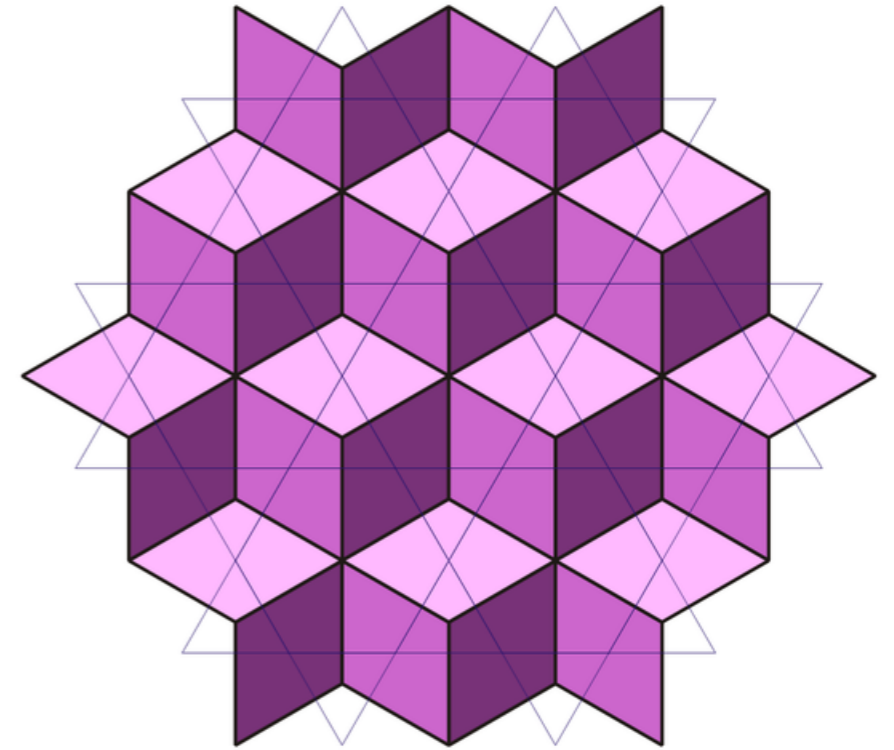




Inria
inventeurs du monde numérique

C@fé-In

Une rencontre informelle
autour d'un sujet scientifique.



Notre science peut-elle être reproductible ?

Damien.Saucez@inria.fr

Gauche ou droite?

- *“Under our model, it turns out to be optimal for the Democrats to move slightly to the right but staying clearly to the left of the Republicans’ current position on economic issues.” [1]*
- “[...]because of a data coding error on one of the variables, all our analysis of social issues is incorrect. [...]” [2]

[1] Gelman A, Cai CJ. Should the Democrats move to the left on economic policy?. The Annals of Applied Statistics. 2008;2(2):536-49.

[2] Gelman A. Correction: Should the Democrats move to the left on economic policy?. The Annals of Applied Statistics. 2013;7(2):1248-.

Etrange mais fabuleux

- Geoffrey Chang découvre la structure cristalline d'une certaine protéine membranaire.

- Belles publications

3 Science, 1 Nature, 1 PNAS et 1 JMB.

- Toutes rétractées en 2006 à cause d'une erreur de signe dans un logiciel...

certains de ces papiers sont encore cités en 2019.

Essay

Why Most Published Research Findings Are False

John P. A. Ioannidis

Summary

There is increasing concern that most current published research findings are false. The probability that a research claim is true may depend on study power and bias, the number of other studies on the same question, and, importantly, the ratio of true to no relationships among the relationships probed in each scientific field. In this framework, a research finding is less likely to be true when the studies conducted in a field are smaller; when effect sizes are smaller; when there is a greater number and lesser preselection of tested relationships; where there is greater flexibility in designs, definitions, outcomes, and analytical modes; when there is greater financial and other interest and prejudice; and when more teams are involved in a scientific field in chase of statistical significance. Simulations show that for most study designs and settings, it is more likely for a research claim to be false than true. Moreover, for many current scientific fields, claimed research findings may often be simply accurate measures of the prevailing bias. In this essay, I discuss the implications of these problems for the conduct and interpretation of research.

factors that influence this problem and some corollaries thereof.

Modeling the Framework for False Positive Findings

Several methodologists have pointed out [9–11] that the high rate of nonreplication (lack of confirmation) of research discoveries is a consequence of the convenient, yet ill-founded strategy of claiming conclusive research findings solely on the basis of a single study assessed by formal statistical significance, typically for a p -value less than 0.05. Research is not most appropriately represented and summarized by p -values, but, unfortunately, there is a widespread notion that medical research articles

It can be proven that most claimed research findings are false.

should be interpreted based only on p -values. Research findings are defined here as any relationship reaching formal statistical significance, e.g., effective interventions, informative predictors, risk factors, or associations.

