Foundations of Trust and Distrust in Networks: Extended Structural Balance Theory Response to Reviewers

First of all, we would like to thank all the reviewers for the valuable comments and their careful reading of the paper. We have modified the paper to address all of them. Below are the comments to each reviewer. In case a comment was made multiple times, we address it once and point the similar comments to our previous answer.

We have made many minor changes to the paper. The major changes are highlighted in blue text.

1 Reviewer 1

Comment. The treatment of trust (and distrust) as a symmetric relation, i.e. with an undirected graph, seems an oversimplification. The authors appear to be aware of this, as they point out themselves that trust and distrust are not symmetric constructs. Still in Section 5/6/7 all graphs are treated as undirected, with the conversion process from directed to undirected graphs presented in Table VIII. The experimental results show that the proposed approach performs well for sign prediction in these derived undirected graphs, but it leaves me wondering what effect the conversion to these undirected graphs has on the applicability of the trust prediction method. Trust prediction methods are typically used as an intermediate step in recommendation systems. Does the accuracy of recommendations not deteriorate when limiting to the use of undirected graphs?

Answer. This is a valid criticism of distance based methods. While they allow for a global representation of all relations and network level optimization, they also suppress information regarding the directionality of individual trust relations. It is possible to consider different schemes for representing the trust relations and translate them into tolerance expressions. We have discussed this in the related work and conclusions sections as future work.

Comment. The authors compare their trust prediction results with those of DuBois and Golbeck et al. from 2011 as a state of the art approach. In the mean time, Golbeck has published new work on trust prediction, using Getoor et al.'s PSL framework, in a paper that also strongly references structural balance theory: A Flexible Framework for Probabilistic Models of Social Trust Bert Huang, Angelika Kimmig, Lise Getoor, Jennifer Golbeck, International

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Answer. We have cited this paper in related work. This paper presents a logical inference method for structural balance theory, but does not allow for a representation of the amount of stress in relations.

Comment. The authors assume a linear ordering of trust degrees. Victor et al. have pointed out in their work that this does not allow a clear distinction between "absence of trust because of presence of distrust" versus "absence of trust because of lack of knowledge". They use trust scores that allow incomparibility. Practical Aggregation Operators for Gradual Trust and Distrust P. Victor, C. Cornelis, M. De Cock, E. Herrera-Viedma Fuzzy Sets and Systems 184(1), p.126-147, 2011

Answer. Thanks for pointing out this work. We have cited this paper and discussed its relevance to our work in the related work section. It provides an interesting future direction for our work.

Comment. This paper is an extended version of a conference paper. I suggest adding a reference to the conference paper in the manuscript.

Answer. Done.

Comment. p.8: "may sometimes cause stress (in +, +, +)" -¿ not clear what is meant here because (+, +, +) seems stress free?

Answer. Corrected, (+,+,+) is stress free. Incorrect example was given.

Comment. Definition 3.1: it is slightly confusing to use the same letters A, B, C for free as well as bound variables; it would be better to have a notational distinction between the triad A, B, C (these are the free variables) and all pairs (A,B) in it (these are the bound variables); e.g. "A triad A, B, C is balanced if for all pairs (X,Y), ..."

Answer. Rephrased as suggested.

Comment. sometimes the 'O' for neutral is italic and sometimes not (in the text and in the tables)

Answer. The notation is made uniform.

Comment. p.11: "Let n * n matrix X ... denoting node i's location in m-dimensional space" $->$ why m-dimensional? I was expecting n here. An explicit mention of what n and m are would be helpful here.

Answer. These have been clarified.

Comment. the numbers in the circles in Fig. 3/4/5 are barely visible. It also took me a while that e.g. d06 is the distance between node 0 and node 6; it would be helpful if 0 and 6 were in subscript here.

Answer. This is now fixed.

Comment. why is G* not mentioned in Table IX?

Answer. G* is added and numbers are clarified.

Comment. p.22: the text mentions 3.75 (twice) but the number in Table XII is 3.76

Answer. This has been corrected.

