

View Reviews

Paper ID

4950

Paper Title

Mixed-Membership Stochastic Block Models for Weighted Networks

Reviewer #1

Questions

1. Please provide an "overall score" for this submission.

6: Marginally above the acceptance threshold. I tend to vote for accepting this submission, but rejecting it would not be that bad.

2. Please provide a "confidence score" for your assessment of this submission.

4: You are confident in your assessment, but not absolutely certain. It is unlikely, but not impossible, that you did not understand some parts of the submission or that you are unfamiliar with some pieces of related work.

3. Please provide detailed comments that explain your "overall score" and "confidence score" for this submission. You should summarize the main ideas of the submission and relate these ideas to previous work at NIPS and in other archival conferences and journals. You should then summarize the strengths and weaknesses of the submission, focusing on each of the following four criteria: quality, clarity, originality, and significance.

In this work, the authors propose a mixed-membership stochastic block model for weighted networks. Their contribution is focused on the use of a hierarchical extension that allows the hyperparameters of the poisson distribution to be drawn from a different gamma distribution for each pair of classes. They also describe an inference algorithm that combines collapsed variational and stochastic inference.

All in all, the proposed model along with the inference described is interesting. However, the model itself is incremental in concept and the proposed inference, while motivated by the complexity of the model and the size of the networks applied on in the experimental section, is not strongly supported. Details later. I vote for acceptance but wouldn't argue if it's rejected.

The work is clearly presented and to my understanding technically sound. The supplementary material covers sufficiently the updates related to the proposed inference.

My biggest concern and confusion is related to the experimental section and more specifically, to the analysis of the model performance. The performance of the mode is not always superior to the models it is compared against and this is fine. I am aware that there might be many reasons that can challenge such a statement e.g. choice of model, dataset, inference algorithm and combination of the above. However, the reasons the authors present to explain the underperformance away is confusing (see section 5.2 in the submission).

Line 231: "As one can note, MMSB models outperform the other models...is limited"

The plots do suggest this. However, I tend to believe that the nature of the datasets might be the actual reason why mixed membership is better fit. More details on the datasets would provide a better intuition. I am aware though of the space constraints.

In general, after looking at the performance of all the models in all datasets, the MMSB models do not always perform better and the SBM can always be a more reliable choice. Without better look at the nature of the datasets and a thorough analysis of the influence of the inference algorithm the plots are hard to interpret and

4. How confident are you that this submission could be reproduced by others, assuming equal access to data and resources?

3: Very confident

Reviewer #2

Questions

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4: An okay submission, but not good enough; a reject. I vote for rejecting this submission, although I would not be upset if it were accepted.

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Review Summary

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Overall, while an extension of the MMSB to weighted edges is potentially interesting, I think the paper is not quite ready for NIPS. There are numerous clarity problems, the technical contributions don't seem especially strong (mostly piecing together smart innovations from other papers), the treatment of the MMSB baseline is questionable, and the benefit of actually modeling weighted edges vs. unweighted edges is not assessed either qualitatively or quantitatively (experiments only look at binary link prediction, which many models can already do). I think a careful revision and resubmission elsewhere could make a strong research product, but the present paper is just not ready.

Paper Summary

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This paper extends the existing mixed-membership stochastic block model (MMSB), originally proposed by Airoldi et al., to handle observed networks where each observed edge might have an associated weight. While weighted network models exist for the stochastic block model ([4], [5]), which assigned all edges related to a node to the same cluster, no such ability exists for the *mixed membership* version of this model, which is the key contribution of this paper.

In fact, two versions of the model are presented:

- * W-MMSB, where block-block connection affinity parameters ϕ are drawn from a shared Gamma prior
- * W-MMSB-bg, where the ϕ parameters have a hierarchical prior, with block-block specific parameters

Another claimed contribution is the development of an inference algorithm for these models which combines stochastic variational inference (VI) and collapsed VI (by marginalizing out π and ϕ during inference).