“Negative” is actually a misnomer, and the misinterpretation is widespread.

is characteristic of the field and can vary a lot depending on whether the field targets highly likely relationships or searches for only one or a few true relationships among thousands and millions of hypotheses that may be postulated. Let us also consider, for computational simplicity, circumscribed fields where either there is only one true relationship (among many that can be hypothesized) or the power is similar to find any of the several existing true relationships. The pre-study probability of a relationship being true is $R/(R + 1)$. The probability of a study finding a true relationship reflects the power $1 - \beta$ (one minus the Type II error rate). The probability of claiming a relationship when none truly exists reflects the Type I error rate, α . Assuming that c relationships are being probed in the field, the expected values of the 2×2 table are given in Table 1. After a research finding has been claimed based on achieving formal statistical significance, the post-study probability that it is true is the positive predictive value, PPV. The PPV is also the complementary probability of what Wacholder et al. have called the false positive report

$\times 2$ table, one gets $PPV = (1 - \beta)R / (R - \beta R + \alpha)$. A research finding is thus

[3] Ioannidis JP. Why most published research findings are false. PLoS medicine. 2005 Aug 30;2(8):e124.

Essay

Why Most Published Research Findings Are False

John P. A. Ioannidis

Google Scholar

SEARCH SIGN IN



John P. A. Ioannidis

FOLLOW

Professor of Medicine, [Stanford University](#) (previously at U Ioannina, Greece)
Verified email at stanford.edu - [Homepage](#)

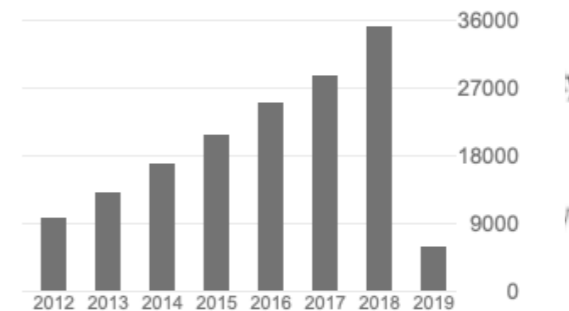
[Evidence-based medicine](#) [research methods](#) [meta-analysis](#) [clinical epidemiology](#)
[genetic epidemiology](#)

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TITLE	CITED BY	YEAR
Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement D Moher, A Liberati, J Tetzlaff, DG Altman, Prisma Group PLoS medicine 6 (7), e1000097	41036	2009
Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement D Moher, A Liberati, J Tetzlaff, DG Altman, G Antes, D Atkins, V Barbour, ... Annals of internal medicine 151 (4), 264-269	40414	2009
Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement D Moher, A Liberati, J Tetzlaff, DG Altman, Prisma Group PLoS medicine 6 (7), e1000097	40364	2009

Cited by [VIEW ALL](#)

