

ACM Transactions on Recommender Systems

Decision Letter (TORS-2022-0043)**From:** lichen@comp.hkbu.edu.hk**To:** qq20001224m@sjtu.edu.cn**CC:** qq20001224m@sjtu.edu.cn, yb57411@szpt.edu.cn, mathshenli@gmail.com, chiangel@sjtu.edu.cn, xinxin@sdu.edu.cn**Subject:** ACM Transactions on Recommender Systems - Decision on Manuscript ID TORS-2022-0043**Body:** Dear Ms. HANCHI HUANG,

I have received a recommendation from the Associate Editor in charge of your paper and regret to inform you that the paper has not been accepted for publication in TORS. Please see the summary recommendation of the Associate Editor and the reviewer comments below.

Thank you for your interest in ACM TORS, and best wishes with your continued research.

Dr Li Chen
Editor in Chief, ACM Transactions on Recommender Systems

Please find below the comments of the Associate Editor:

Associate Editor

Comments to the Author (required):

Dear authors,

we received all reviews for your manuscript and rendered a final decision, which is to reject your manuscript at this point in time. You can find the detailed comments and suggestions of the 3 reviewers below. To summarize the main concerns:

- The novelty of the approach is limited.
- The evaluation protocol is poorly described and unclear.
- The results are not reproducible.
- The paper is very hard to read and contains many mistakes (ortographically, grammatically, sentence structure, vague terminology, methodological description, lacking/unclear content).

Even though this is certainly not the outcome you had wished for, we nevertheless hope that the detailed comments will help you improve the manuscript and resubmit after a very careful revision at some later point in time.

Please find below the comments of the reviewers:

Reviewer: 1

Recommendation: Reject

Comments:

The authors provide a new perspective to adopt Reinforcement Learning to mention recommendation by considering each user as an agent. The technical part much follows the RL framework as expected. The key contribution would be to what extent the adopted RL model would benefit the application, mention recommendation.

There are related studies on mention recommendation, and the authors list a good number of benefits of mention recommendation in the first paragraph of Intro. However, it remains questionable on to what extent the need of mention recommendation to twitter or weibo users. That can be partially justified by the number of mentions in the collected data. However, at least in Twitter, a good number of mentions are by product of tweet reply. If a user replies a tweet, then the tweet carries an '@' sign and if the tweet is involved in a multi-user conversation on Twitter, the '@' signs are added to each reply to all users. These @ signs are generated

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automatically during a reply tweet. The question here is how many users would need the service of mention recommendation. The authors may need to consider a data analysis or user study to find out better support for this research problem.

The related work is poorly prepared. The most relevant work which also uses MARL approach is not well described. There is only one high-level sentence to tell the difference. The one sentence difference may be fine for models following different approaches, but for methods also using MARL, the one sentence is not sufficient.

The major issue is the experiment design. Table 7 is the only information for the datasets. It is not clear how the train/test data is split. How the experiments are conducted? Is the problem setting following the timeline of tweets/weibo? In a twitter conversation, users will mention each other why replying the tweet. Some users will be mentioned extensively by other users due to the hot topic (or bursty events). All these reply on dynamic context along time. Hence simple baselines like recent mentions, and recently frequent mentions shall be strong competitors. The experiment section has too many detailed information missing, hence it is hard to interpret the results.

Additional Questions:

What is the key contribution of this paper?: The paper adopts reinforcement learning to mention recommendation.

Novelty: Fair

Novelty and potential for innovation details: In general, this is yet another paper adopting RL to a specific problem setting, mention prediction in this case. While this is a common research practice, the novelty is not a key contribution here.

Relevance to the journal (TORS Scope Statement): Good

Relevance to the journal: The problem is indeed a recommendation problem.

