Reviewing
Fast science or slow science?
Weekly reading goof stuff or
daily reviewing of rubbish?

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Computers and language

- Computational linguistics: focus on algorithms (mainly machine learning and now deep learning with neural networks), very mathematical

- Natural language processing: focus on language interpretation tasks: parsing, entities in text, time, sentiment, machine translation, summarisation, ....

- Empirical modelling of data

- Evaluation data for specific tasks

- State-of-the-art results on the specific task

- Training data (collections of texts), lexicons, ontologies (resources)
Fast science route
From conference to book

• Conferences
  • A-conferences: ACL, NAACL, EACL, COLING, EMNLP,
  • B-conferences: RANLP, CICLing, LREC, …
  • Acceptance rates: https://aclweb.org/aclwiki/Conference_acceptance_rates
  • Submissions: ACL2017 1,419 papers (829 long and 590 short), from which we sent for review 1,318 papers (751 long and 567 short).
  • Organised in different areas with area chairs and program committees for each area

• Interdisciplinary papers are always a problem: AI-related conferences: International Semantic Web, Extended Semantic Web, Digital humanities

• Hundreds of workshops (30 at LREC2018)…..

• Journals (takes 1, 2 years to be published), Books…. usually editor with separate authors writing chapters
Structure of a paper

- Abstract: clarity (!)
- Introduction: clarity (!), what is the contribution of this paper
- Related work: make sure state-of-the-art is there and make sure it is clear what is different in this paper
- Main approach: originality
- Data: use data that are considered to be standard data sets or motivate otherwise
- Experiment: clarity, soundness
- Result: what is the state-of-the-art, do you beat them or can we learn something
- Discussion and conclusion: what is the contribution
ACL Review form

• **Appropriateness**

• **Clarity**: For the reasonably well-prepared reader, is it clear what was done and why? Is the paper well-written and well-structured?

• **Originality**: How original is the approach? Does this paper break new ground in topic, methodology, or content? How exciting and innovative is the research it describes? Note that a paper could score high for originality even if the results do not show a convincing benefit.

• **Soundness/correctness**: First, is the technical approach sound and well-chosen? Second, can one trust the claims of the paper -- are they supported by proper experiments and are the results of the experiments correctly interpreted?

• **Replicability**: Will members of the ACL community be able to reproduce or verify the results in this paper?

• **Meaningful comparison**: Do the authors make clear where the problems and methods sit with respect to existing literature? Are the references adequate? For empirical papers, are the experimental results meaningfully compared with the best prior approaches?
ACL Review form

- **Substance:** Does this paper have enough substance, or would it benefit from more ideas or results? Note that this question mainly concerns the amount of work; its quality is evaluated in other categories.

- **Impact of ideas or results:** How significant is the work described? If the ideas are novel, will they also be useful or inspirational? Does the paper bring any new insights into the nature of the problem?

- **Impact of accompanying software:** If software was submitted or released along with the paper, what is the expected impact of the software package? Will this software be valuable to others? Does it fill an unmet need? Is it at least sufficient to replicate or better understand the research in the paper?

- **Impact of accompanying data set:** If a dataset was submitted or released along with the paper, what is the expected impact of the dataset? Will this dataset be valuable to others in the form in which it is released? Does it fill an unmet need?

- **Recommendation:** In deciding on your ultimate recommendation, please think over all your scores above. But remember that no paper is perfect, and remember that we want a conference full of interesting, diverse, and timely work. *If a paper has some weaknesses, but you really got a lot out of it, feel free to fight for it.* If a paper is solid but you could live without it, let us know that you're ambivalent. Remember also that the authors have a few weeks to address reviewer comments before the camera-ready deadline.
ACL Review form

**RECOMMENDATION (1-5)**

- 5 = This paper changed my thinking on this topic and I’d fight to get it accepted;
- 4 = I learned a lot from this paper and would like to see it accepted.
- 3 = Borderline: I’m ambivalent about this one.
- 2 = Leaning against: I’d rather not see it in the conference.
- 1 = Poor: I’d fight to have it rejected.

**REVIEWER CONFIDENCE (1-5)**

- 5 = Positive that my evaluation is correct. I read the paper very carefully and am familiar with related work.
- 4 = Quite sure. I tried to check the important points carefully. It’s unlikely, though conceivable, that I missed something that should affect my ratings.
- 3 = Pretty sure, but there’s a chance I missed something. Although I have a good feel for this area in general, I did not carefully check the paper’s details, e.g., the math, experimental design, or novelty.
- 2 = Willing to defend my evaluation, but it is fairly likely that I missed some details, didn’t understand some central points, or can’t be sure about the novelty of the work.
- 1 = Not my area, or paper is very hard to understand. My evaluation is just an educated guess.
F-measure bashing

• What to do if you cannot beat the state-of-the-art?

• Google, Facebook, IBM publish papers but they do not share: code, executables, data and the computing power

• Compare separately against open-source and proprietary results

• What if I still do not beat the state-of-the-art?

• Should not be a problem if your research is new and leads to new conclusions using a sound (replicable) method!!!
Slow science

- What we need is a replication index in addition to a citation index!!!
- Replicate the work of others (state-of-the-art, replicable, transparent)
- Do a proper error analysis; what is the problem space
- Make your research available as you want other to do
- Be patient and persistent:
  - a rejected paper is not a always bad paper (unless it is rejected 5 times)
  - resubmitted paper wins the best paper award in the next A-conference
  - learn from the feedback, improve, continue your work and re-submit