	All	Since 2014
Citations	195387	132973
h-index	176	127
i10-index	823	684



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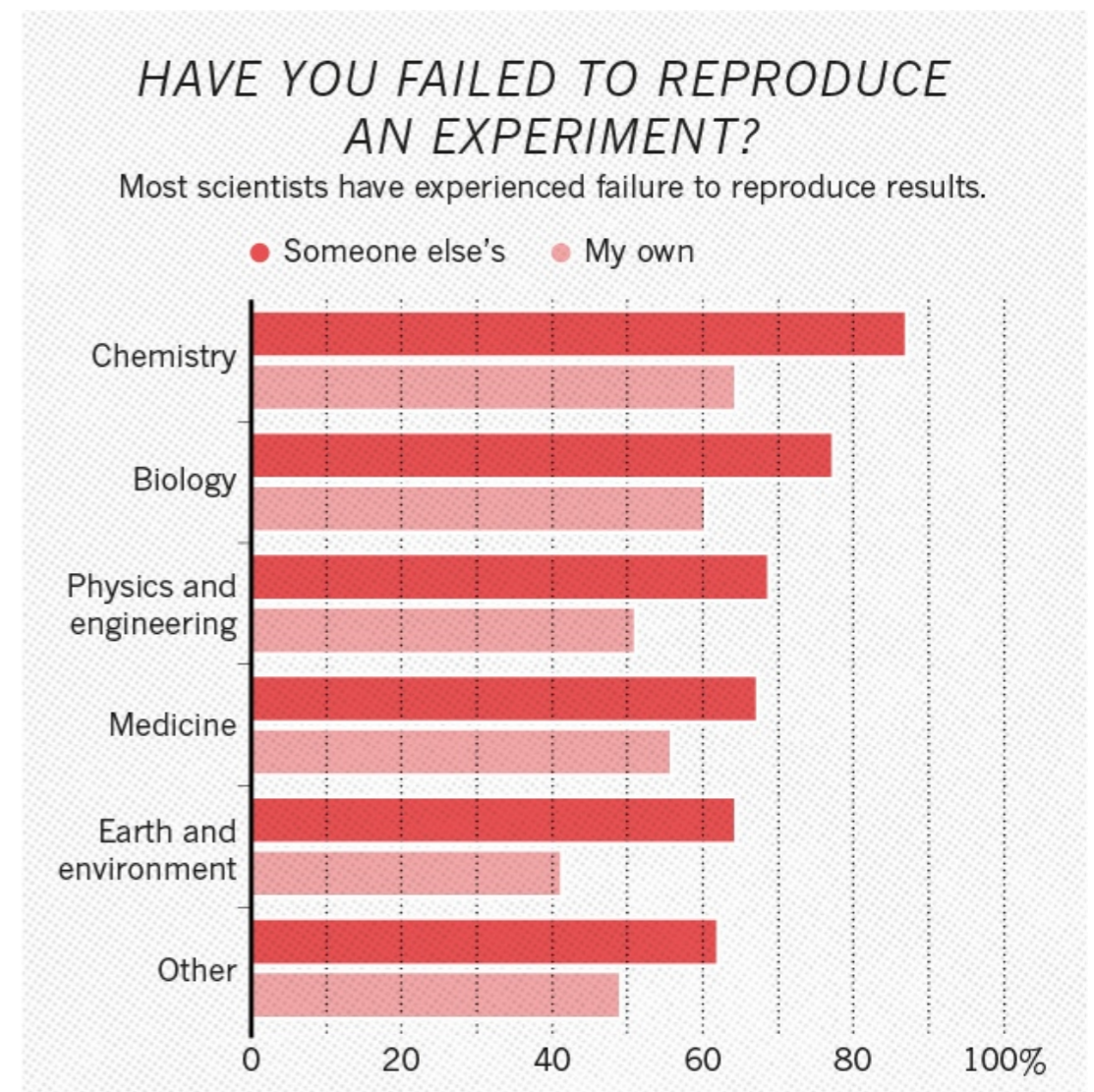
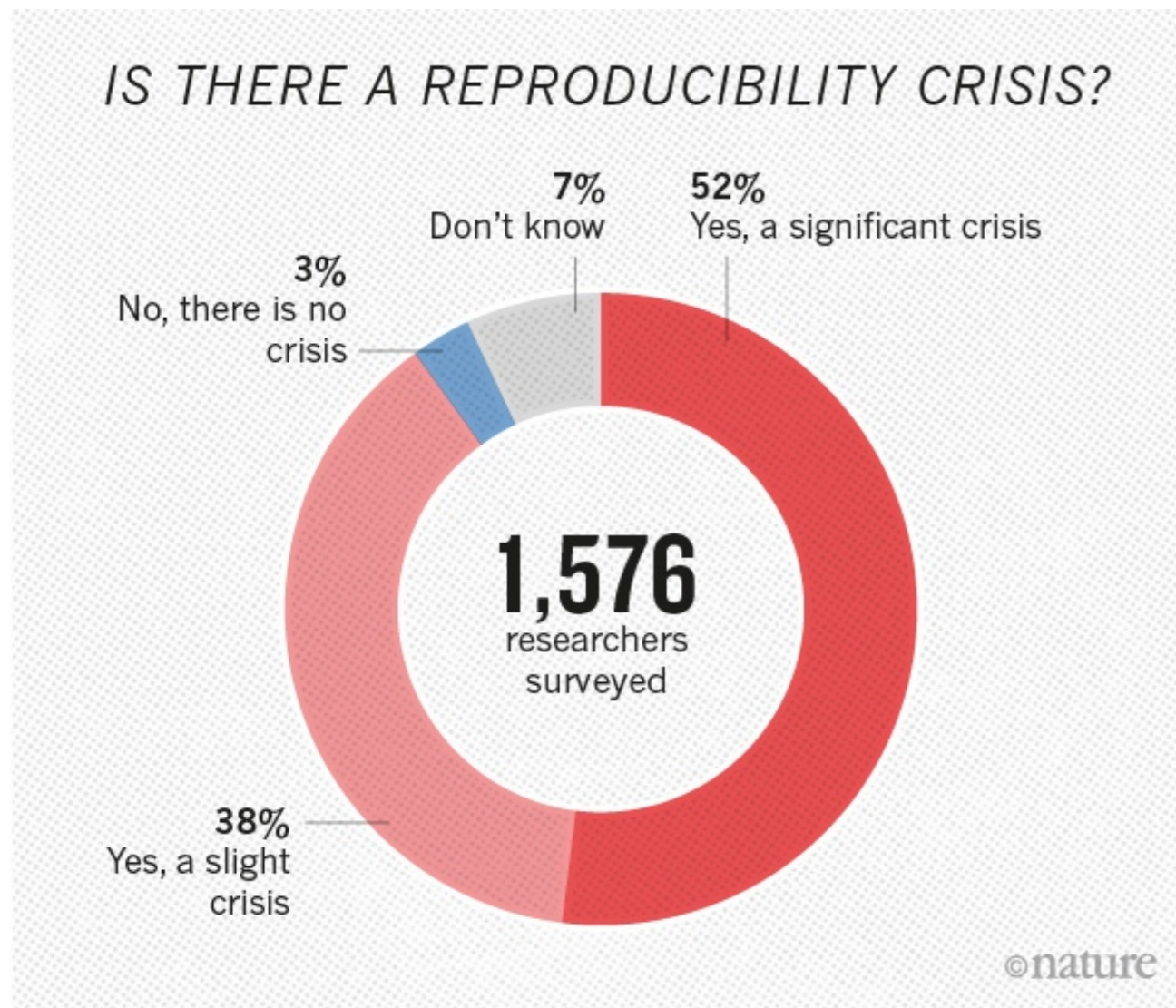
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[3] Ioannidis JP. Why most published research findings are false. PLoS medicine. 2005 Aug 30;2(8):e124.

