Reviewer

T-MM State of the Journal

› T-MM is in good shape and continues to rise in impact

› **First-round decision** time: 15.8wks (⬇15.4 from 18.4)
   – TIP 22.0(22.3), **TCSVT** 7.4(9.0), **TIFS 12.4**(13.8)
   – TPAMI 19.9(20.2), TNNLS 15.5(17.8), TKDE 25.9(24.3)

› Impact Factor in **2021: TMM 8.182 (from 6.513)**
   – 2015:2.303, 2016:3.509, 2017:3.977, 2018:5.452, 2019:6.051
   – Benchmarks: TOMM 4.15(3.275/3.144), TCSVT 5.87(4.68/4.13),
               TIP 11.04(10.85/9.34), TNNLS 14.25(10.45/8.79),
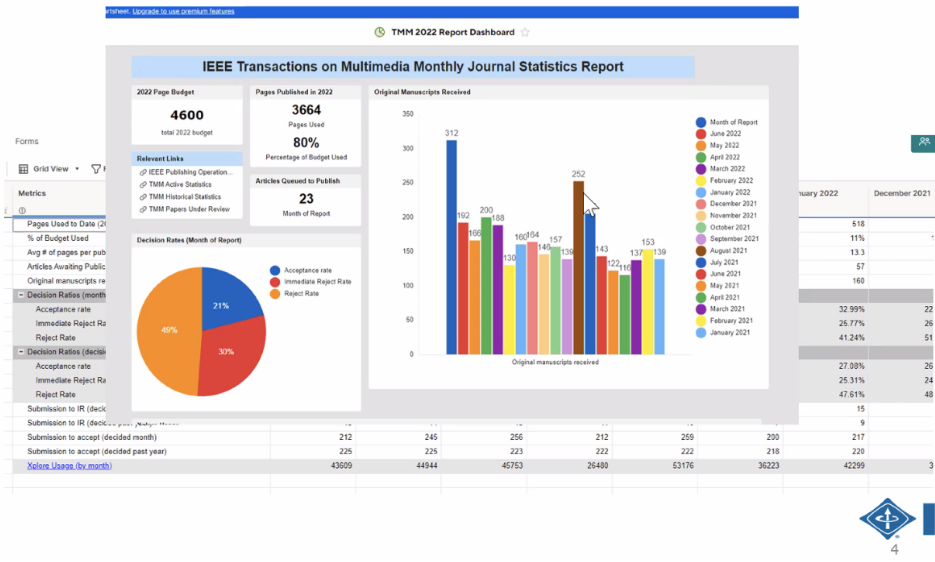   TPAMI 24.31(16.39/17.86), TKDE 9.23(6.97/4.93)

◆ IEEE

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Administrative Reject

› Administrative Reject
   – Decision can only be sent out by the EiC
   – Currently about 25% of submissions

› 4 types of Administrative Reject
   - Immediate Reject - Presentation and Comprehension
   - Immediate Reject - Out of Scope
   - Immediate Reject - Plagiarism and Author Misconduct
   - Immediate Reject - Technical Content **(two sets of eyes)**

› AE desk reject without review
   – Provide a concrete justification
   – Ask EIC if unsure
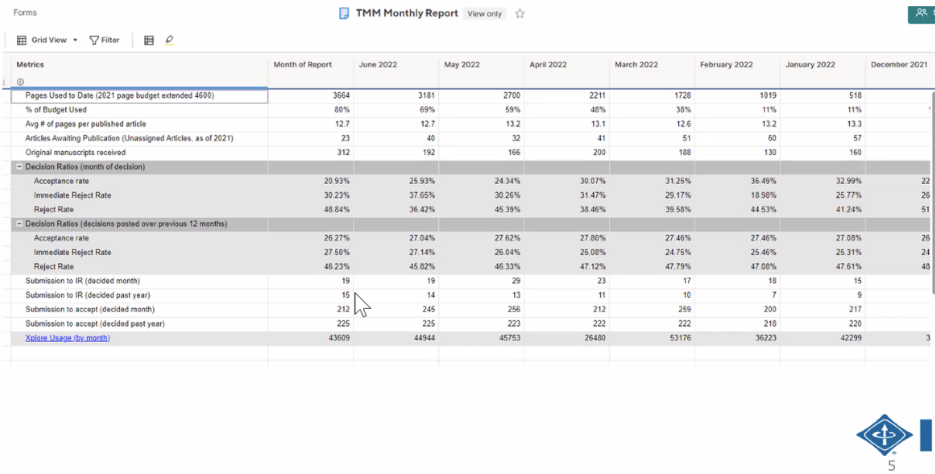   – Regarding extensions of conference papers (30%-30% rule)

◆ IEEE

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Acceptance rate: 32% in 2019-2020
28% in 2020-2021
26% in 2021-2022

TMM 2022 Report Dashboard

IEEE Transactions on Multimedia Monthly Journal Statistics Report

2022 Page Budget	Pages Published in 2022	Original Manuscripts Received
4600 total 2022 budget	**3664** Pages Used	
	80% Percentage of Budget Used	

