

So You're a Program Committee Member Now: On Excellence in Reviews and Meta-Reviews and Championing Submitted Work That Has Merit

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ABSTRACT

This document discusses what goes into being a great reviewer, meta-reviewer, and all-around champion for the papers entrusted to you as a program committee member. Although written with the MobileHCI conference in mind—a “smaller venue” focused on mobility rather than a large “flagship conference” like CHI—much of the discussion applies to program committee work, reviewing, and even authoring more persuasive papers in general. It also touches on some of the responsibilities, pitfalls, and true joys of ushering a paper into the scientific canon of Human-Computer Interaction.

Excellence in program committee work requires a certain attitude: an ability and desire and *by-golly stubborn willingness* to make yourself see hidden gems, and new facets of contribution, in papers that occasionally have rough edges—yet valid and important contributions to offer the field nonetheless. You must embrace the belief that the chief responsibility of a program committee member is to *accept papers*, and not to find flaws and reject work at every opportunity.

WELCOME TO THE COMMITTEE!

You're a program committee member now. Congratulations. You've earned it, and make no mistake: it's an honor to have you aboard. But with this role comes great responsibility.

Attitude

The first thing to address—and perhaps the most important characteristic of a successful program committee member—is the small matter of *attitude*.

Are you a Paper Champion, or do you seek out flaws? Even worse, are you one of those Paper Killers, a roving mercenary capable of dispatching a submission with a few choice words? I would certainly hope not.

Keep in mind that it's easy to point out flaws and reject imperfect work—and that every paper has imperfections. But

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it poses a formidable intellectual challenge to perceive the veiled merits of a paper, and then respectfully direct the authors on how they might raise the level of their discourse to leap above the all-important bar of acceptance, or even that of excellence. And it takes plenty of diligent and dedicated work as well, not something to be undertaken lightly or in a last-second rush to meet the reviewing deadline.

The success of a conference and the future growth of the field—not to mention the careers of those who labor to expand its confines—depend entirely on YOU and the quality of the work you undertake, whether your role is that of a chair, committee member, or external reviewer.

Hidden Implications of Reviewing and of Being Reviewed

We all have done reviews of scientific papers, and possibly a great many of them. We might look upon these unseemly requests in our inboxes with dread—and perhaps doubly so when we are on the receiving end, and have to endure a horrorshow of less-than-glowing reviews.

It happens to us all.

Some shrug it off, suggesting that “growing a thicker skin” is some necessary rite of passage for graduate students, as if our lack of an impenetrable hide were somehow a failing of human evolution.

But from the experience of having reviewed manuscripts probably numbering now in the thousands, and having had my own work rejected a seemingly equal number of times, here's what I've arrived at as the ugly truth:

Reviews—especially hastily written and overly-critical hatchet jobs—often do far more harm than good.

Why do I say this? Well, presentation matters in scientific papers: the quality of the figures, the organization and flow of the text, how well the paper lives up to the promises it makes. If those are even a little bit off, papers tend to fare poorly in reviews. Yet reviewers often simply have the sense that the paper was not entirely satisfactory—that something was off—and then search for reasons to explain why.

Even a largely content-free *positive* review can be harmful, because it fails to deliver strong reasons for acceptance that can sway the program committee's discussion in favor of the

paper; and furthermore such a review robs the author of an opportunity to improve the paper prior to publication.

So unless the author is lucky and an experienced reviewer understands the topic well enough to spot where a paper's presentation may have gone off the rails, it's entirely possible—likely even—that the reviewer will offer up dubious advice. This also means that papers may sometimes be rejected for reasons that are largely confabulations of a flaw-seeking reviewer who simply didn't find the paper to be that interesting, or sufficiently well-written, or close enough to their familiar topics of study. A poor review can even stem from a reviewer's distaste for certain types of contributions—or from the author's failure to frame such contributions as persuasively as possible.

Now don't get me wrong—reviewers regularly apply keen insight to identify real and significant problems—but often it's a simple matter of how a result is framed and justified and discussed, rather than the dreaded “fatal flaw” that ostensibly makes work unpublishable.

The reaction to this negative feedback loop of reviewing and being reviewed is predictable. Researchers howl in protest, and then get discouraged, and then spend inordinate amounts of time trying to write “perfect” papers that are “bulletproof” and otherwise well-fortified against the inexhaustible army of critique wielded by reviewers.

It's critical to resist this sentiment, both as an author and especially as a reviewer or program committee member with the potential to reinforce it even further. Otherwise we collectively risk limiting ourselves to narrower and safer subjects, the field to far less ambitious papers.

