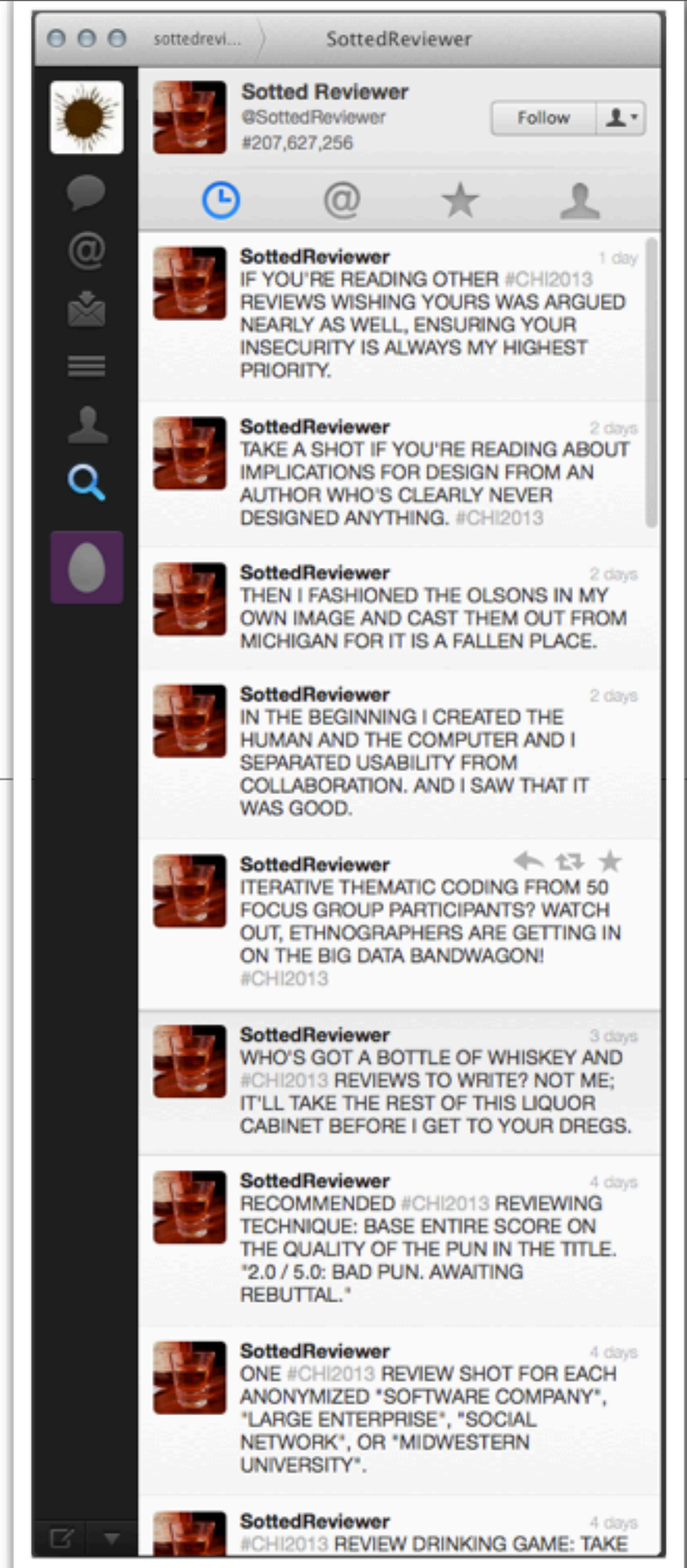
# Reviewing in HCI

Strongly inspired by Michael McGuffin
http://profs.etsmtl.ca/mmcguffin/



# What is reviewing ? How does it work ?

In HCI conferences:

- .: authors submit papers
- .: program chair(s) distribute them to PC
- .: program committee / meta-reviewers choose reviewers
- .: reviewers are
  - .: invited or from pool of volunteers
  - .: can be experts, students of supervisors,
  - .: people known to *not* be experts
- .: authors have one chance to get paper accepted.

# What is reviewing ? How does it work ?

In journals:

.: Authors submit papers

.: Editor and/or assistant editor(s) choose reviewers

.: 2/3 rounds of reviewing.

In both:

.: Review process is confidential, blind or double-blind.

.: Review usually consists of numerical score
+ recommendation to accept or not, along with comments and constructive criticisms.