Relevant Links
- IEEE Publishing Operation...
- TMM Active Statistics
- TMM Historical Statistics
- TMM Papers Under Review

Articles Queued to Publish
23
Month of Report

Decision Rates (Month of Report)
- Acceptance rate
- Immediate Reject Rate
- Reject Rate

21% / 49% / 30%

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Acceptance rate: 32% in 2019-2020
28% in 2020-2021
26% in 2021-2022

TMM Monthly Report View only

| Metrics	Month of Report	June 2022	May 2022	April 2022	March 2022	February 2022	January 2022	December 2021
Pages Used to Date (2021 page budget extended 4600)	3664	3181	2700	2211	1728	1019	518	
% of Budget Used	80%	69%	59%	48%	38%	11%	11%	
Avg # of pages per published article	12.7	12.7	13.2	13.1	12.6	13.2	13.3	
Articles Awaiting Publication (Unassigned Articles, as of 2021)	23	40	32	41	51	60	57	
Original manuscripts received	312	192	166	200	188	130	160	
− Decision Ratios (month of decision)								
Acceptance rate	20.93%	25.93%	24.34%	30.07%	31.25%	36.49%	32.99%	22
Immediate Reject Rate	30.23%	37.65%	30.26%	31.47%	29.17%	18.98%	25.77%	26
Reject Rate	48.84%	36.42%	45.39%	38.46%	39.58%	44.53%	41.24%	51
− Decision Ratios (decisions posted over previous 12 months)								
Acceptance rate	26.27%	27.04%	27.62%	27.80%	27.46%	27.46%	27.08%	26
Immediate Reject Rate	27.50%	27.14%	26.04%	25.08%	24.75%	25.46%	25.31%	24
Reject Rate	46.23%	45.82%	46.33%	47.12%	47.79%	47.08%	47.61%	48
Submission to IR (decided month)	19	19	29	23	17	18	15	
Submission to IR (decided past year)	15	14	13	11	10	7	9	
Submission to accept (decided month)	212	245	256	212	259	200	217	
Submission to accept (decided past year)	225	225	223	222	222	218	220	
Xplore Usage (by month)	43609	44944	45753	26480	53176	36223	42299	3

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What papers are suitable for TMM?

- Must cite and discuss prior work published in TMM
  - Otherwise why is it suitable for TMM?
  - Need to establish at least novelty over previous TMM papers!
- Multimedia != CV
  - Tired topics: person re-id, super-resolution, saliency
  - Multimodal data and/or multimdimensional problems
  - A novel problem beats a few percent better
- Writing problems
  - Too many equations, very little idea/insight
  - Writing is not for the authors themselves!
  - Self-plagerism, < 30% overlap with the conference version
- What if reviewers ask to cite irrelevant papers?
  - Should cite the relevant ones, but can decline irrelevant ones (notify AE/EiC)

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Motivations

- The explosive growth in AI related fields
  - Need I say more? (7000 CVPR'20 submissions x 3 reviews) / 7 = 3000 reviewers!
  - TMM submission number increased by 40% in 2021 over 2020 (spilled over?)
- 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
  - We are in the same ecological system TOGETHER

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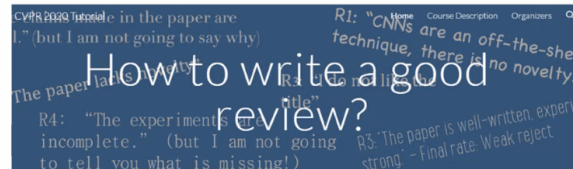
Outline

- 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 Do not do to others what you do not want others do to you
  - 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?
  - What if you ARE 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 <u>on time</u>
  - 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 turn deserves another
- Do not use small excuses to reject
  - Look for reasons to accept
- Do not simply wave the two big sticks/hammers
  - "No/little novelty"
  - "Poor/insufficient experiments"
- Do not force a gazillion of new experiments
- **Do read other reviews and rebuttal (be open-minded)**

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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?
  - Purely 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?

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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"

H. Ledbetter

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

never
extremely rare
very rare
rare

frequency in computer vision literature

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 ⇒ 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?"

Watch out for fake complexity as novelty!

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)
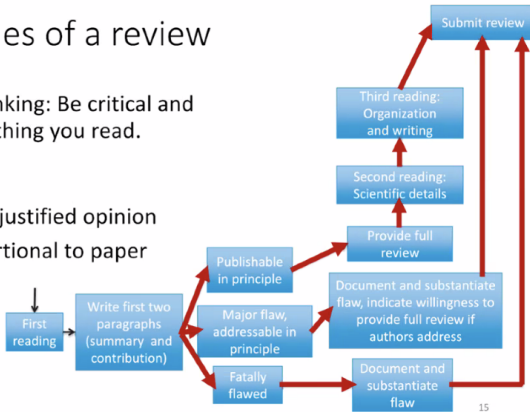 - You are the fairest of all (esp. given the wide use of paper matching algorithms)

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