

How to Properly Review AI Papers?

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Valse Webinar



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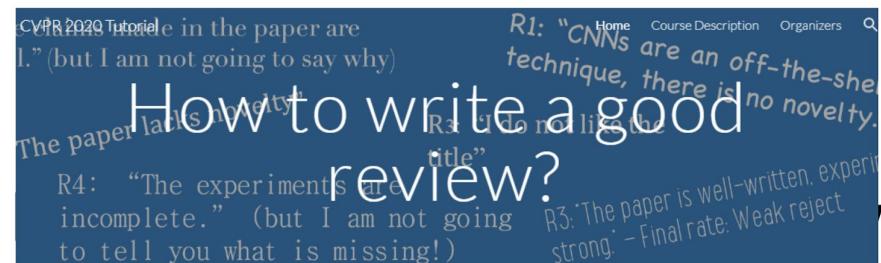
Adversarial Tasks

- How to write an AI paper?
- How to review an AI paper?



Motivations

- The explosive growth in AI related fields
 - Need I say more? $(7000 \text{ submissions} \times 3 \text{ reviews}) / 7 = 3000 \text{ reviewers}$
- The rise of new and inexperienced reviewers
 - PhD students
 - MS students
 - Undergraduate students
 - Researchers from other fields
- Lack of systematic training for reviewers
 - Unsupervised learning from personal (often bad) experiences
- A well recognized problem for the communities 冤冤相报何时了
 - The health and growth of a field depends crucially on the quality of peer reviews

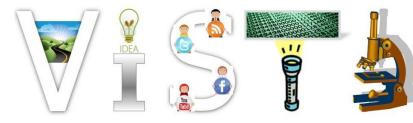




Outline

- 他山之石 What other experienced researchers have said
 - Rick Szeliski (Facebook) [[slides](#)]
 - Jordi Pont-Tuset (Google) [[slides](#)]
 - Fatma Güney (Koç University) [[slides](#)]
 - Konrad Schindler (ETH Zürich) [[slides](#)]
 - Michael Goesele (Facebook) [[slides](#)]
- Main Principles
 - 己所不欲，勿施于人
 - It takes a community 社区是由每个人组成的
 - Look for reasons to accept
 - Writing is important
 - The No. 1 goal of top conferences/journals
 - What if you are not familiar with the area?





社区是由每个人组成的

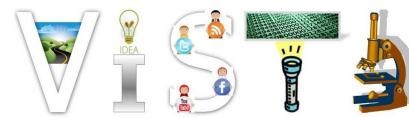
- Protect, not pollute the playing ground
 - Everything could come back and haunt you
- Be responsive
 - Late/missing reviews hurt everyone involved (and you too)
- Be responsible
 - Senior researchers should review their students' reviews!
- Do not force authors to cite your own (irrelevant) papers
 - Do suggest missing important references (不夹带私货, 但要堵绝恶意不引用)
 - Do point out plagiarism / double-submission
- Do not force authors to cite (unpublished) arxiv papers
 - Not peer reviewed
 - May contain errors and false information (some could be malicious!)



己所不欲，勿施于人

- Submit the reviews on time
 - Aren't you tired of waiting forever for the first round decisions from TPAMI/TMM/etc.?
- Spend enough time understanding the paper
 - Aren't you mad at the reviewers who clearly did not read your paper adequately?
- Be constructive
 - One good deed deserves another
- Do not use small excuses to reject
 - Look for reasons to accept
- Do not simply wave the two big sticks
 - “No/little novelty”
 - “Poor/insufficient experiments”
- Do not force a gazillion of new experiments
- **Do read other reviews and rebuttal (be open-minded)**





Look for Reasons to Accept

- What is novelty?
 - Novel problem (most exciting) or generalizable method or deep insight
 - Novel solution to an existing problem
 - Is “combination of known components” by definition trivial?
- How important is higher performance? 不以百分点论英雄
 - Statistically significant? sufficient data size?
 - Pure brute-force?
- A balancing act
 - Highly novel/exciting but with clear flaws (has enough been demonstrated?)
 - Flawless but not exciting (what could the community learn from such work?)
- Terminology game
 - Does the same terminology mean the same thing?
 - Does different terminologies mean something new?