Soundness

(Comment on the soundness of the idea, and the methods and techniques proposed.): Poor

soundness details: There are major issues with the problem formulation and experiments, see my detailed comments.

a) Experimental evaluation:

(Consider, for example: Do the authors use an appropriate methodology and suitable statistical methods? Is the choice of baseline methods in empirical comparisons well justified? Are the experiments reproducible?): Poor

Experimental evaluation details: It is not clear which train/test split is used and it is not possible to reproduce the results.

b) Theoretical background and evaluation:

(Consider, for example: Have the research questions and hypotheses been made explicit? Have all claims been substantiated through empirical evidence, theoretical considerations, or proofs?): Fair

Theoretical background and evaluation details: I have questions regarding the problem setting, the evaluation protocol, and the baselines.

Comparison with previous and related works: Fair

Comparison with previous and related works details:

Presentation:

(Comment about the quality of English and the structure of the paper: Are there parts that should be rewritten, expanded upon, or dropped? Is the paper concise?): Fair

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Presentation details: It seems that the authors are not very familiar with LaTeX commands. There are many missing references, making the paper a bit hard to follow.

Reasons to accept (Provide 1-3 concise bulleted-list items.): The authors provide a new perspective to adopt RL to a niche recommendation problem.

Reasons to revise/reject (Provide 1-3 concise bulleted-list items.): The authors do not provide a good understanding of the research problem itself.

There is no much details on the evaluation protocol and the results are not reproducible.

Does this paper have potential real world significance?: No

Real world significance comments:

Reviewer: 2

Recommendation: Major Revision

Comments:

This paper presents an interesting topic and innovative combination of several recent methods for the purpose of mention recommendation. The paper is almost well-written, except for several typos, forgotten references, low-quality figures used, and misplaced figures in the results section; however, in some parts, the paper is full of claims made without any scientific base. This makes the scientific evaluation of the work a little questionable. I think the paper can be accepted after a major revision. Please find my comments below.

1. Quality of Fig 1 needs to be improved. Do not include an image/screenshot from your diagram.

2. There is a need to a more compelling and scientific expression on why RL is preferred over other methods. Please refer to this survey for why RL should be used for RSs and cite it in your work:

"Afsar, M. Mehdi, Trafford Crump, and Behrouz Far. "Reinforcement learning based recommender systems: A survey." ACM Computing Surveys (CSUR) (2021)."

Similar information can be found in other surveys like:

"Lin, Yuanguo, et al. "A survey on reinforcement learning for recommender systems." arXiv preprint arXiv:2109.10665 (2021)."

"Since mention recommendation is a multi-round recommendation process involving the complex and dynamic relationship among the post publisher, the post's content, and other users on social networks, it seems more natural to use reinforcement learning (RL) instead of supervised learning which assumes environments' stationarity, and to use multiple agents instead of a single agent to model evolving interactions between users while making personalized decisions for each user."

3. I'd suggest adding a subsection in section 2 and survey MARL methods used for RSs. You should also explain what is MARL, their general classification, and why they are good for RSs. I also couldn't find a part in your literature review that sets your work apart from other methods. For example, you should explain Ref. [9] in more details and completely compare your work with it and say what is your advantages.

4. Precision, recall, and F1-score are metrics borrowed from Information Retrieval field. It should be mentioned somewhere in your paper.

5. thus slowly updated with DGNN via only one feedback at each round.--> why only one feedback? What happens if you provide more feedbacks?

6. *thre* --> can you use a better symbol for this?

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7. Ref for SeqConv [?] in page 8.

8. It is not obvious how static and dynamic parts are used/concat for state representation.
9. Action subsection of MDP is too complex to understand and needs rewording. Try to provide in a sentence what is the action of each agent? One/K users?
10. Customized experience replay. In this section, there are a lot of seemingly interesting contributions to the original ER. However, almost none of the contributions are supported by a scientific basis. For example, why do you cluster samples to 10 clusters and not more, or less. Or what is the reason of selecting 80% threshold for standard error? and many more reasoning like these.
Also, it seems that calculating all these six factors in every training round/step is heavy. There is no discussion about the overhead introduced in the method.
Also, is there an implementation you suggest for your custom ER? For prioritized ER paper, proposed by Schaul et al., they suggest various implementations for efficient use.
11. Quality of Fig. 3 and 4 should be improved.
12. The place of figures and tables in the results section should be adjusted, to improve readability. I.e., they must come after discussing them, not before.
13. Twitter public dataset [?] --> rereference.
14. The paper needs a section to discuss the shortcomings of the method proposed.