By the same token, a paper doesn't necessarily have to be “ambitious” to be important and publishable. You never know the impact of a project at the outset. Who would have thought that hot-gluing an accelerometer to a handheld device would lead to the single highest-cited paper of my research career? As researchers we simply have to pick projects we believe in, large or small, and follow them down the rabbit hole. And sometimes, as reviewers, we have to trust that exploring rabbit holes of uncertain value is in itself well worth the price of admission.

An Opportunity

Every time you receive an invitation to a program committee, journal editorial board, or even a request to review, think of it as an opportunity. To discover interesting new work. To help usher new ideas into the literature. And to instigate a positive feedback loop of constructive commentary on the work that you read.

As a program committee member assigned to a paper, you wield particularly great power over another researcher's hard work. Careers, tenure cases, funding, and the positive mentorship of students ride on your carefully considered comments and decisions. We are empowered to discover

interesting work and bring it to the attention of researchers and practitioners worldwide.

Years down the road, a future generation of researchers may look back on a paper you helped guide to successful publication and say:

Yes, here it is, the first place that an intriguing new idea made its foray into the wilds of human knowledge.

Some ideas may be provocative, and spark whole new directions, even though they have some flaws and limitations and don't really pan out in their preliminary form. Others will wither and disappear from attention, perhaps awaiting the day when a topic returns to the fore, perhaps languishing forever in the many volumes of forgotten knowledge that humanity ceaselessly accumulates.

It is important to realize that any and all of these outcomes are okay, even desired.

A Mantra for Papers on the Edge

The great thing about papers near the leading edge of innovation is that not everyone will agree they have merit.

To break through this we must understand that there's tremendous value in shepherding interesting new perspectives of uncertain value, stolid new building blocks of foundational knowledge, and provocations to accepted practice into the literature. Often a paper's real value is to spark discussion and further inquiry into what might be a promising new direction.

But remember that rabbit hole: it's hard to tell at the outset. Or even when the paper is submitted for review.

The literature, the collective manifestation of many incredibly talented colleagues great life's work, has a remarkable way of sorting it all out. The literature bubbles up the very best of everything we've ever done—perhaps even in ways that we never anticipated or imagined possible—given enough time.

Which leads us to our mantra for papers on the edge:

When in doubt, trust the literature to sort it out.

But in order to take advantage of this seemingly miraculous ability of the literature sort it all out, we have to publish the papers first. Which brings us to the topic of *The Good Review*.

The Good Review

The Good Review should have a number of paragraphs (or perhaps a couple or even several pages of well-considered commentary if warranted). A sentence or two will not cut it, even if only to say “This paper is great!” Why is it great? On what grounds should it be accepted? What are the contributions it makes and why do you see them as noteworthy, or even important? I always worry when I receive a good review of this sort on one of my submitted papers, because it is content-free, because it carries no

weight. I know it will be disregarded because it lacks any convincing reasons to accept the paper.

If you get back a content-free review of that sort on one of your assigned papers, either positive or negative, your instructions are simple:

*You can and you must demand better of the reviewer.
Even (and especially) if said reviewer is yourself.*

Sometimes a terse review is just an honest symptom of saying yes to too many requests, or other time and life pressures. But it's still not acceptable. Don't wait until it's too late. If necessary, find somebody else who can deliver.

The Good Review reflects on the contributions or possible contributions of the work, and discusses the weaknesses and limitations in a positive manner, but most particularly *clearly calls out the strengths and utility* of the work as well. The good review considers how the author's arguments, results, and demonstrations fit into closely related work as well as the field as a whole. The very best reviews raise whole new perspectives and angles of contribution that might be suggested by the work, or propose connections to areas of the literature that the author might not have thought of or even been aware of. A few missed citations represent an opportunity, not a reason to reject. And a failure to justify or fully motivate certain decisions likely represents a correctable oversight, not an unequivocal sign of poorly-conceived research.

The Good Review will raise smart and tough questions which the authors can then address in their revisions, or it might raise fresh considerations or new aspects of a design space that the authors hadn't fully fleshed out. It might even make great suggestions for how the authors could improve the articulation or organization of their work. Yet it must remain the author's paper to write, the interpretations and opinions expressed their own—not yours.

The Excellent Secondary Review

In most program committees, including MobileHCI, members take on two distinct roles depending on the paper: that of *Primary*, the person who takes chief responsibility for assessing a paper, or that of *Secondary*, who adopts a supporting but still critical role.

For MobileHCI in particular, your role will be that of Secondary for half of your assigned papers. You will also be responsible to assign one external reviewer to each such paper, and for cajoling said reviewer to produce a Good Review on time, and to the quality extolled above.

