Dr. Eick

COSC 6335*“Data Mining”*

ProblemSet3 Fall 2024

Reviewing Data Mining Papers

Deadlines: Your Task5 report is due on Saturday, November 16 in Kritik; however, some Kritik peer reviewing of other groups’ submissions still needs to be done in the window November 17-22, 2024.

Last updated: October 28 at 3p

Task 5: Reviewing a Data Mining Paper

Peer-Reviewed Group Project

# In this task you will review the paper titled “*Unsupervised Deep Subgraph Anomaly Detection*” by Zheng Zhang, and Liang Zhao which was the winner of an ICDM 2022 paper award; this is the link to download the paper:

[Unsupervised Deep Subgraph Anomaly Detection](https://www2.cs.uh.edu/~ceick/DM/AA2022.pdf)

The Task1 groups, we formed early September, will be used for Task5.

After you understood what this paper is all about, conduct a web search looking for papers with similar themes as the paper you need to review. Next, write your review, complying with the following review template:

1. Summarize what the research area and the topic of the paper is and what its contributions are 3-4 paragraphs); *write in a neutral or positive tone no matter how bad the paper is*!
2. Evaluate the contributions of the paper and its writing (at 3-7 paragraphs); follow the questions and criteria of the KDD 2012 Reviewing Criteria (you can find at the end of this document as an appendix). In particular, assess the **novelty**, **technical quality**, **significance and impact**, and **clarity of writing of the paper**. If the paper makes contributions that do not fit into these 4 criteria, summarize those in an optional “*other contributions” paragraph*)
3. What are the 3 strongest points of the paper (just one sentence for each point)?
4. What are the 1-3 weakest points of the paper (just one sentence for each point)?
5. Assess the educational value of the paper for graduate students (2-3 paragraphs)! Is the paper a good starting point to do work/research in the area? Does the paper do a good job in introducing the goals and objectives and the methods of the field of research? Does the paper do a good job in getting graduate students excited about working in the research field? What did you learn from reading the paper?
6. Numbered List of Specific Comments and Questions (e.g. *the claim stated in the second paragraph is not clear; I do not agree with the conclusion in the third paragraph…, symbol x was never defined, it is not clear to me what the purpose of Section 4.3.2 is; the author introduced formulas 2.4 that are never used in the remainder of the paper*, I do not understand what the term x means,…). Each review should have 5-8 specific questions/comments!
7. Summarize the findings of the web-search you conducted; also evaluate what you found and how it relates to the paper your reviewed (3-6 paragraphs)
8. Broader Impact (2-3 paragraphs); what real world applications will arise from this work? Assess how the paper will help society to make earth a better place! Does the paper foster new research/new approaches that could be investigated in future research? Does it establish new connections between different, originally disconnected research communities?
9. The paper won the 1st Place of the ICDM 2022 Best Paper Award. Explain why you believe this happened! (1-2 paragraphs)
10. Give the paper a numerical score (1-7) using the KDD-2012 Criteria (described in appendix 1); 7 scores (add scores for educational value, broader impact and overall score!)!
11. Assess the usefulness of Task5! (1 paragraph)!