Crise de la reproductibilité[4]

- Prise de conscience à partir début des années 2000.



[4] Baker M. 1,500 scientists lift the lid on reproducibility. Nature News. 2016 May 26;533(7604):452.

*“non-reproducible single
occurrences are of no significance
to science”*

– Karl Popper

Terminologie

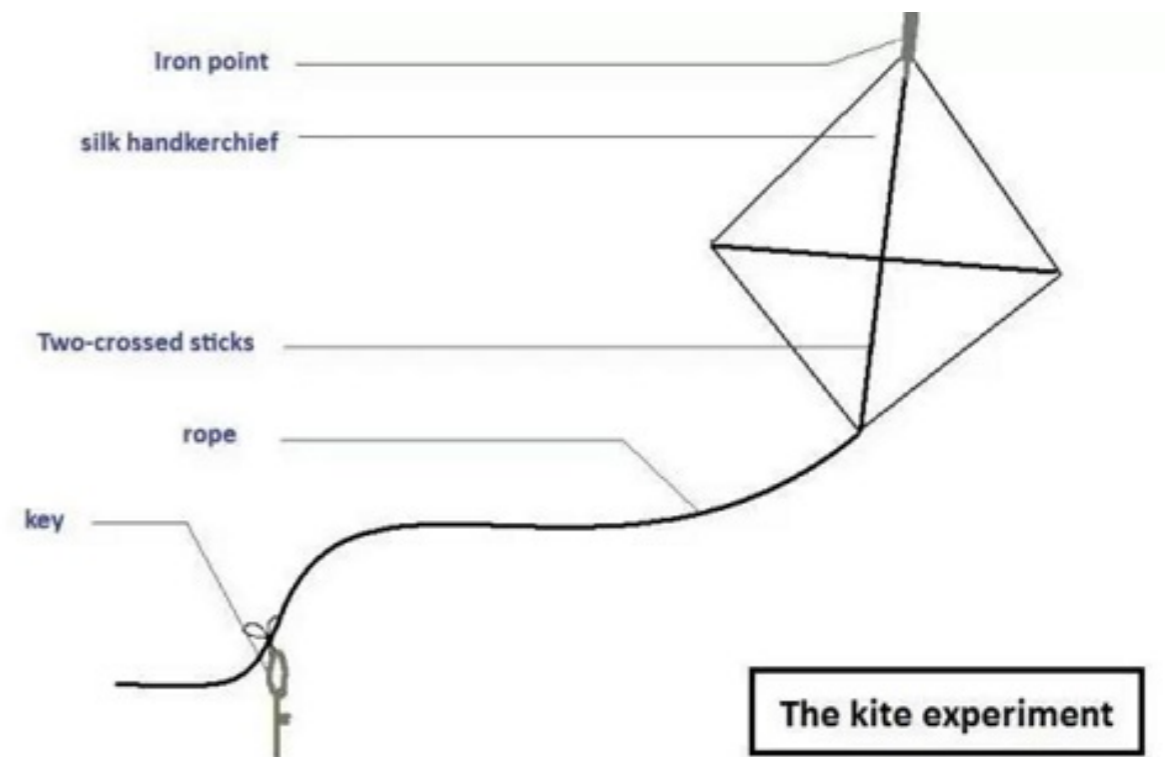
Recherche expérimentale

1. Emettre des hypothèses
2. Reproduire un phénomène
3. (In)validation des hypothèses.



Production d'artéfacts*

- Artéfact: objet digital
 - créé par les auteurs pour être utilisé dans l'expérimentation
- ou généré par l'expérimentation.



