How to write a review

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Outline

- What is a review?
- Why should you review?
- How do you review a paper?
- What *not* to do?
- What are the dilemmas?
- Case study

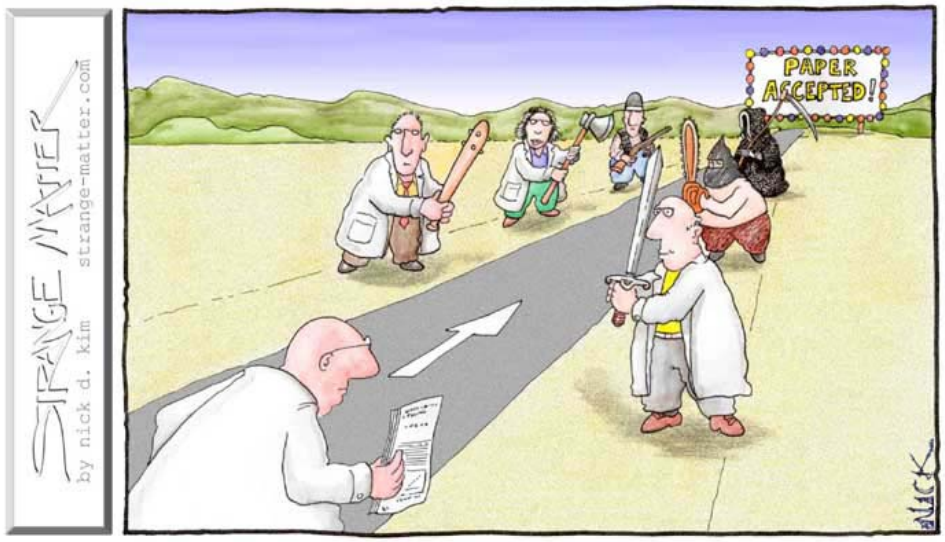
What is a review?

Prof. Alan Bundy

- "Something that will ruin your day" *Alan Bundy*
  - Even if it is good!
- The stamp of scientific quality
- Feedback from your peers
  - Future directions?

What is a review not?

- Acceptance/rejection
  - Editors/Program committees accept/reject
  - You recommend!
- A place for bias, prejudice, personal animosity, …
  - Though it often appears to be so



Most scientists regarded the new streamlined peer-review process as 'quite an improvement.'

Why should you review?

- You'd much rather enjoy Paphos
  - And so would I!
- But science would grind to a halt
  - Not immediately, of course

Reasons to review

- Duty
- Fairness
  - 2-3 reviews written/ paper written
- Promotion
- Education
  - Good reviewers write good papers?

Bad reasons to review

- To settle old scores
- To advance your own theories/hinder rivals
- To get latest results
  - Unpublished papers are strictly ***confidential***

How do you review?

- Read the paper
- Read the review form
  - Useful dimensions to look at
  - Novelty, Clarity, Importance, Timeliness …
- Read the paper
- Wait a few days
- Read paper
- Write review

Everyone has their own method

How do you review?

- Put yourself in author's shoes
  - Think how you would like to read this review
- Offer ***constructive*** criticism
  - Don't just tell them something is inadequate!
  - Tell them how they might fix it

What not to do?

- Miss the deadline
  - We all hate late reviews
- Display partiality, bias, animosity, …
- Destructively criticize
  - Always work out what they would need to do to fix problems