Novelty

Novelty — A Stick to Beat Authors?

Of course we do not want trivial repetitions or copies

BUT very few ideas are really completely new!

- transfer from other scientific fields
 - e.g., image retrieval
- independent rediscovery
 - e.g., RANSAC
- engineering application of theory
 - e.g., MRFs
- assembly of known components
 - e.g., panorama stitching

“they steal from me, but I steal from everybody”



Novelty in What Sense?

New **question** nobody thought of yet

- e.g., Im2GPS

New technical **solution** to a known problem

- e.g., FlowNet

Better **analysis** and understanding

- e.g., ResNets as ensembles

Better **results**

- e.g., Monodepth

Wrong question: “Is this interesting for me?”

Better question: “Could this interest someone at CVPR?”



Novelty

Levels of Novelty

New concept for everybody in the world

- theory of relativity, new animal species, ...

New concept for computer vision

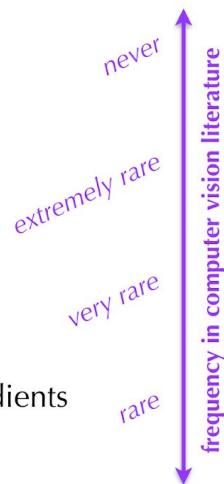
- e.g., level set methods, MRFs, AlexNet

Important extension or algorithmic novelty

- e.g., α -expansion, batchnorm, ResNet

New, clever engineering with known ingredients

- e.g., SIFT, vocabulary tree, VGG



You rarely get the chance to review such papers.
Try not to mess it up!

Levels of Novelty

Useful minor upgrade

- tweaks of loss function, efficient real-time versions, ...
- *frequent*

Application to new task

- GANs for X , X for mobile robots...
- *frequent*

Consolidation and Infrastructure

- comparisons, benchmarks, revisits of forgotten knowledge...
- *moderately frequent*

Wrong question: "is this already known to anyone?"

Better question: "could this advance computer vision?"



Novelty

Role of Performance Numbers

If it is innovative, don't obsess about numbers

- tuning of a new method has not yet been crowd-sourced

Good numbers alone are not (a sign of) novelty

- good performance **can** be due to a new approach (AlexNet)
- or due to more engineers, or lots of data, or overfitting, or...

No numbers on real data can be a good sign

- the most useful applications are those that can be solved **only** with computer vision \Rightarrow no way to obtain ground truth!

Novelty vs. safety

- be aware of bias towards incremental extensions: method is known to work, in reviewers' comfort zone, "hard to reject"

Wrong question: "~~does it give the best numbers?~~"

Better question: "could it play a role to push future numbers?"

Relevance vs. Elegance

Beauty is in the eye of the beholder, relevance much less

- Ask yourself whether it could be useful for **any** application

Remember, break-throughs can be simple (in hindsight)

- e.g., SIFT, bilateral filter, ResNet

Be aware, break-throughs can be complicated

- e.g., Pock/Chambolle primal-dual algorithm

Value technical elegance

- but don't forget, computer vision is an engineering science: (almost) all our maths is trivial for the right mathematician

Wrong question: "~~Is it pretty? too simple? too complicated?~~"

Better question: "Could it be useful? Is the complexity needed?"



Writing is important!

- Paper is written for others (HELP REVIEWERS HELP YOU!)
 - Clarity
 - Proper English
 - Neat tables, illustrative figures, and effective examples
 - Is “*paper gestalt*” justified? (can you train yourself?)
 - Deep Paper Gestalt claims it can safely reject 50% of the bad papers while wrongly reject only 0.4% of the good papers
- Innocent mistakes
 - Apparent errors (symbols, equations, annotations, references)
- Intentional/malicious acts
 - Ignore important/well-known/recent/classic references (reinventing the wheel)
 - Smoke and mirror (for GPU abuse)
- Could poor writing be the reason to reject
 - Did the authors respect the reviewers and their time, and act professional?



What is the Goal of Top Conferences/Journals?

