model we are introducing here provides. Specifically, we have added in a lengthier description of ERGM based approaches such as SAOM and TERGM, MRQAP (this was per the remarks of another reviewer), and the growing literature on multilayer networks.

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.

There is a technical paper forthcoming in the Annals of Applied Statistics that discusses the technical properties of the model we introduce here in much greater depth. This current JPR paper is meant to provide an introduction and application of this model for scholars working in international relations.

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 extant approaches in the literature are not focused on running the type of model that we introduce here. There are a number of approaches available for studying longitudinal networks, the most popular of which include SAOM and TERGM, but to our knowledge none

of these approaches provides an easy way for users to model the formation of endogenous, longitudinal networks – the same is true for the multilayer/multiplex literature. At the same time, as we note in the “Performance analysis” section of our paper that more work needs to be done in terms of predictive performance here. There are a number of avenues that we will be pursuing in future research to address this issue.

3. REVIEWER: 2

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. 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.

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4.1. Major Comments. 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.

We have rewritten parts of the paper to more clearly set up the discussion that started in the original page 6 of this paper. We have also added in an explanation for how to write out the model for a single observation into the text of the paper following our presentation of the model using multilinear operators. The model for a single country-country-variable

observation can be written as:

$$(1) \quad y_{ijvt} = \sum_i \sum_j \sum_k b_{1ii'} b_{2jj'} b_{3kk'} x_{i'j'k't},$$

where i represents a sender country, j a target, k a particular relational variable, and t a time point.

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.

We have tried to restructure the introduction through introducing a set of concrete questions towards the beginning of the manuscript that the model we introduce here can help to study. We have chosen not to focus on a single, published article in the introduction because we want to highlight the fact that the issues we raise with the dyadic framework are not just limited to one or two articles but are very much emblematic of how research into relational data structures is done across the field.

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.

We agree that both sets of problems are likely when network dependencies are ignored, we have tried to clarify this point in the opening paragraph.

4.2. Detailed Comments. “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.

Per your suggestion, we have added in additional citations.

Citation issue “Hoff (201)”. (although I like the idea of Hoff writing about this in 201AD)

Fixed this citation error.

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.

Added in a parenthetical note indicating that the two are equivalent.

you probably need to cite Schrodtt's 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?

We added a citation to Schrodtt, Gerner and Yilmaz (2009) to account for their work on the CAMEO codings. Yonamine (pg. 8, 2011) cites Duval and Thompson (1980) for developing the original quad measures so we added a citation to their work as well.

“the model performance noticeably declines” in what sense? Speed? Predictive Accuracy? RMSE?

When including all four relational covariates, the model takes noticeably longer to run, and the accuracy across each of the relational dimensions also declines (higher RMSE and lower R^2).

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.

We added in a footnote at the beginning of the results section noting that in its current iteration the multilinear tensor regression (MLTR) model takes a lengthy time to run. In the application that we provide in this paper, running the Gibbs sampler for 8,000 iterations took approximately 28 hours. We are working on rewriting the MLTR model in C++ using the Rcpp framework to help speed up the sampler.

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.

We agree that the performance analysis here is rudimentary. However, the goal of this specific paper was to highlight the capabilities of this model in shedding greater light on the dependencies underlying dyadic interactions. In the iteration of the model we present here there is clearly meaningful room for improvement in terms of performance. The next iteration of this project will be much more focused on enhancing the predictive performance of this model.

5. REVIEWER: 3

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.

Since this is a linear model, it is for numerical dyadic data, and the main alternative approach seems to be Multiple Regression Quadratic Assignment Procedure (MRQAP). A comparison with MRQAP seems called for.

5.1. Major Comments. 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.

This is the standard assumption of all VAR models: Temporal dependence is described by the autoregression (Θ), and contemporaneous covariance is described by Σ . To this issue, we are not sure what other alternatives to the model the reviewer might be suggesting. We do agree, however, that the performance analysis here is rudimentary. However, the goal of this specific paper was to highlight the capabilities of this model in shedding greater light on the dependencies underlying dyadic interactions. In the iteration of the model we present here there is clearly meaningful room for improvement in terms of performance. The next iteration of this project will be much more focused on enhancing the predictive performance of this model.