* <https://www.acm.org/publications/policies/artifact-review-badging#appendix>, 2019-02-25

† Crédit image sur <http://www.benjamin-franklin-history.org/kite-experiment/>, 2019-02-25

Reproduire un phénomène?*

- Répétition

même équipe, même environnement expérimental

- Réplication

équipes différentes, même environnement expérimental

- Reproduction

équipes différentes, environnements expérimentaux différents

* <https://www.acm.org/publications/policies/artifact-review-badging#appendix>, 2019-02-25

Pourquoi est-ce
compliqué?

Un problème
sociologique

Prime à la nouveauté

We expect submissions to be daring and emphasize novelty and creativity. We recognize that more novel concepts can be harder to fully develop and evaluate, and the review process will take this into account. We encourage authors to discuss the limitations of their ideas in addition to the benefits.

Unlike some previous years, SIGCOMM 2019 will not have a separate experience track. However, we do encourage the submission of experience papers, particularly from industry, that provide detailed technical insight into real-world deployments of novel networking technologies and systems.

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Processus de review

Submission Instructions

Submissions must be original, unpublished work, and not under consideration at another conference or journal. Each submission must be a single PDF file in two columns, 10 point format following the sigconf format of the **2017 ACM Master Article LaTeX Template**. Use the sample latex file `sample-sigconf.tex`. Submitted short papers should be no longer than six (6) pages, and long papers no longer than twelve (12) pages, including all material except references. SOSR '19 is double-blind, meaning that authors should make a good faith effort to anonymize papers. Papers should be submitted electronically via the submission site.

Accepted papers will be published in the ACM Digital Library. We stress that the publication of short papers at SOSR does not preclude the later publication of a full-length version of the paper at a conference or in a journal. Authors of accepted papers are expected to present their papers at the symposium and will have the opportunity to present a poster/demo at an interactive session, co-located with ONS.

Please direct any submission-related questions to sosr19chairs@colgate.edu.

Submission site:

Please visit <https://sosr19.hotcrp.com/> to submit.

Important Dates

Abstracts due: November 8, 2018 (5pm PT)

Paper submission: November 15, 2018 (5pm PT)

Notification: January 15, 2019

Camera-ready due: February 28, 2019

Conference: April 3-4, 2019 in San Jose, CA

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Forme des publications

“Always remember that it is impossible to speak in such a way that you cannot be misunderstood [...]”

– Karl Popper

Biais de publication

- La communauté favorise des résultats “positifs”

e.g., difficile de publier des résultats qui confirment l’hypothèse nulle.

Peu de valorisation

- Pour les auteurs

impression de perte de temps,

rarement considéré pour les postes ou promotions.

- Pour les reviewers

prend beaucoup de temps,

demande des compétences techniques,

demande des ressources.

Un problème
technique

Complexité

- Logiciels

dépendances, licences, interactions.

- Données

représentation, stockage, description.

- Matériel

resources de calcul, architectures, disponibilité.