- Making sure good work is published
 - Very important, 做伯乐
- Making sure bad work is not published
 - No so important, 水货会自然流失
- Test of time
 - 时间, 才是检验good work的唯一标准
 - Oral vs. Poster (our own study shows...)
 - Best papers (test-of-time award papers?)
- Different standards for different tiers of venues?
 - Yes





What if you are not familiar with the area?

- Keep an open mind
 - It's a learning opportunity (and get inspired accidentally)
- Do you best to follow the paper
 - Check the main references if necessary
- Do not get affected by personal taste (e.g., data-driven vs model-driven)
 - Consider if the paper is worth seeing by the community
- You are doing an important job in this case !
 - 当局者迷, 旁观者清 (conflict of interest in either way)
 - You are the fairest of all (esp. given the wide use of paper matching algorithms)



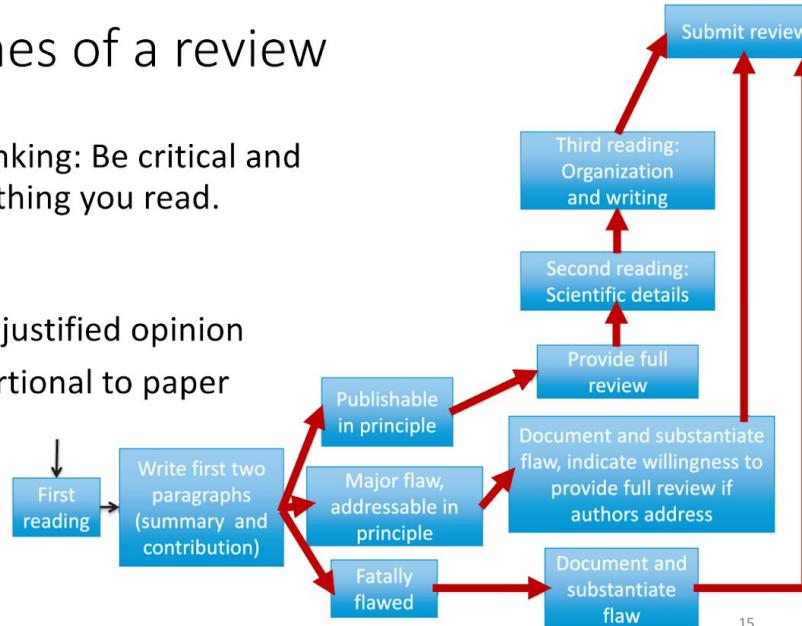
The Review Flow Diagram

Three outcomes of a review

Important shift in thinking: Be critical and doubt absolutely anything you read.

Practical tips:

- aim for strong, well justified opinion
- review effort proportional to paper quality

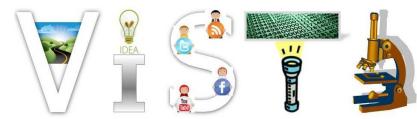


Taken and modified from: K. A. Nicholas, W. Gordon: A Quick Guide to Writing a Solid Peer Review. Eos, Vol. 92, No. 28, 12 July 2011

15

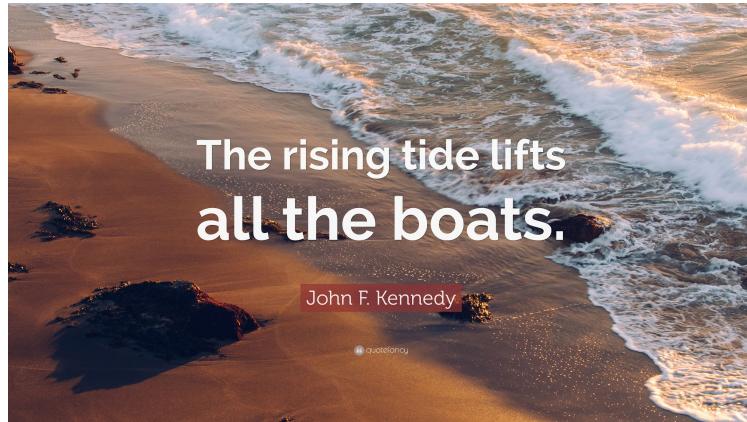


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Let's Make Reviews Great Again!

Thank you!



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