Collect and share reviews

- Learn if others agree with your opinions
- Thicken your skin

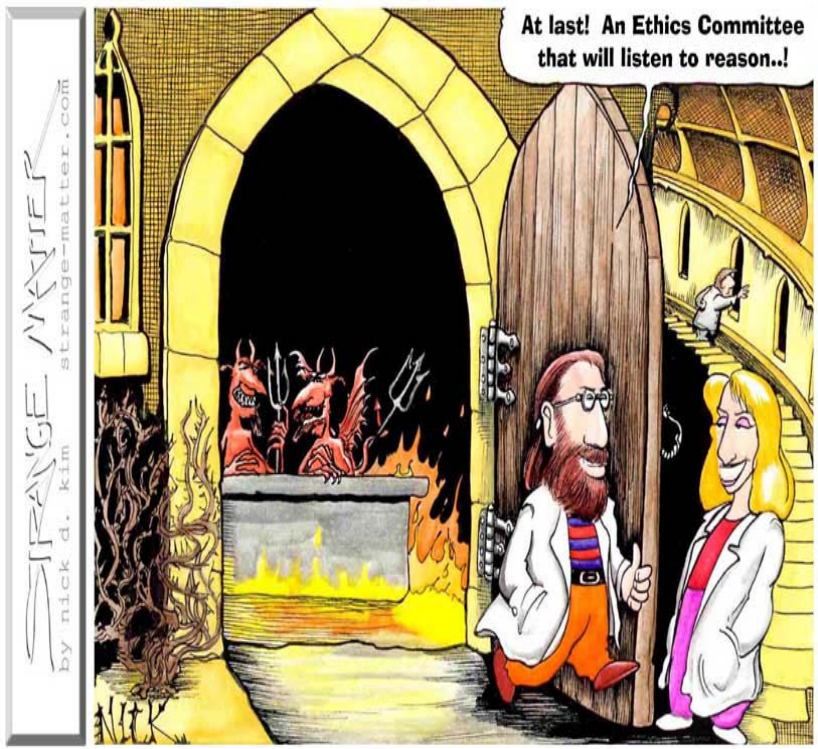
Clearly, the author fails to understand the work of Walsh in this area …

Since they mention no related work, this paper cannot be original …

This idea is too simple not to exist already …

This work is good but I don't understand why Bundy hasn't done this already?

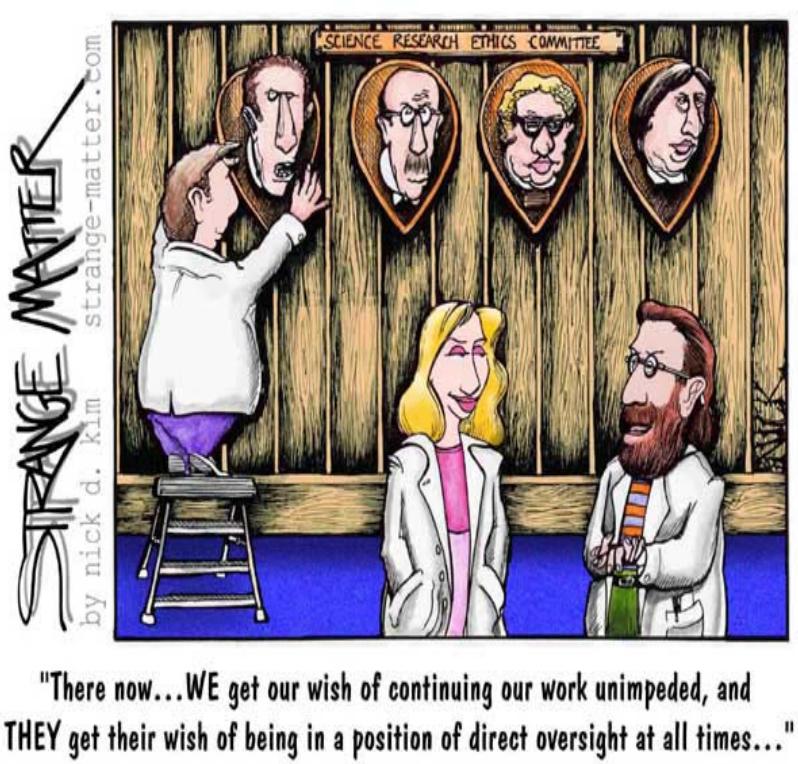
Ethical dilemmas



- You are working on the ***same*** problem
  - Talk to Editor/Program Chair
- You already reviewed and rejected paper
  - Look for changes

Ethical dilemmas

- This journal submission already appeared at a conference
  - Does it extend previous appearance?
- An almost identical paper already appeared
  - Unless it was at a workshop, inform Editor/Chair



Case study

- "Stochastic Constraint Programming"
  - By Toby Walsh
  - Be frank, the feedback is good!
- What do you think?