# Ethics

Reviewing is serious business:

.: It can affect careers of others,

.: It can affect the reputations of conferences and journals

Ethical guidelines:

.: avoid conflict of interest,
even *appearance* of conflict of interest,

.: both for your own reputation and the one of the conference

Rule of thumb:
*if you wonder whether there is a conflict, there is one*.

# Why should you bother doing reviews ?

- Can help with your own research
  - gives you new ideas for research projects / directions
    - but don't steal ideas, or use them before they're published
  - introduces you to new background literature
  - shows you new methods
  - can motivate you to try harder or get out of a slump out of sheer competitiveness can inspire you / introduce you to new areas of research, help you put together new course material, etc.
- forces you to keep *reading* papers on a regular basis
- helps you become a better writer
  - see, first hand, examples of what makes a paper good/bad, and what reviewers like/dislike
  - (publishing your own papers also helps here)
- helps you establish & maintain reputation as expert in your field(s)
  - (publishing your own papers also helps here)
- get early scoop on new research in your field(s)
- influence content and quality of research in your field(s) and in conference(s)/ journal(s) you care about
  - (publishing your own papers also helps here)
  - reviews can contain arguments for the type of paper that should be accepted
- looks great on CV (e.g. under "community service"); shows you're an active and acknowledged member of your community
- get you name in conference proceedings
- can lead to more "community service" later on, e.g. being part of PC, or chair, editor, ...
- relatively small time investment (possibly 1-10 hours per paper, only a few times per year)

# Process

.: Print out paper (right away)

.: Read it with pen and highlighter

.: note down ideas for anomalies, holes, weaknesses, strengths

.: circle / underline any mistakes (punctuation, spelling, ...)

.: check off references as they are cited

.: look at what is referenced

.: can you think of anything else that should be referenced ?

.: check the math, arithmetic, logical structure of arguments

.: mark anything of interest to you, but not for the review

# Novelty

.: if the paper is very much in your area, do a detailed review, and try to check the cited and un-cited literature as thoroughly as possible.

.: if the paper is very much not in your area, don't worry too much about looking at cited work, and in your review confess that you didn't read any of the cited work and are just assuming that the work is novel

.: when in doubt, assume authors are honest, and make your assumption explicit

# Guidelines

1. Identify main claims / contributions
   - .: if the lit review is very good, this may be a secondary contribution in itself
   - .: are the ideas novel ?  useful ?  incremental ?
   - .: does the author do a good job contrasting and differentiating their stuff from previous work (including non-academic previous work) ?
2. Identify main weaknesses
   - .: are the results of the experiment unsurprising given previous work ?
3. On the basis of 1 and 2, base your overall recommendation
   - .: briefly explain your decision
   - .: try not to give a borderline grade (i.e. 3/5)

# Pitfalls of reviewing

- Be sensitive to the fact that authors may have already considered ideas and possibilities that you suggest, but may have chosen to cut stuff due to page limits.
- If you think it's more important they include / beef up other stuff, suggest what to cut or shorten.
- Don't be vague.
  Examples of vague comments from reviewers:
  - "there's plenty of previous work ...", authors will want concrete refs (even URLs, refs to magazines, or company/product names are good)
  - "there are lots of mistakes; I don't have time to point them all out; I'll just point out a few and hope the other reviewers do a complementary job"

# Secondary review elements

- .: design flaws in experiment
- .: mistakes in math
- .: mistakes or holes in reasoning / logic
- .: possibilities for explanations of effects, and designs, overlooked
- .: use of incorrect / non-standard notation
- .: incorrect or non-standard terminology
- .: if your review is not long by now, also identify typos (unless there are *lots*).
  - .: If the authors are obviously non-native English speakers, just point out the few flaws in composition that would throw off a native speaker.
  - .: If there are lots of spelling mistakes, just say "do a spell check"

# Feelings

# DUB For the Future

thoughts on human-computer interaction, user interfaces, design

SATURDAY, NOVEMBER 07, 2009

## I give up on CHI/UIST

The CHI reviews just came out and I have to say I'm pretty unhappy... not with the **numbers** per se... (one paper I co-authored has a 4.5 average out of 5 and I'm sure I'll get a fair number of papers accepted), but instead with the attitude in the reviews. The reviewers simply do not value the difficulty of building real systems and how hard controlled studies are to run on *real* systems for *real* tasks. This is in contrast with how easy it is to build new interaction techniques and then to run tight, controlled studies on these new techniques with small, artificial tasks (don't tell me this is not true as I have done it and published good papers of this style also).