Comment. Small language errors (non exhaustive list):
p.5: none the existing − > none of the existing
p.5: instead close friendships − > instead of close friendships
p.5: below certain threshold − > below a certain threshold
p.5: computational costly − > computationally costly
p.6: citeSingh08 − > something is wrong with the citation here?
p.7: in formula right before section 3.1, the 2 in e2 should be a subscript
p.7: sentence under principle 1: A, B, C should be italic
def. 3.1: if for all triads − > if all triads
p.8: the Davis's balance theory − > Davis' balance theory
p.12: each unknown edges − > all unknown edges
p.12: Let the E − > Let E
p.13: in the Section 7 − > in Section 7
p.15: weaker than graphs 1 and 2 both − > weaker than in both graphs 1 and 2?
p.17: One of the recommended parameter configuration − > configurations
p.17: that as SM/SG does not − > that SM/SG does not
p.17: We expect that structural − > We expect structural
p.18: for any of the version − > grammatically incorrect?
section 7: 'Slashdot' is misspelled as 'Slahsdot' is many captions of figures
p.21: When people judging − > judge
p.23: as pair of nodes − > as pairs of nodes
p.24: to have to many − > to have many
p.24: Epinion − > Epinions
p.28: bodirectional − > bidirectional
p.28: will becomes − > will become

Answer. All these have been corrected, thanks a lot for reporting these.

2 Reviewer 2

Comment. The first one is to address the issue of readability. The paper is densely written and its formulae are not necessarily easy to follow for many TWEB readers. Certain statements could be elaborated on to make things clearer for those who do not work directly on this area; e.g. the statement in the first paragraph of page 11 that "tolerance given in Table III are equivalent to Definition 3.2" could be elaborated on a little bit further.

Answer. We have added clarifications in this part and in other parts of the paper to help readability.

Comment. The second improvement would be to provide a brief summary of the methodology that was followed to test/explore the claims that are made for the proposed model. That would help the reader understand how each of sections 5, 6 and 7 relate to the overall methodology. A defence of the methodology either rigorously or in terms of it being established in research practice would strengthen the paper significantly. In addition, there are large discussion sections (e.g. in the first two paragraphs of section 7.3) that maybe should be moved or collected in a discussion section - as they stand they can be disruptive to the flow of the paper which is already dense and its methodology, in the current form of the paper, not entirely clear.

Answer. We have added a significant introduction to the experimental results to both improve readability and to explain the methodology.

Comment. page 5, paragraph 2, line 8: "instead close relationships" $->$ "instead of close relationships"
page 5, paragraph 4, line 1: explanation of acronym PP is not given before the table on page 13.
equation on page 7: "e2" $->$ "e_2"
page 7, paragraph 4, line 5: the definition of tolerance could be clearer; as it stands, it is not clear how/whether it relates to relationship changes to address stress.
page 7, principle 1: explanation of notation (A, B)' and its difference to (A, B) is missing.

Answer. These have been fixed and clarified.

Comment. page 9, paragraph 4: The discussion in this section could be more clear on whether increased trust necessarily relates to privileged access to information. This point needs to be discussed more thoroughly, especially given that the proposed model uses metric distances; However, this discussion could be in a separate section as; in that paragraph it does not make the distinction between w+ ties and other ties any clearer anyway.

Answer. We have added clarification here about our use of the term privilege and its limitations.

Comment. Table IV; triad (s+ w+ s-) is repeated (3rd and 4th place on the first cell), while triad (s+ w+ w-) seems to be missing from that table?

Answer. Corrected.

Comment. page 12, paragraph 4: the claim that De Leeuw's stress majorization technique is considered to be one of the best needs to be supported by citation(s).

Answer. Rephrased and provided a citation.

Comment. page 16, paragraph 3: "how good the convergence" $->$ "how well the convergence"
page 18, last paragraph, line 2: "any of the version" is unclear/vague
page 19, first paragraph of section 7, line 2: "with force directed algorithm" $->$ "with the force directed algorithm"
page 19, first paragraph of section 7, line 4: citation to support the claim for best performance in the literature?
page 20, paragraph 5, line 3: "we considering" $->$ "we are considering"
page 21, paragraph 2, last couple of lines: "to check to which" $->$ "to check which"
page 22, paragraph 1, line 1: the use of numbers in parenthesis for rating values, such as "(4.60/4.76)", need to be explained.

Answer. Fixed and rephrased as appropriate.

3 Reviewer 3

Comment. In principle 1 what is $(A, B)'$? It is not defined, making it hard to understand principle 1.