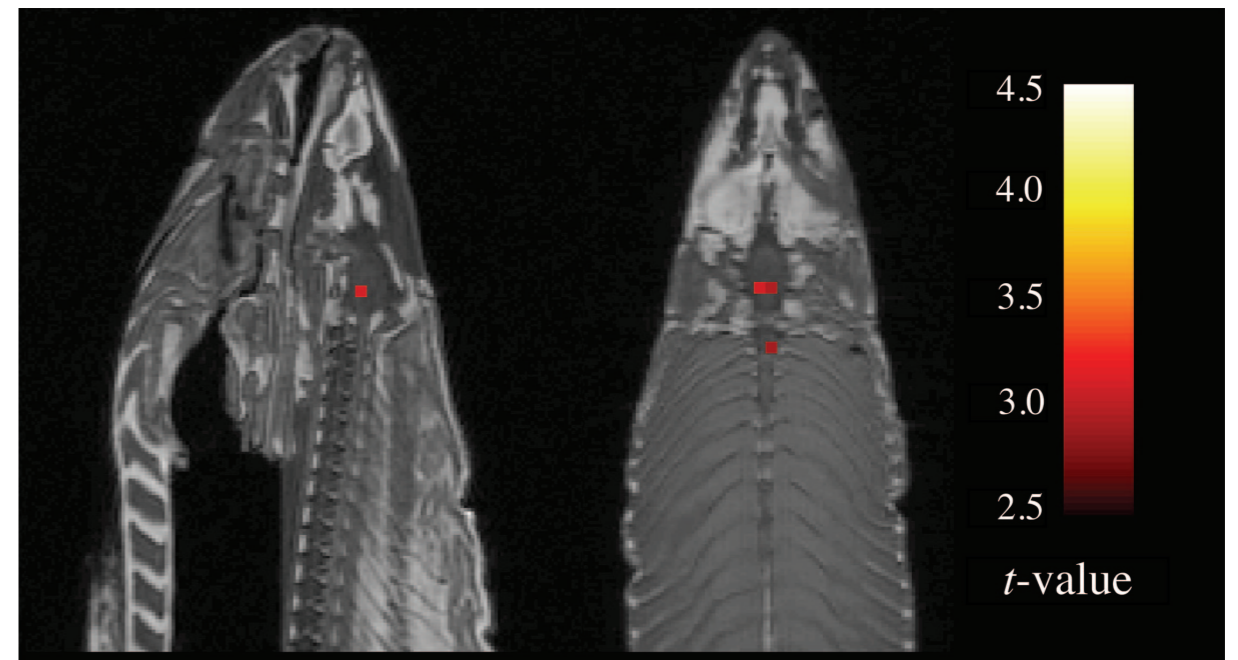


Fig. 1. Sagittal and axial images of significant brain voxels in the task > rest contrast. The parameters for this comparison were $t(131) > 3.15$, $p(\text{uncorrected}) < 0.001$, 3 voxel extent threshold. Two clusters were observed in the salmon central nervous system. One cluster was observed in the medial brain cavity and another was observed in the upper spinal column.[5]

Accès aux artefacts

- Comment donner accès aux artefacts?

Avant publication (double-blind)

Après publication

- Epreuve du temps

e.g., changement d'institution, passage à un autre projet.

