Week 2B: Research 101

Reviewing Research

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Slides adapted from Matthew Hicks’ “Research 101” lectures
Reminders

Sign up for paper presentations by **Tuesday, September 6th**

Signup sheet here: [https://docs.google.com/spreadsheets/d/1dtoXZnEM36yaQISgo--qPeEuTzI5jsNyFSkKKePDgsk/edit?usp=sharing](https://docs.google.com/spreadsheets/d/1dtoXZnEM36yaQISgo--qPeEuTzI5jsNyFSkKKePDgsk/edit?usp=sharing).

**Audience:** you are not required to read the paper.

- You *are* required to participate in the class discussion of the paper.

**Presenters:** your job is to *teach* us the paper.

- Contextualize, pros and cons, main contributions, summarizing, identifying key assumptions.
- Prepare a short slide deck (you are free get "inspired" from existing presentations).
- Deliver a 10 – 20 minute presentation (with a 10 minute audience discussion to follow).

**Course Syllabus & Schedule:** [https://www.cs.utah.edu/~snagy/courses/cs5963/](https://www.cs.utah.edu/~snagy/courses/cs5963/).

Questions? Contact me at snagy@cs.utah.edu.
Questions?
Today

- 20 minutes – **Research 101: Reviewing**

- Rest of class – **Guest lecture by Chad Brubaker**
  - Tech Lead Manager on Android Security at Google
  - Co-lead for Security/Privacy on Exposure Notifications
  - @chadbrubaker__
Reviewing
Why review research?

Reviewers are the essential gatekeepers that make our research system work.
Why review research?

- Accept everything
  - No way to keep up
  - Each reader must gauge what value is
  - Risk (more) flawed results
  - All work stays at local maximum
  - How do we identify/reward/encourage the best?

- Accept nothing
  - Science stalls
Why am I covering this here?
Reviewing is a process

1. Does the problem matter?

2. Are the claimed contributions enough?

3. Are the contributions supported?
   a. Design
   b. Evaluation
What makes a problem important?

- Timely
  - E.g., Meltdown and Spectre
  - In-browser crypto mining malware

- Obvious next step

- Evaluates a common assumption

- Must be surprising in some way

- Opens a new and realistic line of research
Are the claimed contributions enough?

- Are they new?
- Do the contributions push the area forward?
- Do they open a new area of investigation?
Does the system support the contributions?

- **Watch out for a design bait-and-switch**
  - Intro mentions X, but the authors implement Y
  - But Y != X in meaningful ways

- **Evaluation funny business**
  - Do the authors evaluate what they design?
  - They fabricate a chip, but use simulations for experiments

- **Is the evaluation fair?**
  - We use these benchmarks [which behave in a way that suits our system]
  - We allocate 1 GB of memory [our competitor is memory limited]
  - Watch out for how randomness can influence results
Fixable, but grave sins

- Identify stated and **unstated** assumptions
  - This can break a paper or be easily fixable

- Are all assertions made in the paper supported?
  - Prevent future papers from citing an unsupported statement in this paper
  - “The Dobber method is superior to the Fastly method.”
What goes into a review

- Summary
- Venue-dependent scores
- Pros
- Cons
- Detailed feedback to authors
What goes into a review

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- Venue-dependent scores
- Pros
- Cons
- Detailed feedback to authors

Write a good summary... you will need it!
What goes into a review

- **Reject**: just stop
- **Weak reject**: the paper isn’t not ready yet
- **Weak accept**: it’s ready but not compelling
- **Accept**: will argue for

**Venue-dependent scores**

- **Summary**
- **Pros**
- **Cons**
- **Detailed feedback to authors**
“The bar” is conference-dependent

7. Overall, how good is it? What do you recommend?

   Can you put the paper into one of these categories?

1. Major results - very significant. (fewer than 1% of all papers written.)

2. Good, solid, interesting work; a definite contribution. (fewer than 10% of the papers you will see.)

3. Minor, but positive, contribution to knowledge. (perhaps 10% to 30% of the papers submitted.)

4. Elegant and technically correct but useless. This category includes sophisticated analyses of flying pigs, as mentioned above.

5. Neither elegant nor useful, but not actually wrong.

6. Wrong and misleading.

7. The paper is so badly written that a technical evaluation is impossible.

Source: The Task of the Referee, A. J. Smith
What goes into a review

- **Summary**
- **Venue-dependent scores**
- **Pros**
- **Cons**
- **Detailed feedback to authors**

**Main points** that other PC members will read
What goes into a review

Summary
Venue-dependent scores
Pros
Cons
Detailed feedback to authors

Be helpful, but don’t go out of your way for blatantly unpublishable papers
Things good reviewers do

- Be constructive, concrete, and courteous
- Spend time with borderline papers
- Help authors improve their paper
  - “Use this tool”
  - “Evaluate your system this way”
  - “Pitch your contribution this way”
  - “You can fix your system by doing…”
  - “Cover this related work [1], it relates to your paper this way”
- Don’t just say something exists... point to it!
Things bad reviewers say

- “This is the worst paper I’ve read”
- “I can do it better, so let’s reject it”
- “Here is every single grammar error”
- “They didn’t work on the ‘right’ problem”
- “I don’t like this inconsequential low-level detail”
- “Here’s a review I wrote for a previous version of the paper”
- “They didn’t cite these non-peer-reviewed works” (e.g., arXiv)
- “It’s too similar to these other works that I didn’t actually read”
Conference reviewing cycle

- Authors Submit
- 2–3 months
  - Reviews from PC
  - PC Discussion
  - Author Rebuttal
  - PC Discussion
  - Author Revision (if requested)
- 1–3 weeks
  - PC Discussion
- 1–2 months
  - PC Discussion
  - Revision Decision
  - Final Decision

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A final note...

It is easier to be a detractor than to be a champion; be a champion!
Questions?