

# View Reviews

<b>Paper ID</b>	609
<b>Paper Title</b>	Line Simplification Algorithms for Trajectory Compression [Experiments and Analyses]
<b>Track Name</b>	Research -> February 2019

## Reviewer #2

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### Questions

#### 1. Overall Rating

Reject

#### 2. Relevant for PVLDB

Yes

#### 3. Are there specific revisions that could raise your overall rating?

No

#### 4. Summary of the paper (what is being proposed and in what context) and a brief justification of your overall recommendation. One paragraph

Using real datasets, the authors review and evaluate a number of line simplification algorithms under several different perspectives. I'm unable to recommend its publication because I find the (large) number of experiments are largely of quantitative nature without guiding the users to which algorithm to use depending on the characteristic of the their dataset, which I think would be more interesting from a user perspective.

#### 5. Three (or more) strong points about the paper (Please be precise and explicit; clearly explain the value and nature of the contribution).

S1. A large number of algorithms are investigated.

#### 6. Three (or more) weak points about the paper (Please indicate clearly whether the paper has any mistakes, missing related work, or results that cannot be considered a contribution; write it so that the authors can understand what is seen as negative)

W1. There's no discussion re how the data distribution affects (or not) the performance of the various investigated algorithms.

W2. The discussion is basically quantitative, with so many number and techniques that it becomes overwhelming.

W3. The description of the investigated algorithms are hard to follow. Similarly, the discussion of the experiments is also hard to follow. (A running example would help a lot in that respect.)

**7. Novelty (Please give a high novelty ranking to papers on new topics, opening new fields, or proposing truly new ideas; assign medium ratings to delta papers and papers on well-known topics but still with some valuable contribution).**

Novelty unclear

## **8. Significance**

Improvement over existing work

## **9. Technical Depth and Quality of Content**

Syntactically complete but with limited contribution

## **11. Presentation**

Sub-standard: would require heavy rewrite

## **12. Detailed Evaluation (Contribution, Pros/Cons, Errors); please number each point**

While I appreciate the substantial amount of work the authors put in this paper I found it to be basically on the quantitative side whereas I think there would have (much) more value if it were on the qualitative side. The use of real datasets is laudable, but just reporting how each of techniques performed is of limited values given no real insight on the dataset's characteristics. (Indeed, the authors make no effort to describe the datasets, not even their sizes are reported!.) That is, what I think it would be (much) more important for a user to learn would be "given the characteristics of my dataset" which technique would work better? This would raise important questions, e.g., how to characterize the datasets, and I believe that would be worth publishing results.

Also, while the paper does try to have a survey like value, it does not quite accomplish that goal as the description of the algorithms are too hard to read. For instance, each technique should be illustrated (to some extent) on a running example, that would add "tutorial value" to the paper. I do understand that given the limited space there's not much one can do it, but I'd not blame the venue limitations, but rather the choice of venue. I'll add to that the this kind of research would be much more valuable for SIGSPATIAL's audience than VLDB's.

Given all the issues above I'm not able to recommend the paper's publication at VLDB, but I also need to make it clear that I don't see any technical flaw, rather I see limited value in the paper as it is, and I'd suggest the authors to consider publishing it (after adding a qualitative component to the comparison) to a journal where they'd not be constrained by space.

Minor issues:

The caption of Table 1 seems to inconsistent with the table itself.

If one uses lat-long then the use of Euclidean distance is a good approximation but is not the right way to measure distance (unless one assumes the planet is flat!)

There a few typos and grammar errors that require a careful revision by the authors.

**Reviewer #3**

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## Questions

### 1. Overall Rating

Weak Reject

### 2. Relevant for PVLDB

Yes

### 3. Are there specific revisions that could raise your overall rating?

No

### 4. Summary of the paper (what is being proposed and in what context) and a brief justification of your overall recommendation. One paragraph

This experimental paper systematically evaluates 11 representative line simplification algorithms for trajectory data on four datasets using three error metric.

### 5. Three (or more) strong points about the paper (Please be precise and explicit; clearly explain the value and nature of the contribution).