Additional Questions:

What is the key contribution of this paper?: A combination of several techniques is interesting.

Novelty: Good

Novelty and potential for innovation details:

Relevance to the journal (<https://dl.acm.org/journal/tors> target="_blank">TORS Scope Statement): Excellent

Relevance to the journal:

Soundness

(Comment on the soundness of the idea, and the methods and techniques proposed.):
Good

soundness details:

a) Experimental evaluation:

(Consider, for example: Do the authors use an appropriate methodology and suitable statistical methods? Is the choice of baseline methods in empirical comparisons well justified? Are the experiments reproducible?): Good

Experimental evaluation details:

b) Theoretical background and evaluation:

(Consider, for example: Have the research questions and hypotheses been made explicit? Have all claims been substantiated through empirical evidence, theoretical considerations, or proofs?): Good

Theoretical background and evaluation details:

Comparison with previous and related works: Fair

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Comparison with previous and related works details:

Presentation:

(Comment about the quality of English and the structure of the paper: Are there parts that should be rewritten, expanded upon, or dropped? Is the paper concise?): Good

Presentation details:

Reasons to accept (Provide 1-3 concise bulleted-list items.): Paper is well-written, provides enough contribution, and evaluation to be accepted

Reasons to revise/reject (Provide 1-3 concise bulleted-list items.):

Does this paper have potential real world significance?: Yes

Real world significance comments:

Reviewer: 3

Recommendation: Reject

Comments:

The paper aims at solving the "mention recommendation" problem on Social Networks, such as Twitter. It aims at extending state-of-the-art approaches by considering two yet neglected aspects: (1) the dynamic cooperation among users; (2) the sparsity of users' interactions. The authors propose a new algorithm named MA-DGNN which is a graph-based multi-agent reinforcement learning model. The proposed model is compared against baselines on real-world and synthetic datasets.

The paper is hard to read:

- it is rather vague in the description of the proposed solution and its evaluation;
- across the whole document the terminology is inconsistent;
- concepts are not formulated clearly;
- English needs to be proofread.
- Many missing references

Please find here my comments for each paper section. I report major issues I spotted.

Abstract:

MA-DGNN has two components

1. Time delayed aggregation graph policy network
2. Custom prioritised experience replay

In A: there is a "time delayed graph support operation"; it is unclear what it is. The author says it acts as a controller for large user networks with interaction dynamics and sparse communication. What is controlled is unclear. In (A) a multi-hop information diffusion mechanism helps users with sparse interactions to gain advice from users that are densely interacting. Also, this part is unclear to me, what is the help a user gains from this?

In B: 6 chosen metrics (which one?) are selected to deal with data imbalance.

User-user utility matrix is updated by decomposing the neural matrix based on similarity loss (this sounds unclear).

MARL -> credit assignment of what?

Policy Network and Critic networks -> where do they come from? What is a policy network? What is a critic network? Please, better contextualize your work by saying if it is based on actor-critic reinforcement learning.

I think that the provided details should not be part of the abstract since they do not tell much to the reader and creates confusion.

INTRODUCTION

The introduction presents the mention recommendation problem, the sketch of the proposed solution and the authors' contribution.

Figure 1 is not helping in explaining the presented solution functioning. The of the figure is misleading.

The end of the third paragraph should be expanded, the claims are general and there

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is no information about how the proposed solution can cope with the presented limitations. In particular, I refer to the "different level of relationships between users" and "related information" (related to what?).

