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IN THIS Issue

From the Editor's Desk: Getting your paper rejected -- Part 2 Understanding the Reviewer

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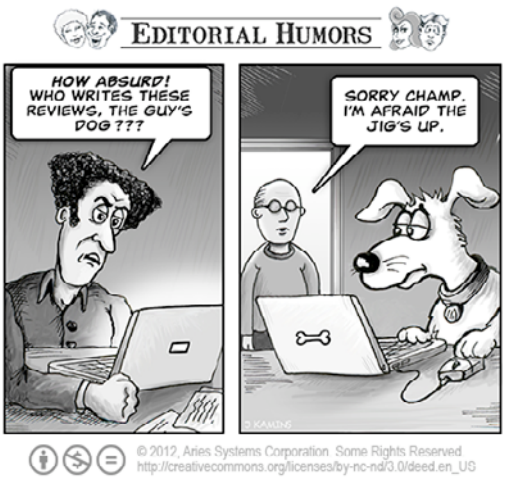
In the previous "From the editor's desk" (*IAPR Newsletter*, January 2015, I started this series thinking I could present my top 10 reasons for a paper to get rejected straightaway. Then I realized that having a coach and a buddy (could be one or multiple persons) is probably the most important starting point of producing a paper at all. So, with this article, I assume this constraint has been met. You have a coach and a buddy. You have written a paper. And, your paper has been rejected. Somehow, the reviewers didn't understand at all what you did. What went wrong? The easiest thing to do is to blame the reviewers. I admit that that is usually the thing I do as well. But when multiple reviewers agree that a paper cannot be accepted, we have to accept that the problem could be the paper.

Now the interesting journey starts. Is it the paper content that is wrong, or is the packaging? When it is the content, reviewers will mostly write something like "limited novelty", "insufficient validation", or something equivalent. That is obviously reason number one in my list. Something is simply done and written down. That's all. That is what colleagues call "me too". I can do something that others can do as well. I can segment



CALLS for PAPERS

For the most up-to-date information on IAPR-supported conferences, workshops and summer schools, please visit the IAPR web site: *www.iapr.org/conferences/*

GREC 2015
11th International Workshop on
Graphics Recognition
Sousse, Tunisia
Dates: Aug. 20-21, 2015
Deadline: May 10, 2015

CIARP 2015
XX Iberomamerican Congress on
Pattern Recognition
Montevideo, Uruguay
Dates: Nov. 9-12, 2015
Deadline: May 15, 2015

HIP 2015
3rd International Workshop on
Historical Dicument Imaging and Processing
Gammarth, Tunisia
Dates: Aug. 22, 2015
Deadline: Jun. 1, 2015

ACPR 2015
3rd IAPR Asian Conference on
Pattern Recognition
Kuala Lumpur, Malaysia
Dates: Nov. 3-6, 2015
Deadline: Jun. 1, 2015

DICTA 2015
International Conference on
Digital Image Computing:
Techniques and Applications
Adelaide, Australia
Dates: Nov. 23-35, 2015
Deadline: Jun. 29, 2015

ICVNZ 2015
Image and Vision Computing New Zealand
Auckland, New Zealand
Dates: Nov. 23-24, 2015
Deadline: Sep. 18, 2015

PSIVT 2015
7th Pacific Rim Symposium on
Image and Video Technology
Auckland, New Zealand
Dates: Nov. 23-27, 2015
Deadline: Jul. 24, 2015

2016

ICPR 2016
23rd International Conference on
Pattern Recognition
Cancun, Mexico
Dates: Dec. 4-8, 2016
Paper Submission Deadline: May 2, 2016
Contest, tutorial and workshop proposal Deadline: January 13, 2016

an image, too. I can recognize bikes, too. I can add noise to Lena and remove it, too.

Okay, you'll say, I did a bit more than that. Still my paper got rejected. Still the reviewers didn't get the point. Well, then it is time to get to understand the reviewers.