S1. Chose a nice collection of LS algorithms

S2. Solid and comprehensive experiments

S3. Insightful observation and discussions

### 6. Three (or more) weak points about the paper (Please indicate clearly whether the paper has any mistakes, missing related work, or results that cannot be considered a contribution; write it so that the authors can understand what is seen as negative)

W1. Could have analyzed some more aspects for the trajectory

W2. Limited extra value is offered beyond the previous experimental paper for this topic.

### 7. Novelty (Please give a high novelty ranking to papers on new topics, opening new fields, or proposing truly new ideas; assign medium ratings to delta papers and papers on well-known topics but still with some valuable contribution).

Novelty unclear

### 8. Significance

Improvement over existing work

### 9. Technical Depth and Quality of Content

Insignificant contribution

### 11. Presentation

Reasonable: improvements needed

### 12. Detailed Evaluation (Contribution, Pros/Cons, Errors); please number each point

1. The experiment studied the impacts of sampling rates and points per trajectory on the LS performance. Actually, some other features of trajectories can also heavily affect the LS performance. For example, the shape complexity (number of sharp turning points) of trajectory can have impact on the compression ratio and average error for PED and DAD metrics. The speed variance of a trajectory can have impact on the SED metric.

2. The unit of the error parameter epsilon is incorrect in the experiments on SED metric, e.g., Figure 2, 8, 14. It should be meters rather than degree.

## Questions

### 1. Overall Rating

Weak Reject

### 2. Relevant for PVLDB

Yes

### 3. Are there specific revisions that could raise your overall rating?

No

### 4. Summary of the paper (what is being proposed and in what context) and a brief justification of your overall recommendation. One paragraph

This paper has an extensive study on Line Simplification algorithms. Although there is great effort in this study, I am not sure about its appropriateness to VLDB.

### 5. Three (or more) strong points about the paper (Please be precise and explicit; clearly explain the value and nature of the contribution).

- Extensive experimentation
- Clear English presentation
- Extensive related work section

### 6. Three (or more) weak points about the paper (Please indicate clearly whether the paper has any mistakes, missing related work, or results that cannot be considered a contribution; write it so that the authors can understand what is seen as negative)

- Results are not surprising or better said not different to what one would expect in most cases
- Not sure enough is added beyond [41] which was presented in VLDB recently
- Too many results without insightful discussion are put into the paper
- Experiments feel quite synthetic

### 7. Novelty (Please give a high novelty ranking to papers on new topics, opening new fields, or proposing truly new ideas; assign medium ratings to delta papers and papers on well-known topics but still with some valuable contribution).

Novelty unclear

### 8. Significance

Improvement over existing work

### 9. Technical Depth and Quality of Content

Syntactically complete but with limited contribution

### 11. Presentation

Reasonable: improvements needed

### 12. Detailed Evaluation (Contribution, Pros/Cons, Errors); please number each point

This paper is a detailed evaluation paper with good potential. However, I am not sure whether VLDB is a good fit to the paper or the paper can be made to fit in time... thus the paper is not in its final form yet in my view.

There is already a study on the topic as cited in [41]. Although this paper gives more info, I am not sure whether the results are surprising enough to warrant another

publication in VLDB on this topic. I believe a long journal paper submitted to ACM TODS or TSAS for example would be a better fit. This would also allow for further discussions and insights to be presented about the experiment which is currently lacking.

A key problem is lack of clarification on the queries front. What does error in this research domain mean without an example/reason and a clear query definition. A seemingly small error due to simplification for one application is totally unacceptable for another application. The comparison should be done in light of an application domain discussion that is missing.

There are long sections of numbers quoted in the text. These should rather be converted to discussions about how one approach is better than the other given the context of an application domain. There needs to be guidance for practitioners to make use of this paper with that addition.

There is a large number of figures with very small captions and many lines in charts close to each other. There is some "zooming" effect done by authors but it actually makes more of a case that this is a long, almost survey like, paper.

The beautiful presentation with the table upfront is not repeated with the results section. In part perhaps results are relatively what is expected as also stated by the authors for many cases.

There are means of converting this paper to a more succinct paper with focus on the diff with [41] and also summarizing expected results, using application domains perhaps, and focusing on interesting results. But not sure whether this can be done in time for VLDB. I think submitting this work to some other venue would be a better option.