Novelty is in the eye of the beholder

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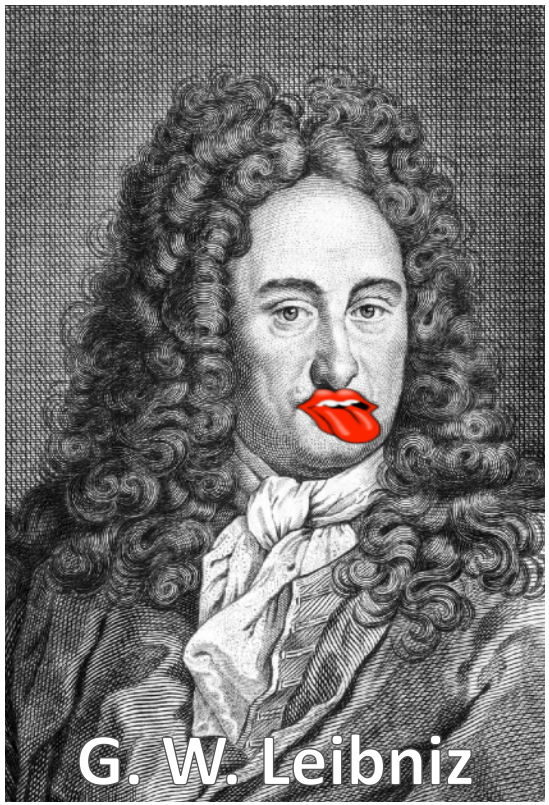
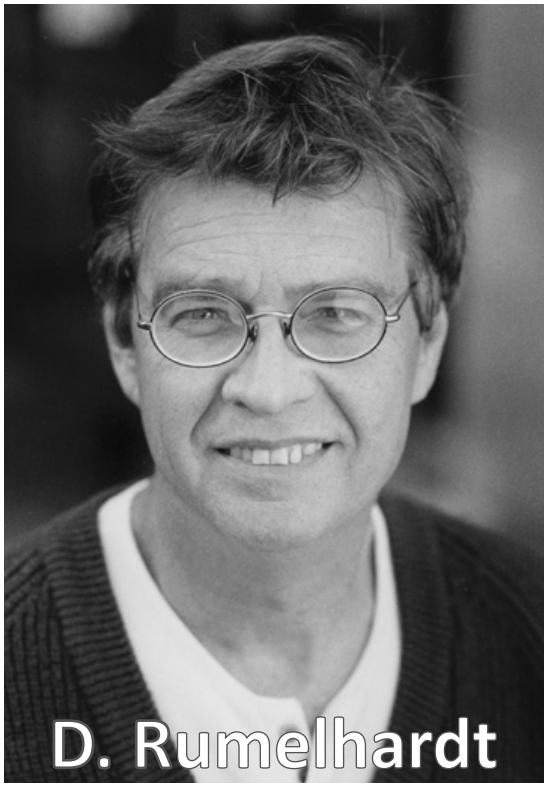
"…The paper lacks novelty. Its only claimed contribution is the 'back-propagation' algorithm, which is just a trivial application of the chain rule of differentiation, known since at least [Leibniz 1676]…"





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?"

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?"

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?"

Novel Ideas vs. Silly Ideas

It is an important part of (engineering) science to try unproven things that **could work**

Are you really sure it is bad, or just unsure if it is good?

- the review should filter out obviously silly approaches
- it should not favour minor variations of well-proven approaches just to "be on the safe side"
- rejecting an important new idea is more detrimental than accepting one that later turns out to be flawed

Beware of fashion waves, monoculture, tunnel vision

- "why don't you use a deep net" is by itself not a valid criticism!

Wrong question: ~~"Can I be sure that this is useful?"~~

Better question: "Is it exciting and without obvious flaws?"

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?"

Take-home message

The concept of novelty/originality is not as well-defined as it seems

You know it when you see a clear case, but most of the time we review edge cases

Think carefully before you write "…the paper lacks novelty…"

Think carefully before you parrot the authors' claim of "…a completely novel approach…"