Understanding reviewers:

What helped me quite a lot is being a reviewer for quite a few journals and conferences, as well as talking with colleagues in the same position. Reviewers need to find time to do reviews. This sounds trivial, but it isn't. In most cases, there's a deadline and plenty of time (one month or more) to do the reviews. In theory. However, in reality, there are other deadlines to be met, courses to give, presentations to prepare, grant and project proposals to write, committee work to be done. And, there is the stack of papers with "To Do" on it. In the worst scenario, it happens that after a reminder slightly before the review deadline, the reviewer has, say, less than a day to review 10 conference papers. This means there may be only about half an hour per paper to read it and to write a review. The quick and dirty way to do it

is to scan the papers to get an impression (which works quite well in many cases, I must confess). A similar sequence of events can play out when reviewing a journal paper.

So, what does the reviewer look for when scanning a paper? This is where the packaging comes into play. Packaging may give good hints as to whether the contents are good or not. My numbers six to ten reasons deal with this issue. So let me count backwards.

Reason 10: Bad Figures.

Figures should be a visual explanation of the contribution. If I get no idea of what the figures should tell me, then something is wrong. If I have to guess what lines and points mean, if the caption is very, very, brief, if I only have a "before" and "after" and a statement like "as one clearly can see", I get annoyed. The figures should clearly show what is supposed to be seen and why it is relevant. So, basically, a reviewer does not like it when (s)he has to guess.

Reason 9: No comparison with state of the art.

Most papers have an obligatory State of the Art section. This is very nice, as it sketches the related approaches. However, when it is a section that can be removed without affecting the paper, something is wrong. If you say that what others did is relevant, then you will have to say why your approach is better is some sense (faster, more accurate, being able to derive something additional). Equivalently: a scientific contribution is suitable for a paper only when it goes beyond state of the art. The author(s) should mention that!

Reason 8: No experiments.

Surprisingly, I still get papers to review in which the authors only present an idea. At most they may show a 'visual proof of concept'. This is rather insufficient as we, the scientific community, welcome ideas that work, that are proven to work. So we need numbers: tables, graphs, whatever. This relates to Reason 9. It is not enough just to say that your method is nice; the authors need to state and deomonstrate how "nice". Does it segment / denoise / recognize better than what we have? Without serious experiments this is difficult to support and is a quite straightforward reason for rejection.

Reason 7: Old references.

References are important for several reasons. They show that you know the state of the art – that is, cite recent papers! They also show that you know what other people do. So, incomplete and old references are very, very, bad. You may have compared your approach with other methods, but when they come from the previous century, the reviewer will have serious problems with your paper. Does this imply that you don't know the actual work? Is there actual work – is the topic relevant at all? For most conference contributions, the chances are quite big that at least in previous years at this conference series papers discussing similar topics have appeared. So… didn't you care about related work? Outperforming a method that is more than ten years old isn't that difficult in many applications.

Reason 6: No State of the Art.

Need I say more? This relates to the previous points. Without state of the art, all experiments are void. You simply stick to "me too". Papers without this section are usually implementations of existing methods. Obviously, as a reviewer no novelty can be found. This also holds for old references: what was state of the art for ten, twenty years is not state of the art today.

Writing these points about packaging, I realize that there are many more, so, I include Reason 11, which I have seen too often in my career, below. What about Reasons five, four, three and two? They are more content-related, so I'll keep those for next time

Reason 11: Wrong template.

Nothing is more annoying than seeing an IEEE template formatted paper for conferences having their papers as LNCS proceedings (or vice versa). Especially when a template is available and often obligatory, this is an almost straight reject. It suggests that your paper has been rejected somewhere else. Why should I read your paper?

Happy Writing!:

For those who do not want to wait until the next newsletter: my list is available in the pdf 10 ways to get your paper rejected. Remember: "Writing a paper is not about describing the system you programmed or the algorithm you improved, it is about 'conveying your idea from your head to your reader's head'" (Simon Peyton Jones, from his excellent slides How to write a great research paper).