**Rubrics**:

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | Level 0 | Level 1 | Level 2 | Level 3 | Level 4 | Weight |
| Summary:  Research Area,  Topic,  Contributions,  Tone | All four points identified by the reviewer group are full of errors and rude | One point out of four identified by the reviewer group have many errors  And tone is slightly rude | Two points out of four identified by the reviewer group have some error  And tone is mild rude | Three points out of four identified by the reviewer group have almost no error And tone is mildly motivating | All four points identified by the reviewer group have almost no error And tone is motivating | 3 |
| Evaluating Contributions:  **novelty**,  **technical quality**,  **significance and impact**,  and **clarity of writing of the paper** | All four points identified by the reviewer group are full of errors | One point out of four identified by the reviewer group have many errors | Two points out of four identified by the reviewer group have some error | Three points out of four identified by the reviewer group have almost no error | All four points identified by the reviewer group have almost no error | 5 |
| Strongest Points | None of the points are valid | All three points are valid but weak | One out of three points are valid and strong | Two out of three points are valid and strong | All three points are valid and strong | 2 |
| Weakest Points | None of the points are valid | All three points are valid but weak | One out of three points are valid and strong | Two out of three points are valid and strong | All three points are valid and strong | 2 |
| Assess the educational value of the paper for graduate students (2-3 paragraphs)!  Is the paper a good starting point to do work/research in the area?  Does the paper do a good job in introducing the goals and objectives and the methods of the field of research?  Does the paper do a good job in getting graduate students excited about working in the research field?  What did you learn from reading the paper? | One or zero out of five points discussed by the reviewer group are well written | Two out of five points discussed by the reviewer group are well written | Three out of five points discussed by the reviewer group are well written | Four out of five points discussed by the reviewer group are well written | All five points discussed by the reviewer group are well written | 3 |
| Specific Comments and Questions | No comments are valid | Most of the comments are weak or invalid | Half of the comments are valid and strong, other half are weak or invalid | Most comments are valid and strong | All comments are valid and strong | 2 |
| Summarize the findings of the web-search you conducted; also evaluate what you found and how it relates to the paper your reviewed | No proper web-search is done by the reviewer group | Reviewer group has proof of doing ordinary amount of web search and the discussion from the web search are also ordinary and not related to the paper’s topic | Reviewer group has proof of doing average amount of web search and the discussion from the web search are also average and marginally related to the paper’s topic | Reviewer group has proof of doing somewhat good amount of web search and the discussion from the web search are okay and highly related to the paper’s topic | Reviewer group has proof of doing extensive web search and the discussion from the web search are proper and fully related to the paper’s topic | 2 |
| Broader Impact (2-3 paragraphs); what real world applications will arise from this work?  Assess how the paper will help society to make earth a better place!  Does the paper foster new research/new approaches that could be investigated in future research?  Does it establish new connections between different, originally disconnected research communities? | All four points identified by the reviewer group are full of errors | One out of four points discussed by the reviewer group are well written and have almost no error | Two out of four points discussed by the reviewer group are well written and have almost no error | Three out of four points discussed by the reviewer group are well written and have almost no error | All four points discussed by the reviewer group are well written and have almost no error | 5 |
| The paper won the ICDM 10-year Highest Impact Award. Explain why, you believe, this happened! | Discussion made by the reviewer group is full of erroneous claims | Discussion made by the reviewer group is below average but huge amount of erroneous claims | Discussion made by the reviewer group is average but has many erroneous claims | Discussion made by the reviewer group is okay but has some errors | Discussion made by the reviewer group is valid and strong | 2 |
| Give the paper a numerical score (1-7) using the KDD-2012 | The rank is totally wrong | The rank is very far from the justified rank | The rank is slightly far from the justified rank | The rank is very close from the justified rank | The rank is justified and okay | 1 |
| Assess the usefulness of Task5 | Discussion made by the reviewer group is full of erroneous claims | Discussion made by the reviewer group is below average but huge amount of erroneous claims | Discussion made by the reviewer group is average but has many erroneous claims | Discussion made by the reviewer group is okay but has some errors | Discussion made by the reviewer group is valid and strong | 2 |

**Appendix 1: KDD 2012 Reviewing Criteria: Research Track**

Below we have provided some guidelines to reviewers on how to write reviews, both the content of reviews and also how the numerical scoring system works. Many of the suggestions below have been liberally borrowed from other conferences - so thanks to the many folks who have contributed to writing these types of "guidance" pages in the past.

**Writing Reviews: Content** (Edited by Ch. Eick)

For each paper you will provide written comments under each of the headings below. Your review should address **both the strengths and weaknesses of the paper** - identify the areas where you believe the paper is particularly strong and particularly weak - this will be very valuable to the PC Chairs and the SPC.