Strengths

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- * Experiments consider several interesting datasets with large size (>10k nodes, >100k edges).
- * The extension to the Beta-Gamma-Poisson likelihood has potential.

Weaknesses

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- * The treatment of the MMSB baseline seems questionable. In Figure 1, MMSB is either the best of all at link prediction (hep-th, astro-ph) or near chance (digg-reply). While the near-chance is dismissed as "high variance of stochastic optimization", it's not clear why larger batch sizes or other easy fixes weren't tried. The MMSB is no more vulnerable to this issues than the proposed W-MMSB, so efforts should be made for a more fair comparison.
- * The variational optimization problem is poorly explained. In some lines, the reader is told that an approximate posterior distribution $q(\phi)$ exists. However, it turns out that these quantities are collapsed (I think) and then just point estimated afterwards.

Originality

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The stochastic block model and its mixed-membership version (MMSB) (Airoldi et al.) have been around while and inspired lots of follow-on work. Weighted extensions of the stochastic block model have been previously proposed (Aicher, Jacobs, and Clauset 2014, more below). While it does appear that a weighted extension of the MMSB hasn't exactly been tried before, I don't think that this extension and its variational inference alg. alone meet the novelty threshold for a NIPS accepted paper, especially since both the model and algorithm used follow templates provided in previous papers (e.g. the use of a Poisson to generate weighted edges is clearly considered by Aicher et al). There is little technical novelty here, when so many opportunities exist (extend likelihood beyond Poisson to better capture real data, improve the K^2 cost of representation, etc.)

Additional (uncited) examples of a weighted-edge SBM using Poisson likelihood:

- * Mariadassou et al AOAS 2010, <https://arxiv.org/pdf/1011.1813.pdf>
- * Karrer & Newman Phys Rev. B 2011, <https://arxiv.org/pdf/1008.3926.pdf>

Significance

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Learning unsupervised structure from relational/network data is a domain of broad interest to the NIPS community and many models have been proposed. Certainly many real-world networks have weights associated with each edge, and so a model that can capture integer weights like the proposed W-MMSB could be useful.

However, I don't think the present paper demonstrates *why* this weighted extension matters, beyond a hand-waving intuitive argument that if the data has weights, a good model should explain them. While sensible, the experiments do not make a strong case to me that modeling *weights* leads to good link prediction or somehow better/more intuitive cluster structure. The only things that were assessed are binary link prediction, and it's unclear why weighted edge models should do any better than models trained to directly model the conditional probability of edge/nonedge.

Also missing is a qualitative study of how clustering structure might change when using weighted edges, and why using **weighted** edges to drive clustering matters. Without this, it's hard to justify a new model when a simpler alternative (use MMSB to fit clusters, and then do post-hoc learning of each block-block Poisson parameters) exists.

Quality

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Modeling assumptions

Is it realistic to expect that **whether** or not an edge exists between nodes, AND the count observed at that edge if it exists, are modeled by the same Poisson distribution? This seems tough to justify, and I'd like to see more justification of this assumption

Does modeling weighted edges really help with binary link prediction?

There seems to be an unstated assumption that modeling weighted edges explicitly should help with binary link prediction (otherwise why do the experiment?), but several empirical results suggest otherwise:

- * For plain block models, the SBM outperforms WSBM on 5 datasets
- * For mixed membership models, the MMSB outperforms W-MMSB on 2 datasets

It's not exactly clear if the weighted models should perform better at this task, since they are forced to predict "weightless" edges. I think the purpose of this experiment may need to be reconsidered. The authors might instead consider an edge-weight prediction experiment, where any non-weighted baseline has a post-hoc Poisson prediction model fitted to its existing cluster structure.

Clarity

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Several issues exist throughout the manuscript in describing technical details at high and low-levels.

C1) Carefully separate the description of variational optimization problem from the predictive link distribution

The predictive link distribution marginalizes out over the block-block connection parameters ϕ . However, the variational distribution maintains an approximate posterior $Q(\Phi)$ (see line 95). This should be carefully noted to avoid confusing the reader.

