View Reviews

Paper ID
477

Paper Title
Semantic Segmentation for Compound figures

Reviewer #2

Questions

1. How confident are you in your evaluation of this paper?
2: Confident

2. Importance/ Relevance
2: Of limited interest

3. Novelty/Originality
3: Moderate original

4. Technical Correctness
3: Probably correct

5. Experimental Validation
3: Limited but convincing

6. Clarity of presentation
3: Clear enough

7. Reference to prior work
3: Reference adequate

8. Overall evaluation of the paper
2: Weak Reject (Recommend for ICME Workshops)

10. Additional comment to author:
The paper Semantic Segmentation for Compound Figures presents a method for detecting sub-figures on paper images. The author divided the task in two, first detecting the sub-figure label, and second, detecting the sub-figures. The author defined some constraints for the proposed model, it cannot be used if the image labels are not present on the images. They try different models for the label detection and compared their approach with a previous study with similar objective. The paper needs to be revised by the author and it means to be rejected. See some comments below.

(1) In the Abstract, if possible add more details of the results obtained in this study.
(2) In Section 2, Related Works, explain what are the results of the mentioned models, where those models fail and what are the strengths of each one. If possible add some previous results obtained with these models for similar studies and compare with them.
(3) In Section 3, it would be better if you explain the models used for both, the subfigure label detector and master image detector, is not clear which model is used.
(4) In Section 4, Results, it would be good if you can compare the proposed method with more than one model. The results are just compared with the results obtained by Tsutsui in “A data driven approach for compound figure separation using convolutional neural networks.”
(5) In Section 5, Conclusion, it is the first time in the paper that you mentioned that the proposed method is anchor-based, if possible add more details of this technique on Section 2 and 3, what are the parameters that you used on

this study for the anchor-box?
(6) Improve the figures and increase the font-size of the labels.

Reviewer #3

Questions

1. How confident are you in your evaluation of this paper?
3: Very confident

2. Importance/ Relevance
3: Of sufficient interest

3. Novelty/Originality
2: Minor originality

4. Technical Correctness
3: Probably correct

5. Experimental Validation
2: Lacking in some respect

6. Clarity of presentation
2: Difficult to read

7. Reference to prior work
3: Reference adequate

8. Overall evaluation of the paper
2: Weak Reject (Recommend for ICME Workshops)

9. Justification (required if score of 1 or 2 has been selected for questions 3-7):
The novelty of the paper is minor. The presented approach is rather descriptive, than technical and mainly focuses on data augmentation for improving the performance of existing methods.
There are problems in the presented approach, since it proposes a sequential methodology, in which the performance of the object detection & visual relationship extraction heavily depends on the quality of subfigure label detection. No analysis of this dependence is examined in the paper. Well known classifiers, such as ResNet ones, as well as networks, such as YOLO ones, are used for the respective detection tasks.
There are many language and presentation problems throughout the paper which make it difficult to read.

10. Additional comment to author:
Please see the comments provided above.

Reviewer #4

Questions

1. How confident are you in your evaluation of this paper?
1: Less confident

2. Importance/ Relevance
2: Of limited interest

3. Novelty/Originality
2: Minor originality

4. Technical Correctness

3: Probably correct

5. Experimental Validation
2: Lacking in some respect

6. Clarity of presentation
2: Difficult to read

7. Reference to prior work
3: Reference adequate

8. Overall evaluation of the paper
2: Weak Reject (Recommend for ICME Workshops)

9. Justification (required if score of 1 or 2 has been selected for questions 3-7):
The work is mostly a combination of already existing deep learning tools. Experimental evaluation is limited even if this is partly justified by the very niche application. The writing is very confusing specially in Section 3.

10. Additional comment to author:
The paper proposes a method for detection and segmentation of compound figures. The paper focuses on a very specific application and adapts standard deep learning models for object detection and image classification to the specific task.

The approach starts from detecting subfigure labels and then uses this information to produce a mask that is applied to the features extracted by the YOLO detector. Results are difficult to evaluate since it is a very niche application with a very few works dealing with it.

Comments:
- The explanation of the method in Section 3 is very confusing, it should be re-written and also an algorithm or pseudo-code synthetizing the procedure would help.
- A “YOLO-style network”, is it YOLO or did you make any change to the approach ?
- The training set looks quite small for the type of employed networks, is there any risk of overfitting ? Showing training and validation curves could help

Reviewer #5

Questions

1. How confident are you in your evaluation of this paper?
3: Very confident

2. Importance/ Relevance
3: Of sufficient interest

3. Novelty/Originality
3: Moderate original

4. Technical Correctness
3: Probably correct

5. Experimental Validation
2: Lacking in some respect

6. Clarity of presentation
2: Difficult to read

7. Reference to prior work
3: Reference adequate

8. Overall evaluation of the paper

2: Weak Reject (Recommend for ICME Workshops)

9. Justification (required if score of 1 or 2 has been selected for questions 3-7):

This paper works on subfigure separation for the scientific papers. In general, the method follows data driven pipeline. In general, the paper writing is rough and typos and expression problems dotted many places. The proposed method is interesting but many details remain unclear because of the low quality writing. The experiments are also not well presented.

10. Additional comment to author:

There are some weaknesses of this paper. (1) Writing. It's rough and unclear. Figures and tables are not well explained. (2) Experiments. The results are basically convincing but in short of some insightful analysis on the obtained results. The comparisons with other methods are also not comprehensive.