Reviews For Paper

Paper ID 470

Title Network Inference by Learned Node-Specific Degree Prior

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Masked Reviewer ID: Assigned_Reviewer_1

Review:

Question	
Paper Score	Borderline, tending to accept
Confidence	Reviewer has understood the main points in the paper, but skipped the proofs and technical details, is not strongly experienced in the associated literature.
Please summarise your review in 1-2 sentences	The paper introduces a node degree prior for the network inference problem and proposes an iterative optimization scheme for conducting the inference. The method comes with theoretical guarantees and good experimental performance but the robustness of the method could have been studied further. * I'm satisfied with the author's response, my recommendation stays the same. *
Detailed Comments	The main contribution of the paper is the novel node-specific degree prior. It has both associated theoretical guarantees and promising empirical performance. The paper also reads nicely. My main concern regarding the proposed degree prior is that it seems to rely rather heavily on the assumption that the observed edges have been sampled uniformly and it is questionable how realistic this assumption is. If the assumption does not hold, it implies the following: 1) Theorem 2.2. presumably does not hold 2) The quality of the node degree estimates obtained using cross-validation is decreased. Furthermore, it does not seem likely that node-specific degree prior information would be readily available so cross-validation seems to be the only available option. Given these points, it would be good to include some discussion on how realistic is the uniform sampling assumption in various real-world settings where network inference problems are encountered. On the other hand, it is very useful that the authors provide some experimental results for the case when the uniform sampling assumption does not hold. The experiments show that violating

	the assumption degrades the performance of the proposed method compared to the benchmark methods, but the proposed method still outperforms them when hub node edges are over-/undersampled by 5%. However, in order to understand the robustness better, it would be interesting to see the performance as a function of sampling bias, going above the 5% difference. Does the method have a breaking point after which the alternative methods start to outperform it? Minor comments: * Theorem 2.2.: \sum_{i=1}^p d_p^* -> \sum_{i=1}^p d_i^* * Table 1 would be much clearer if you put methods (PU, Tri, ...) as columns and inputRelease-testRelease pairs (3.1.94-3 -> 3.2.104, 3.1.94-3 -> 3.2.114, and 3.1.104-3 -> 3.2.114) as rows * section X -> Section X * Eq.(Y) -> Eq. (Y) / Equation Y

Masked Reviewer ID: Assigned_Reviewer_2

Review:

Question	
Paper Score	Borderline, tending to accept
Confidence	Reviewer has understood the main points in the paper, but skipped the proofs and technical details, is not strongly experienced in the associated literature.
Please summarise your review in 1-2 sentences	Summary: This paper proposes a new approach for inferring the missing edges in a network. The main contribution of the paper is to propose a novel *node-specific* degree prior which they use to regularize the standard matrix completion based objective. Results are shown on inferring missing edges in real-world biological networks.
Detailed Comments	**I have read the authors' response and my recommendation stays the same.** Comments: The paper is well-written and motivates itself nicely. The proposed node specific degree prior appears novel and more general than the extant methods (which just use the global power law information)-- perhaps less so to someone who's an expert in this field. The experiments seem sound, except the one on protein-protein interaction networks where the authors use previous release of a dataset as the original graph and the newer release as the ground truth to be predicted. What does the BioGrid database use to update the links in the newer releases? I

	hope that they also don't predict (infer) the edges just as is done in this paper, as then it might not be a valid ground truth!

Masked Reviewer ID: Assigned_Reviewer_3

Review:

Question	
Paper Score	Borderline, tending to accept
Confidence	Reviewer has understood the main points in the paper, but skipped the proofs and technical details, is not strongly experienced in the associated literature.
Please summarise your review in 1-2 sentences	The authors present a method for network inference using a node-specific degree prior. The main innovation of the paper, according to the authors, is introducing the node-specific degree prior and analyze theoretically the recoverability of a network using the prior. They also develop a ADMM based algorithm to solve the associated optimization problem. They validate their method in protein-protein interaction network and show improvement with respect to competing methods.
Detailed Comments	My main concern with this contribution is that a very similar, if not identical, node-specific degree prior has been introduced by Tang et al., ICML 2015 (Eq. (7) in that paper -> Eq. (1) in the current paper), and that paper also contains a ADMM scheme. Therefore, the prior does not seem "novel", as the authors claim. I think it should be necessary to clearly spell out the differences with respect to that paper and why this contribution is substantial. There have been several papers on identifiability of networks, which also use the term network inference, published in recent years. It seems reasonable to make this distinction in the introduction: Daneshmand et al., ICML 2014 Pouget-Abadie & Horel, ICML 2015 ** I have read the authors' response and increased the overall score. I hope the authors clearly distinguish their work from Tang et al., ICML 2015 in the final version of the paper, if accepted. **

Masked Reviewer ID: Assigned_Reviewer_5

Review:

Question	

Paper Score	Clear accept
Confidence	Reviewer has understood the main points in the paper, but skipped the proofs and technical details, is not strongly experienced in the associated literature.
Please summarise your review in 1-2 sentences	The authors propose a node-degree regularized matrix completion approach for predicting unobserved/missing edges in a network from the observed edges. The paper is clearly presented with a good choice of experiments on synthetic and real data. I recommend its acceptance. UPDATE: Read the authors' response. My recommendation remains the same.
Detailed Comments	The authors use several relaxations of the original matrix completion problem that simplifies optimization, which involves non-negative matrix tri-factorization as one of the major algorithmic steps. Cross-validation is used for choosing several hyper-parameters. Operationally, their method tends to outperform a number of competing methods, and is quite robust to sampling bias as demonstrated by its performance against several sampling strategies. One comment regarding the plots: the authors use as the x-variable the percentage of the total possible edge pairs being predicted. It would be helpful to the readers if they clarify what do they mean by predicting a specific percentage of edge-pairs, as their algorithm estimates the full matrix X and then hard-thresholds its entries to obtain predicted edges.