When you write your own review for a paper where you act as Secondary, your role is *almost* exactly the same as an external reviewer—you write a thoughtful and considerate review. Typically you would draft your comments without first looking at the other reviews, so as to offer an objective and independent viewpoint, although this is not required; you are entrusted to carry out your work as you see fit.

However, your Secondary reviews should absolutely be extra thoughtful and considerate, to the extent possible. Once all the other reviews are in you might add further comments on your impression and reactions to the reviews, particularly if it alters your initial impressions of the paper. Also, if you feel uncertain about the paper or lack expertise on some aspect of its subject matter, you should go ahead and say this. However, you must strive to handle your assigned papers to the best of your ability, as program committees only have so much expertise to go around, and papers are submitted on all manner of topics.

The Primary committee member assigned to the paper then takes your comments and those of the external reviewers into account when preparing his or her report: the all-important *meta-review*.

The Judicious Meta-Review—the Report of the Primary.

Perhaps your most critical task as a program committee member is writing a meta-review for each paper where your role is that of “Primary” reviewer. The meta-review may include your own perspectives and commentary on a paper, but critically it must sum up and reflect upon the perspectives raised by all of the other reviewers, including the comments of the Secondary.

Writing a good meta-review is a lot like writing a good review, only it takes into account the points raised by *all* of the reviewers, rather than just reflecting your own opinion. Balance and fairness are watchwords when writing a meta-review. You must consider both the strengths and weaknesses of the work in a fair-minded way.

Typically a good meta-review also discusses what comments you weighted more heavily from the reviewers, and why, in reaching your evaluation of the paper.

For example, you might discount a negative review that harps on a “shallow evaluation” for what is otherwise a provocative and well-realized idea. Or you might underweight a positive review that doesn't give strong reasons for accepting a paper beyond a general affinity for the research topic.

Typically a good meta-review will quote or paraphrase key comments from each reviewer. You might find yourself writing passages such as the following:

R1 argues persuasively that the paper “offers a key new insight into video games for felines, namely by the inclusion of catnip-laced transistors directly into the display.” R3 also characterizes this as “a novel (if unusual) contribution to mobile gaming.” By contrast R2 comments that “mobile apps for cats is a stupid topic,” a perspective that suggests the paper could better motivate its area of research, but one that I discount nonetheless given the generally positive comments of the other reviewers. I would also have liked to have seen more technical details on how the catnip-doping was achieved.

This is just a small sample of the type of balanced commentary you would provide, but you get the idea. It includes both the specific comments of the reviewers and your interpretation of their remarks. It can also include questions, concerns, or suggestions for improvement that you, as the Primary reviewer responsible for the paper, want to raise as well. Furthermore, the meta-review plays the important role of directing the authors to those comments or critiques that you deemed most essential in weighing the merits of the paper for acceptance, and to which the authors should devote the most attention in future revisions.

Even for good papers all of this can be a lot of work, but the burden of responsibility and the depth of commentary you must provide increase exponentially when the paper is problematic, or has many good and not-so-good points that must be addressed in turn. Because there are very few papers which everyone agrees should be accepted, the great majority fall into this dubious gray zone of uncertainty—and significantly expanded effort.

But this is where the committee members earn their keep. We must help authors surface the perhaps-unrecognized contributions lurking within their own work. We must bolster the perspectives they offer. We must raise the level of discourse to be one of thoughtful and considerate commentary among the expert reviewers we recruit.

Even for the many papers that won't make the cut, there is no place in the Good Review—and particularly in the Judicious Meta-Review—for snide remarks, kneejerk reactions, or implicit put-downs that might sound good as off-the-cuff reasons to reject a paper but do the authors little or no good in actually improving their work.

Collectively all of these commentaries should be directed to helping authors produce the best papers possible, whether a particular paper is ultimately accepted to a venue or not, and are an essential outcome of the review process.

The field does not entrust this responsibility to you lightly.

Unique Considerations for MobileHCI, “A Small Venue.”

For MobileHCI in particular, the primary goal of myself and my co-chair Hans Gellersen is to foster a community passionate about mobile interaction. To achieve this, we absolutely wish to embrace the fact that our venue is *not* the single most prestigious outlet sought by researchers for “important” new results.

With such status come freedoms and a willingness—and indeed a desire—to live much closer to the cutting edge of research into all aspects of mobility. We are willing to usher interesting but perhaps imperfect work into the literature. We can look back and see that historically MobileHCI has published a steady stream of significant papers *that have a different flavor* than might found palatable by other venues, and we are proud of this.