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.

The MRQAP is a valuable model in dealing with dependencies in network data, and we have added a brief discussion of it to this paper. However, the goals of the MRQAP framework differs markedly from the multilinear tensor regression (MLTR) modeling framework that we introduce here. Most notably, the model that we are applying here is to be used for instances in which scholars want to explore the coevolution of endogenous, longitudinal networks while also providing an explicit representation of network dependencies, such as reciprocity and transitivity. The MRQAP framework has proven to be a very useful alternative to standard OLS models for studying dyadic relational data, in terms of its ability to correct for structural autocorrelation in the calculation of standard errors. However, as Dekker et al. (2007, pg. 564) note the MRQAP framework is not appropriate when “the focus is on modeling specific network-related dependence structure(s)”, which is our explicit goal here. Further even if we were to shift our focus and move away from providing a representation of the network-related dependence structures, we would need to develop a panel vector autoregression version of the MRQAP procedure. This would certainly be a valuable addition to the network statistics toolkit, but providing it is beyond the scope of this paper.

5.2. Detailed Comments. The title is funny and not convincing.

We have removed any humor from the title. We have retitled it to, “A New Approach to Analyzing Coevolving Longitudinal Networks in International Relations”.

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.

We agree that for the case in which nodal covariates are added to the SAOM approach then nodal heterogeneity is allowed. We have clarified this within the paper during our discussion of the SAOM approach.

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?

The nodes are heterogeneous in how their actions depend on the previous actions of a given country, this is captured by the \mathbf{B}_1 and \mathbf{B}_2 terms. For example, for each county i , the coefficient b_{1iUSA} describes how predictive the actions of the USA are of the future actions of country i . Heterogeneity of these coefficients across countries i describes heterogeneity of the nodes in terms of their dependence on the actions of USA.

p. 4 Writing that Hoff (2014) provides an approach that combines both of these approaches in terms of their strengths 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.

The General Bilinear Mixed Effects (GBME) approach developed by Hoff (2005) is different than the approach we discuss here, and we have modified that section of the paper to more clearly delineate the differences. However, it is important to note that the GBME approach, unlike ERGM based approaches such as SAOM, is suited for handling a variety of relational data types, specifically: gaussian, poisson, or binomial (http://www.stat.washington.edu/people/pdhoff/Code/hoff_2005_jasa/).

p. 5. The paper writes about a set of v relational covariates. 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.

We have removed mentions of “covariates” in order to ease interpretation.

p. 6. Please explain Tucker product.

The Tucker product is a multilinear operator that is used for higher-order singular value decomposition (SVD), the same way that matrix multiplication is used for matrix SVD (De Lathauwer et al., 2000 ; Kolda & Bader, 2009). The Tucker product can be “defined” by writing out the equation for the model with respect to the scalar entries: $y_{ijkl} = \sum_i' \sum_j' \sum_k' b_{1ii'} b_{2jj'} b_{3kk'} x_{i'j'k'l}$. To illustrate how the Tucker product is calculated, say that we want to get the following expression: $\mathbf{Y} = \mathbf{X} \times \{\mathbf{B}_1, \mathbf{B}_2, \mathbf{B}_3\}$, where “ \times ” denotes the Tucker product, \mathbf{Y} has dimensions $a_1 \times a_2 \times a_3$ and \mathbf{X} has dimensions $b_1 \times b_2 \times b_3$. The first step involves reshaping \mathbf{X} so that it is a matrix with dimensions $b_1 \times (b_2 \times b_3)$, then we multiply

on the left by \mathbf{B}_1 , next we reshape the result to an $a_1 \times b_2 \times b_3$ matrix. This procedure would be applied iteratively among the remaining dimensions.

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

The quantile transformations are computed per variable.

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

We have corrected the spelling error and added 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.)

We have changed the index to avoid confusion.

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

We understand the possibility for confusion so we replaced “identifiable” with “meaningful”.