* **Novelty**: This is arguably the single most important criterion for selecting papers for the conference. Reviewers should reward papers that propose genuinely new ideas or novel adaptations/applications of existing methods. It is not the duty of the reviewer to infer what aspects of a paper are novel - the authors should explicitly point out how their work is novel relative to prior work. Assessment of novelty is obviously a subjective process, but as a reviewer you should try to assess whether the ideas are truly new, or area novel combinations or adaptations or extensions of existing ideas, or minor extensions of existing ideas, and so on.
* **Technical Quality**: Are the results sound? Are there obvious flaws in the conceptual approach? Did the authors ignore (or appear unaware of) highly relevant prior work? Are the experiments well thought out and convincing? Are there obvious experiments that were not carried out? Will it be possible for later researchers to replicate these results? Are the data sets and/or code publicly available? Did the authors discuss sensitivity of their algorithm/method/procedure to parameter settings? Did the authors clearly assess both the strengths and weaknesses of their approach?
* **Potential Impact and Significance**: Is this really a significant advance in the state of the art? Is this a paper that people are likely to read and cite in later years? Does the paper address an important problem (e.g., one that people outside machine learning and data mining are aware of) or just a problem that only a few researchers are interested in and that wont have any lasting impact? Is this a paper that researchers and/or practitioners might find useful 5 or 10 years from now? Is this work that can be built on by other researchers?
* **Clarity of Writing**: Please make full use of the range of scores for this category so that we can identify poorly-written papers early in the process. Is the paper clearly written? Is there a good use of examples and figures? Is it well organized? Are there problems with style and grammar? Are there issues with typos, formatting, references, etc? It is the responsibility of the authors of a paper to write clearly, rather than it being the duty of the reviewers to try to extract information from a poorly written paper. Do not assume that the authors will fix problems before a final camera-ready version is published - unlike journal publications, there will not be time to carefully check that accepted papers are properly written. Think of future readers trying to extract information from the paper - it may be better to advise the authors to revise a paper and submit to a later conference, than to accept and publish a poorly-written version.
* **Additional Points (optional)**: this is an optional section on the review form can be used to add additional comments for the authors that don’t naturally fit into any of the areas above.

**Comments that are only for the SPC and PC (optional)**: again this is another optional section. If there are any

**General Advice on Review Writing**: please be as precise as you can in your comments to the authors and avoid vague statements. Your criticism should be constructive where possible - if you are giving a low score to a paper then try to be clear in explaining to the authors the types of actions they could take to improve their paper in the future. For example, if you think that this work is incremental relative to prior work, please cite the specific relevant prior work you are referring to. Or if you think the experiments are not very realistic or useful, let the author(s) know what they could do to improve them (e.g., more realistic data sets, larger data sets, sensitivity analyses, etc).

**Writing Reviews: Numerical Scoring**

For KDD-2012 we are using a 7-point scoring system. We strongly encourage you to use the full range of scores, if appropriate for your papers. Try not to put all of your papers in a narrow range of scores in the middle of the scale, e.g., 3s, 4s, and 5s. Don’t be afraid to assign 1s/2s, or 6s/7s, if papers deserve them. If you are new to the KDD conference (or have not attended for a number of years) you may find it useful to take a look at online proceedings from recent KDD conferences to help calibrate your scores. The scoring system is as follows:

* 7: An excellent paper, a very strong accept. I will fight for acceptance, this is potentially best-paper material.
* 6: A very good paper, should be accepted. I vote and argue for acceptance, clearly belongs in the conference.
* 5: A good paper overall, accept if possible. I vote for acceptance, although would not be upset if it were rejected because of the low acceptance rate.
* 4: Decent paper, but may be below KDD threshold I tend to vote for rejecting it, but could be persuaded otherwise.
* 3: An OK paper, but not good enough. A rejection. I vote for rejecting it, although would not be upset if it were accepted.
* 2: A clear rejection. I vote and argue for rejection. Clearly below the standards for the conference.
* 1: A strong rejection. I'm surprised it was submitted to this conference. I will actively fight for rejection.