Dave Randall: Reviewing for CSCW

The points made below reflect many years' experience of reviewing, meta-reviewing and receiving reviews. They are not meant to 'instruct' but to remind you of some key issues that exercise members of the community all too often.

1. Oddly, less experienced members of the community tend to be much stricter in their scoring of papers than the more experienced. The very best papers, in my view, show a combination of rigor and imagination. Put simply, you want to read papers that can substantiate the claims they make to a reasonably robust standard, and make claims that are interesting enough that you think the community will read (and maybe cite) the paper in time. You should state what you think the main points of the paper are in your review and then whether you think they are adequately substantiated (i.e. strengths and weaknesses).
2. Nothing enrages authors more than people who review papers with little or no understanding of their content. You have nothing to prove as a reviewer and if there are things you don't understand it is OK (actually, good) to say so. If the contents of the paper are obscure to you in general, there is nothing wrong with saying that you do not want to/ cannot review it. Don't pretend that you know what you are talking about if you don't. At the same time, be confident in your judgements- if you have any doubts, let me know and I will offer an opinion.
3. Papers should be judged by standards set from within the perspective/ methodological framework they claim to be part of. It is wrong to criticize a 'quant' paper on the grounds that it doesn't have qualitative elements, or vice- versa. It is wrong to criticise a social psychological paper on the grounds that it is not ethnomethodology, and so on. Disagreement in and of itself is not grounds for rejection. Try to state the criteria on which you base your opinion.
4. Papers generally start out what some statement of what their 'research question' is. This is not much more than stating what the paper is about and shouldn't be confused with a hypothesis to be tested. Be absolutely sure that you have the methodological sophistication that is necessary if you want to be critical of a stance taken by authors. There are many views of, to take one example, grounded theory, and it is wrong to advance a specific view without being aware of other opinion. Too many reviewers base their methodological claims on 'textbook' versions' of method and show little nuance in their judgments. Incidentally, many of my well-known colleagues tell stories of how reviewers criticized them for not citing their own work. It is utterly galling to be told, if you are 'Randall' that you should have read 'Randall'.
5. A conference paper cannot be entirely comprehensive in its literature review. Try to judge the adequacy of the literature review on the basis of whether it covers the main conceptual/ evidential themes, or acknowledged main contributions to the field, and not on whether every single paper written on a theme is cited. Having said that, one would expect authors to show awareness of papers already published in CSCW, ECSCW, JCSCW, GROUP, COOP, CHI, DIS and so on and reviewers are often very critical of papers which ignore that literature. Again, though, some of the best papers

also demonstrate an awareness of a literature outside of these communities, making points of comparison that we might otherwise not recognise.

6. There is a tendency on everyone's part to sit on the fence. '3' is the least useful score for meta- reviewers to deal with and it helps a great deal if you take a side, even if it is only slightly up or down.
7. The CSCW conference has a fairly unique approach to paper acceptance/ rejection insofar as it is a two- stage process. Experienced reviewers aim towards 'helpful' reviews in that they aim to make judgments about whether the paper has sufficient promise and not whether it is ready 'right now'. In other words, might it make a decent piece after your and other concerns are addressed? I've seen many examples of papers which eventually turn out to be of very high quality when authors are given the chance to improve their work. The more specific your recommendations, the better.
8. Very short reviews (3 lines or so) are insulting, even when they are positive. Aim for a minimum of four or five paragraphs. Having said that, don't be verbose or pompous. As I've already said, the best reviews lay out clear criteria for their scores.
9. Papers, in the end, should be judged on their 'contribution'. Of course, this can mean many things. In my opinion, papers should not be rejected because they show negative results- that's good science. They should not be dismissed if they replicate existing work- the standard should be whether we are sufficiently confident about whether existing work is robust enough that it doesn't need replication. If they show positive results, so much the better, but measure the contribution against claims made. Nothing has been 'proved' if a study of eight undergraduates shows a marginally significant result and good papers show an awareness of their limitations. If the paper makes conceptual claims, judge it on whether the conceptual development adds clarity, aids thinking about a topic, or shows enough originality that it changes the way you think about an issue. Neologisms are worthless.

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