From:	WWW 2017
To:	Roberto Togneri
Subject:	WWW 2017 notification for paper 459
Date:	Tuesday, 20 December 2016 6:39:16 AM

Dear Roberto Togneri,

We regret to inform you that your submission was not selected for the WWW 2017 conference:

459 : Finding Word Sense Embeddings Of Known Meaning

This year we received 966 valid submissions, 33% more than last year. We were able to accept only 164 of these, representing an accept rate of about 17%. Due to the limited number of available slots in the conference schedule, we had to make difficult decisions and decline many worthy submissions.

The program committee worked very hard to thoroughly review all the submitted papers and to provide action points to improve your paper. All papers were reviewed by at least three program committee members (and most were reviewed by four or more), and by track chairs (and in some tracks, also by a senior PC member) to oversee discussion amongst the reviewers, and to provide an overall recommendation for the paper. The reviews for your submission can be found below. We hope this feedback will be useful to revise your work for future submissions.

Please consider submitting your work to one of these other venues associated with WWW 2017. The majority of these venues have their submission deadlines set on January 8, 2017 (but please double check the individual web sites below):

- Posters: http://www.www2017.com.au/call-for-papers/posters.php
- Demos: http://www.www2017.com.au/call-for-papers/demos.php
- Workshops: http://www2017.com.au/call-for-papers/workshops.php
- Alternate tracks on
* Cognitive computing: http://www2017.com.au/call-for-papers/cognitive-computing.php
* Digital learning: http://www2017.com.au/call-for-papers/digital-learning.php
* Games, simulations and immersive environments: http://www2017.com.au/call-for-papers/games-simulations-and-immersive-environments.php

We very much hope you will be able to attend WWW 2017 in April in Perth, Australia. Further information about the conference can be found at http://www.www2017.com.au/

Best regards,

Eugene Agichtein and Evgeniy Gabrilovich
WWW 2017 Program Co-Chairs

---------- REVIEW 1 ----------
PAPER: 459
TITLE: Finding Word Sense Embeddings Of Known Meaning
AUTHORS: Lyndon White, Roberto Togneri, Wei Liu and Mohammed Bennamoun

Originality: 4
Impact: 2
Reproducibility: 3
Overall evaluation: 3

---------- Strong Points ----------
* Intuitive idea of how to get rom unsupervised senses (using word embeddings) to lexical senses (given by WordNet).
* Problems encountered during the research are presented, discussed, and solved in a meaningful manner.

---------- Weak Points ----------
* The evaluation could be strengthened, especially for the WSD part. The presented method is only compared to other embedding-based methods, not to other WSD methods. This is an oversight that needs to be addressed (or discussed why this is not possible). Looking at the results of SemEval [27] there are systems with a much better performance presented. I understand that this is not the key contribution of this paper, but the extrinsic evaluation by WSD is well chosen. The authors themselves state that this is just to demonstrate that their method captures senses, not as a viable WSD method on its own. Still, results should be presented properly.
* The refitting method does not work when it is used without smoothing. While the authors discuss this citing data sparsity issues, this is not fully convincing. As this is really the core contribution of the paper, a better analysis would be really useful here. It does not become clear why the smoothing works so much better for word similarity.
* The order of the paper could be improved: the method to derive the unsupervised word senses should be introduced or at least referenced briefly at the beginning of Section 3, so it becomes clear for the reader what the source for refitting is.
* The authors state that the improvement in the word similarity experiments is significant without doing significance tests - this is not good practice, significance tests should be applied.

---------- Detailed Review ----------
The paper presents a method that can predict lexicalized wordsenses (given by WordNet) from latent, induced word senses learned by word embedding methods. The ideas are intuitive and well presented, problems discussed and solved pragmatically. The order of presentation could be improved (see weak points).

To my understanding are two issues, one is more technical, one goes deeper:
1) Experimental need to be presented in a clear manner. The word similarity results need to include significance tests, and the WSD results need to show the state of the art explicitly in the tables, not just be described as "not really comparable". The readers should be able to easily draw their own conclusions, not have to search through all the details.
2) The first core contribution, the refitting, does not really work that well without additional smoothing. The smoothing is well motivated and presented, however it is unclear why the improvements are so dramatic.

All in all, despite the issues, the paper is still very interesting, as the intuition of refitting latent sense to lexical ones (especially via the use of example sentences) is an interesting avenue of research. Connecting embeddings/latent meanings to lexicalized ones is very important for using the ever larger knowledge bases that are available, and this paper makes a useful contribution. I'm sure the paper would be even more helpful to the scientific community if issues raised above are addressed.

---------- REVIEW 2 ----------
PAPER: 459
TITLE: Finding Word Sense Embeddings Of Known Meaning
AUTHORS: Lyndon White, Roberto Togneri, Wei Liu and Mohammed Bennamoun

Originality: 4
Impact: 2
Reproducibility: 3
Overall evaluation: -2

---------- Strong Points ----------
Word embeddings are a broadly useful technique

Being able to produce embeddings for difference senses can improve their accuracy in some applications

Specifying a sense via a single example seems like an easy and useful technique

---------- Weak Points ----------
Not a good fit for the WWW Semantics and Knowledge track

Lack of examples made the paper harder to read

---------- Detailed Review ----------
The work seems worthwhile and well done, but I think it's not a good fit for the WWW conference, not even in

its semantics and knowledge track. This track is focused on semi-structured knowledge in a Web context here as this paper describes a new technique to produce a word embedding for a word sense identified by the word and its use in a text fragment containing it.