In the 4th paragraph, the authors talk about state embedding. What is state embedding? The state representation in terms of features? Or do the authors refer to the state modelling of the MDP formulation of the mention recommendation problem? This is not clear.

Again in the 4th paragraph, the authors write "MARL, which combines the time-delayed graph operation of different orders to serve as distributed controllers...", this sentence is unclear. I can hardly understand what type of operations the authors refer to and what they should control. Similarly, the ambiguity used to describe the personalized replay buffer (PER) makes understanding the authors' idea difficult.

Due to these issues, I cannot comment positively on the claimed author's contribution (the fifth paragraph).

RELATED WORK

The section is too small for a journal paper. The paper selection is very small and the cited studies are not well explained. It looks like a list of papers that is there to fill space. In fact, there is no argumentation about the design choices of the authors as well as no contrast with the state of the art. This section is poor and needs to be rewritten.

METHODOLOGY

This section presents the designed solution.

The problem formulation (Section 3.1) is not clear and uses a sub-optimal notation. In the introduction, the authors say that mention recommendation is activated when the user explicitly triggers the mention function of a social network, ie., by clicking the "@" symbol. Here it seems that a user is always presented with a list of users to mention. The authors denote sets by using square brackets, this is not helping the reader. For instance, if N and T are the sets of users and time steps (interaction) why do the authors need to denote them as $[N]$ and $[T]$. The same is done in later sections where the authors leverage the notation $[6]$ which can be confused with the reference to paper 6. If they want to refer to a range, e.g., from 0 to 6 they should write $[0,6]$. The authors state that the objective is to score well in precision, recall and F1-score because they are widely used metrics in the mention recommendation papers. This is not convincing and I do not think metrics are part of the problem formulation. For instance, think about a mentioned recommendation model that should promote diversity and fairness in the exposition of social network members.

In Section 3.2 is presented the algorithm. Here it is unclear what are the network operations that were presented in the introduction. Moreover, they describe the user-user utility matrix without making clear what utility its values represent. It is unclear if the matrix contains values that come from the real values tight to the social network graph representation (each node is a user). How the value o_t is obtained is also unclear (linear combination of the user-user matrix and what else?).

The rationale of the user-user utility matrix for selecting candidates for the mention recommendations is not clear since the utility is not defined. Moreover, the usage of a threshold is also arbitrary. The section continues with the usage of vague terminology: critic loss (what it is?) and rewards (where do they come from and what they are?).

Subsection 3.2.1

The MDP formulation is not clear. State modelling is divided into static and dynamic parts; their definition is not satisfactory. Moreover, it is contrasting with Figure 2 where the static state contains "tweet" (I think it would be better to use the term post since tweet is specific to a Social Network) and user. According to the description, the static state consists only of a feature vector describing the user. Unfortunately, there is no information about the dimension of these vectors and a comprehensive description of the features and the motivation for considering them.

SeqConv reference is broken and it is not clearly explained why it is considered (why not other approaches?). MDP actions are not clear. The authors describe an action to be a concatenation of real values, but it should be, according to my intuition, a user to be mentioned. A policy π in reinforcement learning is a unique action a that is given by $\pi(s)=a$ where s is the current state of an agent. Rewards: the "K" term is confusing. Here the author says it is a round (of what?) and previously they use the same notation (special character) to denote the list of top-K users to recommend. The sentence in line 39 is ambiguous and not supporting the piecewise function on the same page. Does the reward punish or promote the prediction of real samples? Moreover, the reward is usually formulated as a function of the state, the

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state-action tuple or the tuple state-action-arrival state. Here the notation is different.

Subsection 3.2.2 is unclear. Formulas have inconsistent notation: line 31 misses an addend ϵ^0_i . Notation is confusing and needs to be simplified.

Line 6 of page 10: it is not clear what the cited model is doing.

Formula 7: it seems to be a notation problem. The base case $y_{\{0n\}}$ refers to x_n and the link with formula 7 is missing. What is derived in lines 24-26 is not clear.

