PhD Seminar

Rules for Reviewing Papers

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Simply Answer Five Questions

1. What are the major results?
2. Are they correct?
3. Are they new?
4. Are they clearly presented?
5. Are they worth publishing?

Rules for Refereeing Papers

1. Authors never make mistakes, they intentionally do stupid things
2. If you don't understand something, it's wrong
3. If you do understand something, it's too simple
4. If it does not cite all of your papers, it's under-referenced (reviewers in the "century club" with more than 100 papers can use the 10% rule)
5. If the paper presents new algorithms, it doesn't contain enough empirical work

More Rules …

6. If the paper has a case study, it's not controlled well enough
7. If the paper has a well controlled experiment, it will not apply to the real world
8. If the paper is empirical, it doesn't make enough theoretical contribution
9. If limitations are discussed, criticize the work for being too limited
10. If limitations are not discussed, criticize the paper for being dishonest

The Rest of the Rules

11. Always give at least one irrelevant, inaccessible reference to include. Make it a mandatory change. If the reference is in an unusual language (Portuguese is good), that is a bonus.
12. If you reject the paper, be sure to recommend that it be sent to another journal that you review for. If you are feeling especially energetic, send a note to the editor of the other journal, asking to review the paper.

Five Reasons to Reject a Paper

1. Is the author somebody you hate?
2. Does the paper contradict, supersede, or precede any of your results?
3. Will the author be competing with you for your next grant?
4. Is the author's advisor one of your enemies?
5. Is the paper too original and creative?

Stupid Referee Tricks

- "The presentation needs considerable improvement." – TAV 89
- "A study like this should have been published in about 1980" – TAV 89
- "Better than average American academic paper, below the standard of papers written by European (non-English) academics" – FTCS 1990
- "As usual, Offutt got it wrong" – 1993
- "Pretty good paper" – the reviewer voted "reject"

More Stupid Referee Tricks

- "There is a pretty good chance that the research you want to do is worthwhile, but you really shot yourself in the foot with this proposal" – NSF, 1991
- "Good paper, lots of small mistakes" – voted to accept
- From editor: “We are sorry to say your paper has been REJECTED”

Okay … A Serious Version

- A paper should be accepted or rejected based on its key results, **not** its presentation
- Reviewers must be objective – personal factors should not effect the review
- If you cannot be objective, you have a bias and should not review the paper
  - Recent collaboration
  - Recently at the same institution
  - Advisor / student relationship (ever!)
- You may not use results in paper until it's published
- Authors work hard and deserve your respect

Categorizing Problems

- **Technical Problems**
  - Minor : Mistakes in background, related work
  - Moderate : Does not effect the key results
  - Major : Changes the key results
  - Critical : Negates the key results
- **Presentation Problems**
  - Minor : Typos, spelling, grammar
  - Moderate : Make understanding the paper harder (organization, notation, repeated grammar)
  - Major : Prevent understanding of part of the paper
  - Critical : Prevent understanding or evaluating a key result

Categorizing Omissions

- **Problems of Omission**
  - Minor : Omitted background, related work
  - Moderate : Not part of the key results
  - Major : Missing in the key results (proof or experiment, lack of control in experiment)
  - Critical : Must be in the paper to evaluate the result (experimental study, etc)

    or not enough results

Recommendation

- Accept : Publish with no changes
- Minor revision : Reviewer will not review the changes
- Major revision : Reviewer will review the changes, may recommend reject
- Reject : Do not publish
  - Reject and resubmit : Write a new paper on the same topic (same problem and solution)

Principles of Making Recommendations

- A paper should be rejected on technical grounds, including if the presentation makes the results inaccessible
- If the change may not be enough after re-review, the authors deserve to know
- There is no major revision for a conference
- Going "back to the lab" is always a major change
- If the authors have not done enough, let them decide whether to do more or not
- You might be wrong

Making a Recommendation

	Technical	Presentation	Omission
Reject	Critical Major (1)	Critical	
Major Revision (3)	Major Moderate (2)	Major	Critical Major
Minor Revision	Moderate Minor	Moderate	Moderate Minor
Accept		Minor	Minor

(1) If you believe the change would still not be enough – reject and resubmit might be appropriate

(2) If you don't trust the editor to check the changes

(3) In a conference, "major revision" == "reject"

Rookie Reviewing Mistakes

- Being too critical
  - Look for the good, not just the bad
- Not being critical enough
  - Be confident – even great scientists write bad papers
- Being too defensive of your own work
  - Sometimes authors should discuss your papers
- Expecting perfection from experiments
  - Results are never conclusive
  - Are limitations identified?
  - Does the paper make a contribution?

Rookie Reviewing Mistakes (2)

- Asking for too much
  - Sometimes redoing an experiment is too expensive
  - Some really novel ideas need to be validated in a separate paper
- Some obvious results are publishable
  - What is obvious to you may not be obvious to me
  - Sometimes "what everybody knows" is wrong
  - One of my early papers:

    reviewer 1: everybody knows this, so reject

    reviewer 2: everybody knows this is wrong, paper must be wrong