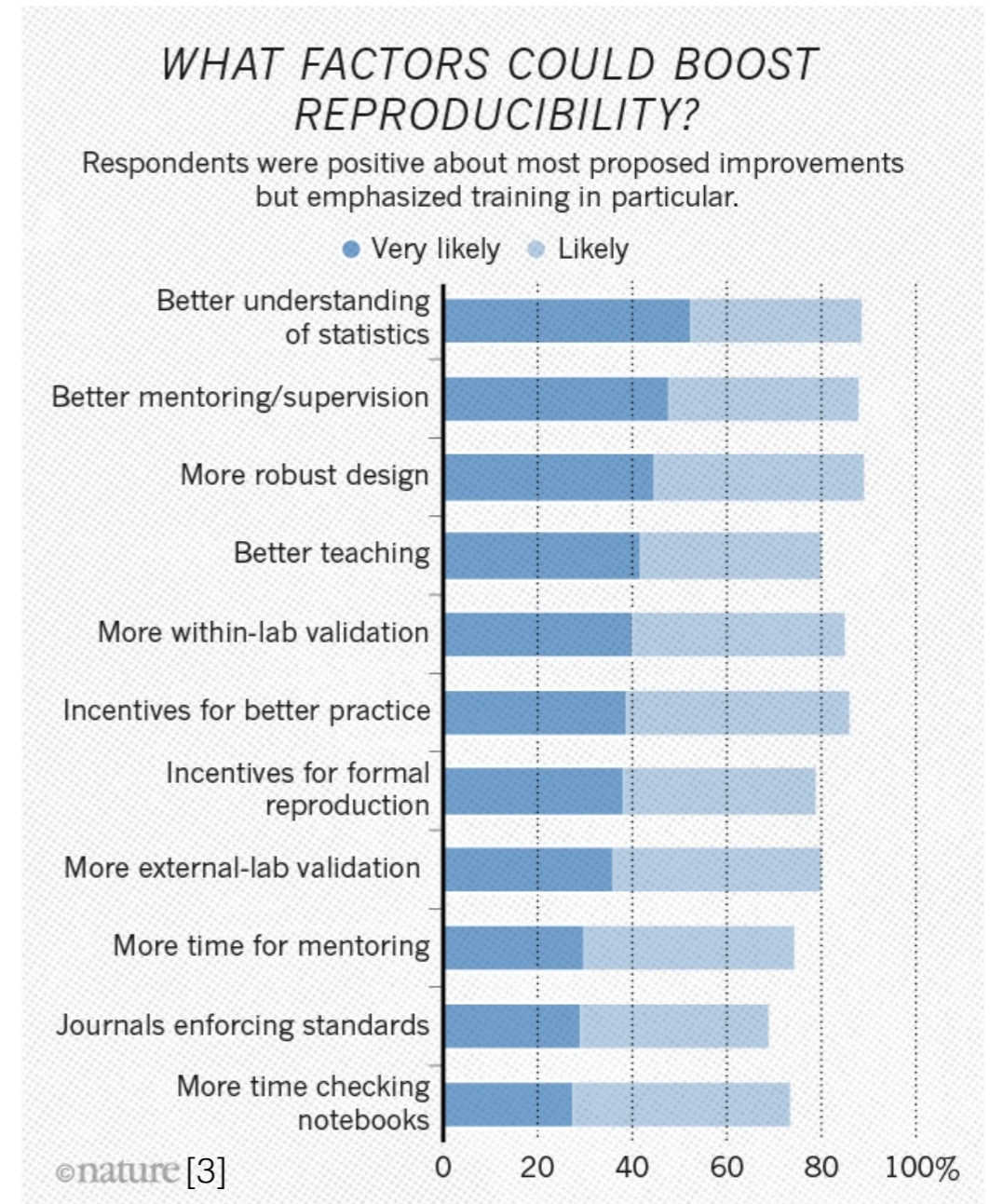
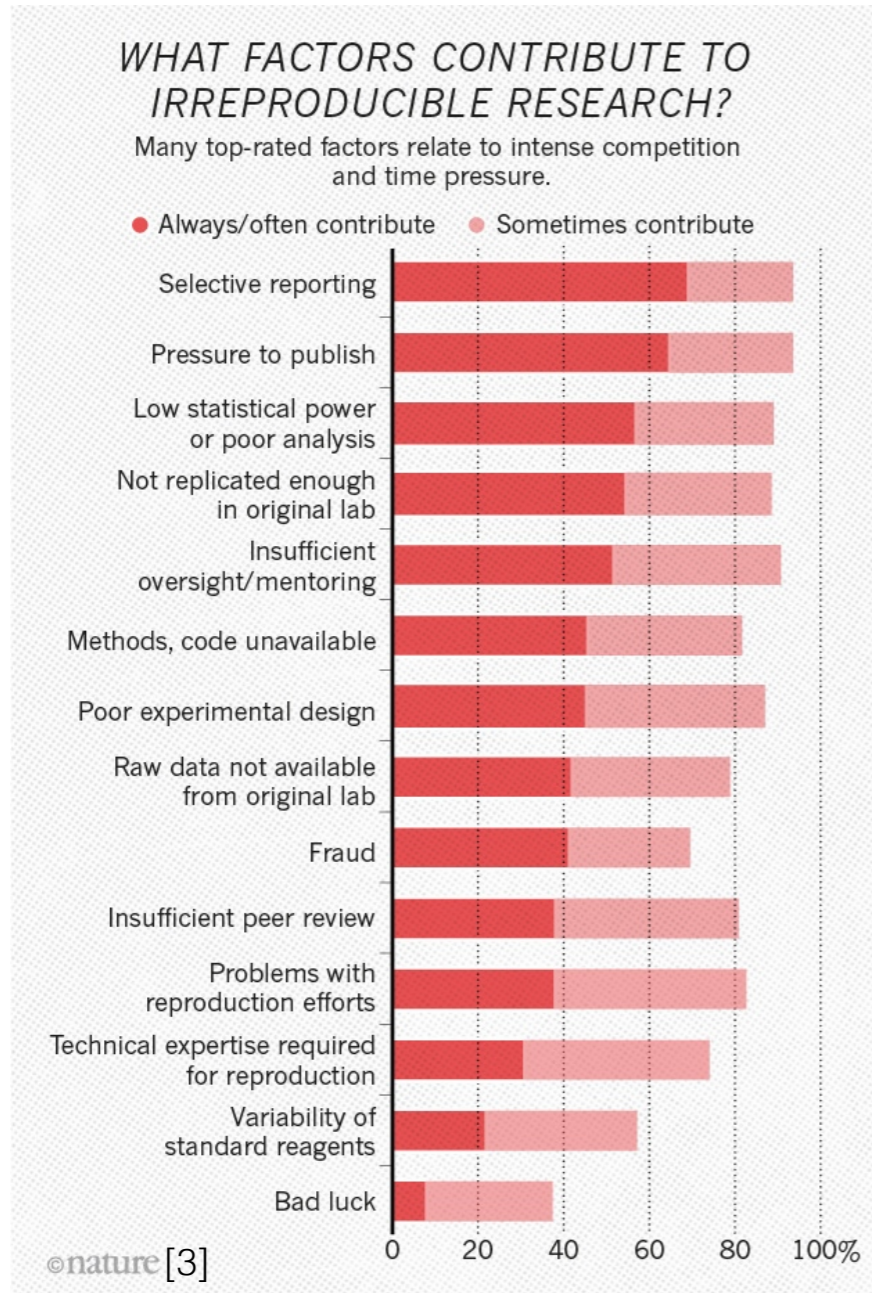
Comment changer la
donne?

“L’enfer c’est les autres” [6]

[3]

[3]

“L'enfer c'est les autres” [6]



[6] Sartre JP. " Huis-clos". 1946.

