NIPS 2013
Neural Information Processing Systems
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Reviews For Paper

Paper ID	1206
Title	Fast Approximate Quadratic Programming for Large (Brain) Graph Matching

Masked Reviewer ID: Assigned_Reviewer_4

Review:

Question	
Comments to author(s). First provide a summary of the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	The contribution addresses the problem of matching graphs, motivated by brain data where graphs are now pervasive. The problem is solved approximately via a relaxation of the original NP-hard problem using a Frank-Wolfe iterative scheme where the subproblem is solved via a Hungarian algorithm. The paper is well written and well motivated with a comprehensive experimental validation. The paper shares some strong similarities with the submitted work available at http://arxiv.org/pdf/1112.5507v1.pdf although the experimental benchmark results in page 5 and 6 seem to be new, with interesting results on neuroscientific data. Typos: p3 : missing space before "Moreover, at worst..." p3 : "the the barycenter"
Please summarize your review in 1-2 sentences	This paper is well written, relevant to the field and provides an extensive experimental validation. It however has a partial overlap with an apparently submitted paper.
Quality Score - Does the paper deserves to be published?	8: Top 50% of accepted NIPS papers
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	2: This work is different enough from typical submissions to potentially have a major impact on a subset of the NIPS community.

Confidence	4: Reviewer is confident but not absolutely certain

Masked Reviewer ID: Assigned_Reviewer_5

Review:

Question	
Comments to author(s). First provide a summary of the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	The authors propose a new algorithm for the quadratic assignment problem which they call FAQ. They demonstrate its use on a number of bench mark data sets, showing it is faster and finds good quality optima. They then demonstrate their work on finding "brain graphs" which are edges between the known anatomical structures of the brain. The writing in the paper is adequate though there are some unusual statements which should be changed. For example the authors refer to an algorithm as being intractable which is not the case, rather the problem is intractable. Some of the writing is also too colloquial for a scientific paper ("thusly" and "closest cousins" just sound odd) The underlying algorithms is fairly standard, gradient descent using the relaxation of their version of the QAP problem. I have several concerns about the paper in particular the author's claim that "We developed a fast approximate quadratic assignment algorithm" but there is no performance guarantee for their algorithm hence I am unsure why they say its an approximation algorithm. Furthermore, it is well known by Sahni and Gonzalez showed that no constant approximation scheme exists unless P=NP. The applied experimental results are also questionable. It is well known that QAP formulations are not the state of the art for brain mapping (i.e. see J. Burge, T. Lane, H. Link, s. Qiu, and V. Clark, Discrete Dynamic Bayesian Network Analysis of fMRI Data, Human Brain Mapping 30:122137 (2009). and J. Liu, P. Wonka, and J. Ye. Sparse Non-negative Tensor Factorization Using Columnwise Coordinate Decent, 45(1),

	649-656, Pattern Recognition, 2012.)
Please summarize your review in 1-2 sentences	The problem setting is of course interesting since Graph matching is popular, but the resultant formulation and results are not a contribution to the field.
Quality Score - Does the paper deserves to be published?	3: A clear rejection
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	1: This work is incremental and unlikely to have much impact even though it may be technically correct and well executed.
Confidence	4: Reviewer is confident but not absolutely certain

Masked Reviewer ID: Assigned_Reviewer_7

Review:

Question	
Comments to author(s). First provide a summary of the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	This paper presents a new algorithm for quadratic assignment problem and graph matching, and shows that it is faster and more accurate than state-of-the-art alternatives (in particular the PATH algorithm). The method is pretty simple and standard: write the objective function as a (non-convex) quadratic form f(P) over permutation matrices, relax over doubly stochastic matrices, find a local minimum (using Franke-Wolf, starting from the uniform matrix), and project back the local minimum to the set of permutation matrices by the Hungarian algorithm. The method bears similarities to QCV, the first step of the PATH algorithm where the same procedure is followed with another, convex quadratic form g(P). In fact, g(P) and f(P) are equal (up to a constant) over the set of permutation matrices, but differ on the set doubly stochastic matrices since g is convex and f is not convex in general. It is then surprising that minimizing f (ie, finding a local minimum) is better than finding the global minimum of g. If this interpretation is correct, would the authors have any opinion on this point?

	Also, I was intrigued by the sentence in the discussion "Zaslavskiy et al. seem to consider but discard FAQ [15] because they did not like projecting onto the set of permutation matrices". Did they consider it? If yes, the reason given for discarding it is strange, since they did the projection for the QCV method. The experimental results clearly demonstrate the superiority of the proposed method. The poor performance of PATH is surprising, in particular compared to alternative methods, since in the original PATH paper it was shown on the same benchmark to perform better. Could that be due to a bad use of the software? In particular, since the original PATH paper provides exact values of the objective function reached by PATH and other methods on 16 of the QAPLIB datasets, could the authors confirm that they reproduced these results?
Please summarize your review in 1-2 sentences	Relatively simple method, which surprisingly seems to work very well. Important problem (although not really machine learning).
Quality Score - Does the paper deserves to be published?	6: Marginally above the acceptance threshold
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	1: This work is incremental and unlikely to have much impact even though it may be technically correct and well executed.
Confidence	5: Reviewer is absolutely certain