Page 11 – Credit assignment. Terminology; do the authors refer to credit or reward? The state definition is saying something different from the MDP formulation. It was not clear that a state is a concatenation of all states as time t . Line 19: Policy loss – terminology, might be the loss of the Policy Network used by the authors. Clearly, a preliminary knowledge section and an improved presentation are needed. Line 22: weight coefficients: the weights are already values do we need the word coefficient here? Here it seems that the user-user utility matrix contains rewards. In the previous section, this was not clear.

Section 3.2.3

Also, this section needs to be rewritten for clarity. What are comprehensive decisions? The term credit is used instead of a reward. The authors construct the user-user utility matrix to obtain weights of linear combinations. This is unclear.

Neural matrix factorization is the model used to learn the user-user matrix. A convincing motivation for using this model is not given. Line 38: there are broken references.

DISCUSSION

This section looks like a rebuttal phase with reviewers' questions and authors' answers. This is not giving much to the paper. All the questions are unrelated, a logical flow is absent.

Line 13: "Also, it is hoped that by using DGNN, ..." apart from the lack of clarity. My interpretation is that the authors want to say that DGNN is useful to generate mention recommendations to users with sparse interactions (not active in the social network) by leveraging information about active users (those who mention frequently?). The authors say that they "hope" DGNN can achieve this objective and there is no evidence for this. Moreover, the experimental setting to prove this is arbitrary and does not use the information of active users.

Line 15: MADDPG first time the acronym appears in the document. "...each intelligence.." which intelligence? Do the authors refer to a Reinforcement Learning agent?

EXPERIMENTS

Research questions are not connected with the paper. They look like a standard procedure extracted from a machine learning textbook: performance against baselines, effects of hyperparameters to the model, influences of model component... to something that is not explained (RQ3).

Line 20: missing reference.

Lines 7-8 page 16: Not clear. A,b,c appear to be linked to category I,II and III on page 15.

Section 5.1.4: it is not saying much. Are the hyperparameters tuned via train-validation testing? Is the same done for all the baselines? Are the baselines optimized for the selected metrics? What is the detailed experimental procedure (data splitting and recommendation generation)?

Sections 5.3, 5.4 and 5.5 are not giving enough details. The obtained results are not commented by contrasting with baselines models. In particular, sections 5.4 and 5.5 are questionable according to their current status. See my previous comments and the text in the review forms.

There are not statistical tests to support the obtained results.

Language issues:

Line 7 "...potential mentionees achieved by a global scoring function."

Line 20 "...the target user would prefer to hit them ..."

Table 7: "Twitters" / Weibo -> posts or microblog posts / What is the meaning of "#Avg. Mention per user" and "#Avg. Mentioned per user"?

...

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Additional Questions:

What is the key contribution of this paper?: The paper aims at solving the "mention recommendation" problem on Social Networks, such as Twitter. It aims at extending state-of-the-art approaches by considering two yet neglected aspects: (1) the dynamic cooperation among users; (2) the sparsity of users' interactions. The authors propose a new algorithm named MA-DGNN which is a graph-based multi-agent reinforcement learning model. The proposed model is compared against baselines on real-world and synthetic datasets.

Novelty: Fair

Novelty and potential for innovation details: The authors tackle a well-known problem, i.e., mention recommendation by proposing a solution that relies on multiple components which are based on a variety of existing techniques. The designed solution builds heavily on papers [33] and [18].

[33] Ekaterina Tolstaya, Fernando Gama, James Paulos, George Pappas, Vijay Kumar, and Alejandro Ribeiro. 2020. Learning decentralized controllers for robot swarms with graph neural networks. In Conference on Robot Learning. PMLR, 671–682

[18]] Shihui Li, Yi Wu, Xinyue Cui, Honghua Dong, Fei Fang, and Stuart Russell. 2019. Robust multi-agent reinforcement learning via minimax deep deterministic policy gradient. In Proceedings of the AAAI Conference on Artificial Intelligence, Vol. 33. 4213–4220.