Also, please avoid calling N^* "global parameters"... they are not separate free parameters in the variational optimization problem. Instead, you can just call them sufficient statistics or global statistics. The term "global parameters" is often used to refer to the parameters of $Q(\Phi)$, or similar variational factors unattached to specific edges/nodes.

C2) Unclear how "collapsed" the approach actually is

C3) Poor description of the MMSB parameter ϕ

Lines 42-45: Is ϕ really best called "distribution"? Shouldn't it just be a parameter that specifies the likelihood of interaction between two blocks? Certainly, we can say that ϕ has a conjugate prior distribution, but ϕ itself is not a distribution.

C4) Possible mistake in link prediction formula

The given link prediction formula in lines 197-198 needs to use " $\exp(-\phi)$ "

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Reviewer #3

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Summary: This paper presents a Bayesian model for weighted graphs (where each edge has an interger valued weight). The proposed model is based on the well-known mixed-membership stochastic blockmodel (MMSB). The main difference is that the Bernoulli likelihood of MMSB is replaced by a Poisson and the beta prior on MMSB link probabilities with a gamma prior on the rate parameter of the Poisson. The resulting model is referred to as "weighed" MMSB (WMMSB). Another variant (WMMSB-bg) is also proposed where the gamma prior is replaced by a hierarchical beta-gamma augmented gamma prior. A collapsed stochastic variational inference (SVI) is proposed for both the models. The proposed models are compared against MMSB and weighted non-overlapping stochastic blockmodel (WSBM).

Strengths:

- A nice extension of MMSB to model weighted graph. In my knowledge, MMSB hasn't been extended for weighted graphs (although the extension is straightforward).

5 of 6 - A scalable, collapsed online variational inference algorithm is proposed for the model. The empirical results look good (though the baselines aren't necessarily among the strongest ones out there).

- The proposed model is a minor variation of MMSB (replacing the Bernoulli likelihood by a Poisson and changing the priors on the model parameters). The SVI algorithm is developed for the model is interesting (although it uses ideas from prior work, such as subsampling the observations). Therefore, the overall novelty is a bit limited.
- More importantly, I would like to point out that there are gamma-Poisson Bayesian models for graphs that can readily model weighted networks and can learn overlapping communities. For example, refer to the Infinite Edge Partition Model (EPM) by Zhou (2015). Such overlapping stochastic blockmodels are actually often considered more expressive than models like MMSB (as shown in the experiments in Zhou (2015)). The EPM actually is designed for unweighted graphs (with Bernoulli-Poisson likelihood to model binary edges) but extending it for the weighted version is in fact easier and straightforward (where we can directly use the Poisson likelihood). Zhou (2015) also provides code for EPM which supports both unweighted and weighted graphs. Something like EPM would have been a very strong baseline.
- The experiments section is weak. Some of the reasons are:
 - The main regimes where the model outperforms other methods is when a large fraction of data is missing. However, there isn't enough of a justification as to why this is the case. It is also unsatisfying that with about 40%-50% of the network given as training data (and rest as test), the proposed method is actually outperformed by some of the other baselines (at least on 4 or 5 of the 6 datasets in Figure 1).
 - The non-MMSB baselines include SBM and WSBM and I think it provides somewhat of a weak comparison. As I mentioned above, models like EPM already exists for stochastic blockmodeling of weighted graphs using Poisson likelihood and it would have made the paper stronger to include some comparison with such methods.
 - All the experiments use $K=10$. I don't think this is a good idea. This number should be selected by cross-validation for each model. I think fixing $K=10$ for all models is somewhat unfair.
 - For imbalanced networks, AUC-PR score tends to be a more appropriate evaluation measure as opposed to AUC-ROC.

Overall, the basic idea of extending MMSB to weighted graphs is interesting (though straightforward) and the paper does a decent job at deriving an efficient SVI algorithm. However, the paper falls short of an adequate empirical evaluation and therefore doesn't appear to be ready for publication at this stage.

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2: Somewhat confident