The paper mentions 'Web' only twice in passing. The introduction points out that the refitting technique has better time complexity than the AvgSimC approach, "making this more a more suitable method for large scale web-scale datasets." Web is mentioned again in suggesting that the lower-time complexity makes the technique used for information retrieval. However, this still requires precomputing all of the word sense probabilities for all of the content words in all of the documents, a scenario that's somewhat discouraging.

A computational linguistics/NLP conference would be a better venue for this paper.

I thought that paper was well structured and well written. However it was dense and thus hard to read. I think some examples would have made it easier (for me a t least) to read.

---------- REVIEW 3 ----------
PAPER: 459
TITLE: Finding Word Sense Embeddings Of Known Meaning
AUTHORS: Lyndon White, Roberto Togneri, Wei Liu and Mohammed Bennamoun

Originality: 3
Impact: 2
Reproducibility: 2
Overall evaluation: -2

---------- Strong Points ----------
This paper proposes a novel "refitting" method to find the new word sense embeddings, with a smoothing technique to overcome the issue of dominant senses probabilities in case of limited training data; a similarity measure for words in context is also proposed.

The paper addresses an important and challenging problem.

---------- Weak Points ----------
Relevancy: the paper is not relevant for the conference

Presentation: the paper is not easy to read and follow

---------- Detailed Review ----------
The paper does not seem to be relevant w.r.t. the topics and interests of the conference/track, It would have been better if the authors discussed or illustrated how the problem of Word Sense disambiguation is related/important in web technologies.

Motivation and readability: although the paper is technically solid, it is not easy to read; and jumping to present only the technical achievements is not acceptable. It would be more helpful if the paper provided a motivation section to discuss why Word Sense Disambiguation is generally important; provide illustrative examples to explain the idea of refitting, smoothing, similarity measure for words, etc.; as well as, discuss the achievements and results of the experiment

Related work, although some important related work is presented in the paper, but more is needed. Quality papers typically provide a short state-of-the-art, rather than presenting only the too-close papers.

Experiment: the conducted experiment seems very promising, but it remains unclear how much improvements the authors made, compared with others.

---------- REVIEW 4 ----------
PAPER: 459
TITLE: Finding Word Sense Embeddings Of Known Meaning
AUTHORS: Lyndon White, Roberto Togneri, Wei Liu and Mohammed Bennamoun

Originality: 3
Impact: 3
Reproducibility: 4
Overall evaluation: -2

---------- Strong Points ----------
* A refitting method to find the new sense embeddings
* A smoothing technique for use with the refitting method
* A smilarity measure for words in context
* Well motivated and clear methodological descriptions
* Good evaluations
* Has the potential for broad impact in information retrieval and query answering

---------- Weak Points ----------
* underperforms as compared to other published methods
* performance is just barely better than using the most frequently occuring sense.
* the relevance of this work to WWW is poorly demonstrated.

---------- Detailed Review ----------
The authors propose a refitting method to generate a sense embedding vector that matches with a labelled lexical
sense. Thus, words in an example sentence can be disambiguated to maximally similar induced sense embeddings.

First, as a non expert in this area, I found the paper to be extremely well written - it is well motivated, clearly explained, provides context and comparison to related work, and gave me a good opportunity to learn about word embeddings and sense disambiguation. Thank you.

That being said, I believe that a key shortcoming of the paper is its relevance to WWW. Just saying "web scale" doesn't really convince me. Thus, the paper might be better received in a different venue, like ACL.

Second, the experiments perform do demonstrate that the approach taken is better than AvgSimC. However, AvgSimC is not the state of the art. The authors argue that this method could potentially be applied to other, better performing systems such as Chen. Demonstrating this might produce a solid incremental advance.

Third, I was really surprised to learn that the baseline most frequent sense basically performs at the same level as the proposed method. I don't know much about the SemEval corpus, and there is little description of how the method failed to vastly outperform in this scenario.

---------- REVIEW 5 ----------
PAPER: 459
TITLE: Finding Word Sense Embeddings Of Known Meaning
AUTHORS: Lyndon White, Roberto Togneri, Wei Liu and Mohammed Bennamoun

Originality: 3
Impact: 2
Reproducibility: 4
Overall evaluation: -2

---------- Strong Points ----------
1. Interesting idea for improvement of word embeddings, making them more useful.
2. Mostly clear presentation.

---------- Weak Points ----------
1. Relevance to the web data.
2. Past literature.
3. Minor problems in the experimentation.

---------- Detailed Review ----------
This paper extends word sense embeddings to accommodate known meanings, and shows how these

embeddings can be used, for example, for disambiguation.

The problem described in the paper is important, and the solution novel. The paper is mostly clear.

A few comments:
1. It is unclear to me how relevant this paper is to WWW. The authors test their method on non-web data.
2. Past literature: A simple web search found several seemingly relevant papers, for example, (Panchenko 2016), (Li et al, 2015). It would be good if the authors could address these papers.
3. No statistical significance testing is shown in the results. Thus, it is hard to say if the differences are meaningful.

I would suggest the authors address these issues and submit their paper to a more relevant conference.

---------- METAREVIEW ----------

There is no metareview for this paper