This is an approach that would not be possible within the strictures of a much larger and more selective venue like

CHI, the flagship conference of our sponsor (SIGCHI). CHI has many virtues, but we do not necessarily seek to emulate its norms for assessing contributions.

We cannot and do not want to be CHI, and an acceptance rate that hovers with its mouth barely above the greasy waterline of 20% is not what we are after at all. The invigorating discourse and stimulating conversations of small venues drown at such levels.

With a more inclusive and more accepting attitude comes a more diverse and far-ranging program, one that attendees and future readers of our accepted papers will likely find far more interesting and provocative as well.

We need your help to pull it off, so please judge wisely and with an open mind.

Correctable Oversights Yes—Boring Tripe No

Nonetheless, one more thing must be said about our goals for the program committee of MobileHCI:

While we are happy to help authors address correctable mistakes and oversights, having a positive attitude towards reviewing doesn't mean we want to accept crap, papers that are simply wrong or misleading, or work that lacks any meaningful research contribution.

Some new nugget of human knowledge must be at stake. It doesn't have to be a shining mother lode of gold, but it must have some value to the audience that will ultimately prospect the paper for new ideas, perspectives, and inspiration. Papers that are tedious and lack insight are a hard sell for the sluice boxes of publication, and doubly so if conference-goers will find the presentation lackluster and boring.

So looking for hidden gems doesn't mean looking at the discarded tailings of our submissions as if they were crown jewels. Papers that uniformly receive poor reviews are not ready for publication. They may even contain actual research flaws, although this is far from a certain proposition. But be that as it may they will not be accepted.

The Joys, and Potential Pitfalls, of Reviewing

There is perhaps no better way to learn how to write great papers yourself, and to keep abreast of developments in your field, than to take on more than your fair share of reviews and to dive into them with gusto.

Reading lots of **unpublished** papers gives you a finely tuned sensitivity to the concerns and expectations of the overworked reviewer. It helps you to gain an almost visceral sense of what works and what doesn't work in the presentation of a paper, and to pick up new tricks of organization, layout, and approach that will be at your beck and call when you wrestle with the write-ups for your own endeavors.

The same is true of program committee work itself, permeating yourself with the types of questions and concerns that colleagues in your broad area of study bring up regarding all manner of research. It can be an eye-opening experience

no matter your level of expertise in the field; there is always something new to learn.

One caution. An easy trap for reviewing is to be overconfident of your opinion. The authors have typically thought very deeply about the subject matter of their paper and probably understand it better than anyone else on the planet, even if that does not always come across in the text. Did you understand the author's goals, results, and discussions correctly? Is it possible that communities with interests and concerns different from your own would find the work informative, even inspiring? Does the paper touch on an area you don't know very well? Or is it by an "outsider" who perhaps touches on an area you know all too well, and hence harbor strong opinions that you might not like to see challenged? Beware of all these pitfalls when making your remarks, and remember that on the other side of the page there are equally well-intentioned authors who are trying to articulate the contributions of their research.

But always listen to your subconscious closely. Don't take the opinions of other "experts" for granted. It is okay to disagree with the authors, the reviewers, or even your fellow committee members, so long as you do it respectfully. Their intentions are noble, their motives imputable. But the hidden implications of reviewing and of being reviewed have sunk their teeth deeply into their psyches, too.

A Final Call to Action, and a Reminder of our Mantra

Thus we hereby anoint you with a critical role, *to accept papers*, and categorically *not* to find hidden flaws and assassinate them wherever possible. Serving on a program committee is not a blood sport—or at least it should not be, all evidence to the contrary, even when the chum of mediocre reviews is in the water.

At MobileHCI we want to champion work with merit wherever possible. We want to present interesting things to our peers so that they can reflect and be inspired by new directions, and so that we can entrust their wisdom and creativity.

Ultimately—when in doubt about the merits of a new idea or a direction that seems to have possible promise—let me emphasize one last time that we should adopt what strikes me as an exceedingly wise mantra:

When in doubt, trust the literature to sort it out.

It's hard to go wrong with this approach when we accept interesting work, give the authors specific and actionable feedback to improve their manuscripts, and publish the papers in an active community eager to embrace new ideas.

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But the opinions expressed—and the responsibility for any errors, oversights, or misstatements—are mine and mine alone.

I am also aware of the recent debate around "[The NIPS Peer Review Consistency Experiment](#)" but I am not sufficiently familiar with the NIPS community nor with the particulars of the study conducted to feel comfortable commenting on it here. But that such a simple test of the peer review system could prove so provocative is an interesting statement in itself.