I really am ready to give up on CHI / UIST and go elsewhere (to another existing community or create a new one -- UISTSys anyone?). I've talked about this for 3-5 years with many of you, but I think I've finally had it as there has really been no change. In fact, I think it has gotten worse. The highest ranked paper we wrote took 6-10 weeks of work and is well written, interesting to read, and synthesizes many studies in multiple communities. It is valuable to the CHI community, but it invents nothing new. I'd love to see it published at CHI and I think there should be room for multiple kinds of work at CHI (including nice surveys, opinion pieces, interaction techniques, fieldwork, and systems work).

The papers we have submitted with truly new ideas and techniques, and years of work behind them, get reviews asking you to do 2-4 years more work. For example, they ask you to create a completely different system by another team with no knowledge of your ideas and run an A vs. B test (because that commercial system you compared to had different goals in mind). Oh, and 8-10 participants doing 3-4 hour sessions/participant isn't enough for an evaluation. You need lots more... They go on and on like this. Essentially setting you up for a level of rigor that is almost impossible to meet in the career of a graduate student.

ABOUT ME

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.: The **pros and cons you identify are more valuable to the meta-reviewer than your overall grade**, so be explicit about all your significant observations, and don't agonize over how to weight things and over your final decision.

.: Don't be condescending

keep in mind that the authors worked hard, if you can recommend what they could do to beef up their paper and a venue to resubmit to, that would be very nice;

.: Be constructive and diplomatic.

.: Criticize the paper, not the authors.

# Epistemologies at CHI

# Sources

.: Guide to Reviewing CHI Papers and Notes
http://chi2013.acm.org/authors/guides/guide-to-reviewing-chi-papers-and-notes/

.: Prof. Kevin McGee on how to evaluate an HCI paper.

.: Prof. Michael McGuffin's on how to review a paper.
http://profs.etsmtl.ca/mmcguffin/learn/academicResearch/tipsForReviewing

.: The CHI 2005 Review Guide.
http://www.chi2005.org/cfp/reviewing.html

.: 1,214 CHI submissions were rejected this year. Why?
http://oulasvirta.posterous.com/86113982

.: A Position on Peer Reviewing in HCI
https://interactionculture.wordpress.com/2012/01/27/a-position-on-peer-reviewing-in-hci-part-1/

# 8 Fundamental Questions to ask as a reviewer

1. What is the Problem or Question the author is attempting to solve or answer?
2. Is the main Problem or Question important enough to warrant study?
3. What is the author's contribution to knowledge about solving the Problem or answering the Question? Important: a "*contribution*" is a contribution to knowledge, so evaluators need to ask "What *do readers learn from reading this paper*?"
4. Is the contribution important or significant ?
    - Is the contribution generally relevant? Does it impact more than just a few people (who is interested in the contribution?). And: does it answer or solve more than just a few specific cases of the overall question or problem?
    - Is the contribution an important advance over what was known before? (how much do they care about it?
5. Is the contribution original? An original contribution to knowledge means: readers of the paper will learn something that they cannot learn anywhere else.

# 8 Fundamental Questions to ask as a reviewer

6. How do readers know that it is original? It is not enough for a reader to believe (or "know") that the work is original, the author must clearly identify "related work" and indicate what makes his contribution different.
   WARNING! "Related work" is work that has tried to either a) answer the same (or similar) question, or b) solve the same (or similar) problem.
7. Can readers trust the validity of the contribution?
   - Does the author motivate and document an appropriate method for arriving at results? (This is what the author did: that is, how the author attempted to solve a particular problem or answer a question – and why the author chose the particular method(s) used.)
   - Do the results seem believable, significant, relevant, and well-documented? (This is what happened as a result of following the particular method(s).)
   - Does the author do an appropriate analysis of those results? (This is the author's reasoning about what the results mean.
8. Is the contribution appropriate for a specific discipline (or conference or journal)?

# Summarizing the contribution

.: The review form typically asks you to state, in two or three sentences, what **contribution** the paper aims to make **to the field** of Human-Computer Interaction.

.: The contribution statement is a **short summary** of answers to the **significance**, **originality**, and **validity** of the contribution to knowledge of the paper.

.: In addition, it will be useful to provide the **context** to the contribution by stating **what is the problem or question** the author is attempting to solve or answer.

.: The summary of contribution is not easy to write, since it needs to cover all the above elements while being as brief as possible.

.: If a paper has originality and validity problems, we will state it briefly in the summary as well.

# Summarizing the contribution