Answer. Rephrased to make this clear.

Comment. The same problem follows to Principle 2.

Answer. Rephrased similarly.

Comment. After Theorem 3, the figure (b_{w+}) and statement above b_+ are using different notations.

Answer. Fixed.

Comment. Please reword Def 4.1, very hard to understand.

Answer. Done.

Comment. In equation (2) isnt it possible that w_{ij} will change with time? for example as an edge changes from strong to weak, for a particular problem? is it better to use w_{ij}(t) here? if this is possible how will it affect solution and the proposed method, though the authors state that setting w_{ij} is not in their scope?

Answer. We have added discussion regarding this. It is true that the weights can change as relations change during the convergence. Our formulation does not allow for both to be changing during convergence.

Comment. Section 5.2 is very unclear, what is that it is trying to say? Is it saying that the transitivity property holds in these three examples? Please rewrite the section, and state the objectives, arguments and conclusions clearly. Also figures edges are not not clear. In the current form it is hard to evaluate this subsection.

Answer. We have added clarification.

Comment. Line 48 page 16, 1/2w, please write it properly, the current form looks like w is in denominator.

Answer. Fixed.

Comment. Page 17 line 6, 10 I think its not d now b, again d_o looks like in denominator.

Answer. Fixed.

Comment. The scalability of the method is an issue, as the authors acknowledges and experiments are performed after down sampling. The method may not be applicable for real networks. Especially, as the contribution of the paper is mostly in application, than in theory. Though the paper has some theory, the method still looks like one of the ways to consider degrees of edge strengths in edge prediction.

Answer. Only the first set of results are run on the down sampled graphs. All the other tests use full graphs and run fairly quickly (1 min max), but do not include unknown neutral edges. If we included these edges, the underlying graph would be a complete graph. However, we show that including these neutral edges do not improve performance in general and hence our algorithm can be run practically on large graphs. As long as the underlying graph is sparse, we are able to run our algorithm in fairly large graphs. We have even improved our implementation further and checked in a newer version of our code in github that cut the running time for the full Epinions graph (the largest graph) in about half. We have clarified this point in our paper.

Comment. The problems is important, and extending SBT may very well be the method to solve it. On the other hand, other trust propagation methods based on Markov random walks etc. are already considering the degrees of trust (some of th references are give in my comments above). Though these methods are not using SBT, why cant we use one of these methods to predict trust in the context of degrees of trust? Do you see any strong reasons why not? In this context just using FD as baseline looks unfair. Do we really need to to extend SBT to infer trust in networks with degrees of trust?

Answer. We discussed this in related work. Trust propagation methods based on Markov random walks provide a natural representation for trust. However, it is not very easy to represent distrust in such formalisms unless an independent model is used. In fact, the work of Golbeck includes two algorithms: one is based on path probability (PP) similar to Markov random walks and the other one based on graph drawing. In our tests, we found that PP does not result in any performance improvement over FD. In essence, presence of distrust makes the problem quite different. In the absence of distrust information, a trust propagation algorithm is likely to be more effective than our method which relies on distrust relations to balance networks. We have made this point clear in the paper as well.

Comment. Please give AUC (area under curve) as quantitative measure for improvement, in ROC curves. Improvements are hard to measure from graphs, as well as it makes it hard for future works to compare against your results.

Answer. This information is included in all figure captions.

Comment. Page 28 line 11 typo

Answer. Fixed.

Comment. Legends in Fig 9,10 etc are over the graphs, besides not properly printed on paper for me, please check if this is a consistent problem for printing.

Answer. Checked, but was unable to reproduce the problem.

Comment. Some of the references from trust analysis in areas, other than Social Networking may be cited. Many trust analysis work in open collections build networks based on implicit links and analyze trust based on these networks. The work may be possibly related, and may have possible impact as well. You may find a survey in "Trust analysis with clustering M Gupta et al." or TWEB 2013 paper SourceRank... R Balakrishnan et al. etc.

Answer. Cited these paper.

Comment. Instead of focusing narrowly on the prior SBT research, the authors may think about applicability trust assessment problems in web, data web, linked data and so. The question, if SBT need to be extended, or one of the other trust analysis method would work better for weighted graphs from the paper.

Answer. Extended the related work to address this comment.