Relevance to the journal (TORS Scope Statement): Good

Relevance to the journal: The paper clearly fits the scope of the journal.

Soundness

(Comment on the soundness of the idea, and the methods and techniques proposed.): Poor

soundness details: The idea, method and evaluation are not sound. There is a misuse of the terminology across the whole paper. For instance, the formulation of the Markov Decision Problem is not clear and is not linked to the objective of the study. The different components of the solution come from existing literature but are never mentioned or discussed in the Related Work section. A preliminary knowledge section is appropriate for the paper. From the Introduction to the Method section the logical flow of the presented concepts is missing. Authors many times refer to concepts that are introduced (partially) in later sections of the document (without linking to them). This makes the paper extremely hard to read. Hence, it is hard to understand the modelling choices of the authors to design the proposed solution.

a) Experimental evaluation:

(Consider, for example: Do the authors use an appropriate methodology and suitable statistical methods? Is the choice of baseline methods in empirical comparisons well justified? Are the experiments reproducible?): Poor

Experimental evaluation details: The methodology is not sound and lacks many details: dataset splitting; validation of all the models (including baselines). Moreover, the used metrics are not justified. I.e, why reporting precision, recall, F1, hit rate and MRR is not commented on. The experiment to show the goodness of the method in suggesting users mentions in a microblog post is based on a totally arbitrary decision about sampling users from an existing dataset (no information about tuning and training all the compared models is given).

The authors say that the code of the existing solution will be made available in a second moment to the paper readers. It is unclear if they will release also all the model validation, hyper-parameters selection and testing (the paper does not comment on any of these).

b) Theoretical background and evaluation:

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(Consider, for example: Have the research questions and hypotheses been made explicit? Have all claims been substantiated through empirical evidence, theoretical considerations, or proofs?): Fair

Theoretical background and evaluation details: Research questions are made explicit, but they are not linked to the paper and seem to be the typical tasks of a Machine Learning experiment from a textbook. The model and baseline validation and optimization are missing. Hence, there are many doubts about the presented results. Moreover, the authors do not use any statistical test to support their claims.

Comparison with previous and related works: Poor

Comparison with previous and related works details: The related work section is very short and needs to be further improved. In particular, the authors focus on the topics "mention recommendation" and "reinforcement learning in recommender systems". The proposed solution does not result to be contextualized and justified against the few cited papers. Design choices are not linked to current needs given the existing methods.

Presentation:

(Comment about the quality of English and the structure of the paper: Are there parts that should be rewritten, expanded upon, or dropped? Is the paper concise?): Poor

Presentation details: I am not a native speaker, but I think the paper needs proofreading to fix the flow of the communication and improve clarity. Several parts of the paper are hard to read due to sentence structure, vague terminology and lacking content. In particular, this is affecting the method section which needs to be completely rewritten. Detailed comments are given in the authors' comments.

Reasons to accept (Provide 1-3 concise bulleted-list items.): This paper:

- fit journal scope
- tackle a relevant topic in the RSs community
- proposes a new algorithmic solution


Reasons to revise/reject (Provide 1-3 concise bulleted-list items.): This paper:

- lacks a sound method presentation; the proposed solution is rather complex and not clearly explained.
- lacks preliminary knowledge and a developed related work section that makes the paper self-contained. The authors mix findings of other papers in the method section
- lacks a sound evaluation. Datasets are introduced, but information about training, validation and testing is not given. Chosen hyperparameters are not justified and there is no information about the optimization of the baselines.
- there is a discussion section that looks like answers to comments from a previous rebuttal phase

Does this paper have potential real world significance?: No

Real world significance comments: The problem the authors aim at solving is well-defined and known to the RSs, Information Retrieval and Machine Learning community. The proposed solution is vague, not well justified and contextualized within the framework of existing solutions. Hence, I cannot comment on the real-world significance of this study.

Date Sent: 09-Jan-2023

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