What did reviewer 1 think?

- Appears to like it
- Main criticisms:
  - Relationship to influence diagrams
  - Algorithm performance
  - Phase transition too preliminary

"The paper reads well. … I have a number of remarks though. First, from a probabilistic reasoning viewpoint, I wonder about the relationship between this framework and influence diagrams (or decision diagrams). It appears to me that what you have defined here is very closely related …Second, from a constraint satisfaction viewpoint: you gave us no indication of how well the different algorithms you presented work in practice. … Third, I think the discussion on phase transition cannot be left at this level. It is not surprising that we have a phase transition here, but what is interesting is the nature of this transition… I think this topic is too serious to sum it up in a small Section … [it] deserves a dedicated and more thorough treatment. I would have preferred to see this space dedicated to experimental results on the performance of presented algorithms"

What did reviewer 2 think?

Total of their written comments:

"It would be nice to include the exact syntax of one SCprogram, as accepted by your system (?), say, for the example of Section 3."

- Appears to like it
  - Very relevant
  - Moderately significant/original
  - Good readability/English
- Minimal comments

What did reviewer 3 think?

- Again appears to like it
  - Very relevant, very original, moderately significant
- Main criticisms
  - Relationship to influence diagrams
  - Phase transition too preliminary

"Clearly, one could just add constraints to influence diagram representation and extend algorithms to exploit them (my preferred approach) but the approach here is still very valid and could motivate researchers in MDPs and influence diagrams to treat constraints as special creatures … so that their special … algorithms can be exploited.

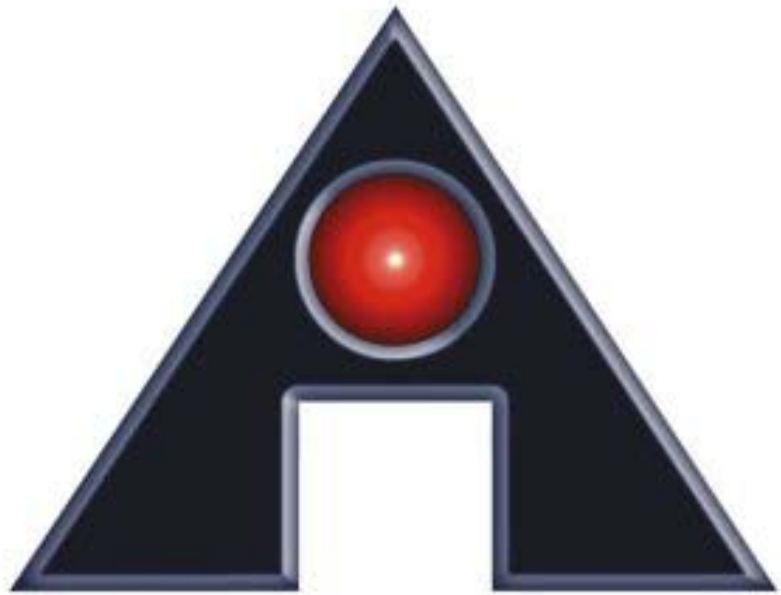
I think the experimental portion of the paper should have been to compare the performance of the algorithms with the performance of traditional MDPs or influence diagram algorithms applied to this class of problems and I speculate that gain can be shown.

I don't find the phase transition experiments of much value at this stage. So, there may be a phase transition, so what?

I recommend that the author will carefully analyze their model against standard influnce diagrams or factored MDPs and discuss the pros and cons."

What did the IJCAI PC think?

- Paper was ***rejected***
  - Along with 75% of the other submissions
  - A less good paper (my and reviewers' opinions) was accepted!
- Some compensation
  - $150,000 to be precise



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Conclusions

- Reviewing can be rewarding
  - Both to authors and to reviewers
- Be constructive
  - Think how you would react to the review
- Take on board your reviews
  - Reviewers hate most being ignored!