[3] Baker M. 1,500 scientists lift the lid on reproducibility. Nature News. 2016 May 26;533(7604):452.

Individuellement

Documentation et automatisatisation

- Meticuleusement documenter et instrumenter les expérimentations

e.g., notebooks.

- Automatiser toutes les tâches ET les valider

e.g., éviter les click, favoriser le code et les scripts.

- Assurer un suivi de version

être clair dans les messages de commit.

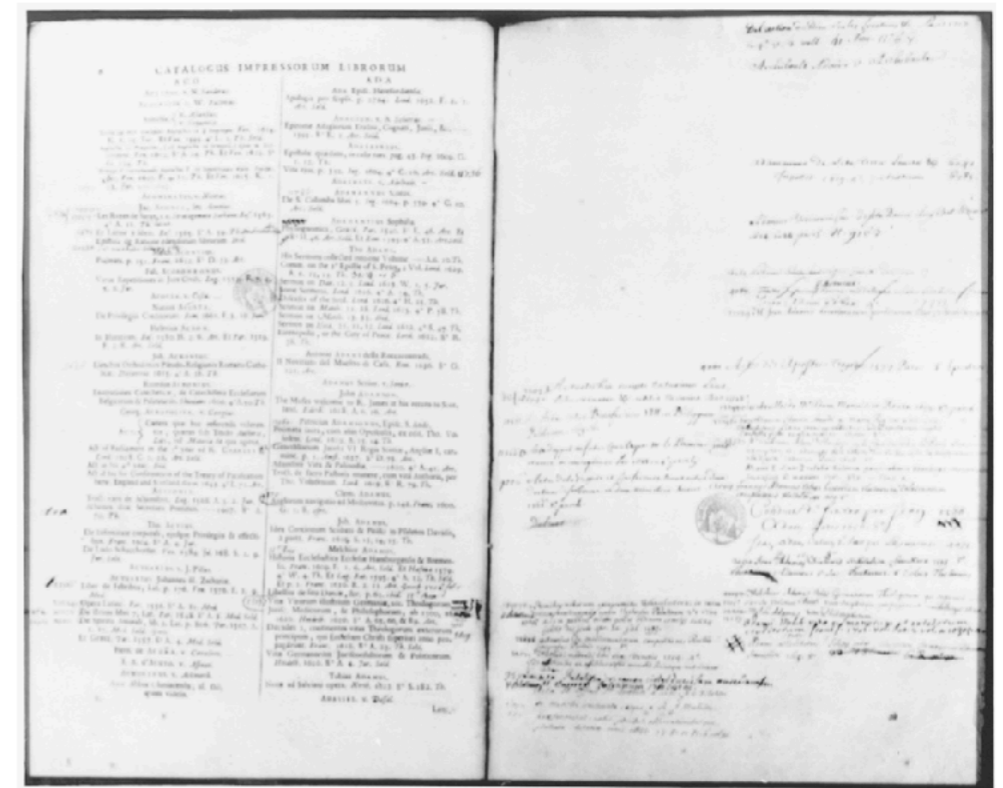


FIGURE 2. Annotations in an interleaved copy of the printed catalog of the Bodleian Library of 1674 are designed to serve as the record of the holdings of the Mazarine Library in Paris in the late seventeenth century. This example of the reuse of another library's cataloging work is comparable to the reuse of electronic library records today. Bibliothèque Mazarine MS 4138–4145, Thomas Hyde, *Catalogus impressorum librorum bibliothecae Bodleiana* (Oxford, 1674), interleaved in 8 volumes. Reproduction by the Bibliothèque Mazarine, Paris. [7]

[7] Blair A. Note taking as an art of transmission. *Critical Inquiry*. 2004 Sep;31(1):85-107.

Partager les artéfacts

- Rendre public les artéfacts

et les documenter clairement.

- Si un artefact n'est pas publiable, le seconder par un artefact public.

Validation

- Mettre en place des techniques de validation des artefacts

e.g., tests de régression, p-value.

- Toujours considérer tous les résultats valides

positifs ou non.

Adapter les reviews

- Discuter la reproductibilité (potentielle) des artefacts dans les reviews

regarder les artefacts.





















Collectivemement

Processus de review

- Prévoir un champ reproductibilité.
- Mettre en place un AEC.
- Fournir une checklist d'évaluation.

SIGPLAN Empirical Evaluation Checklist

This checklist is meant to support informed judgement, not supplant it.

Clearly Stated Claims Example Violations	<input type="checkbox"/>  Claims not explicit Claims must be explicit in order for the reader to assess whether the empirical evaluation supports them. Missing claims cannot possibly be assessed. Claims should also aim to state not just what is achieved but how.	Relevant Metrics Example Violations	<input type="checkbox"/>  Indirect or inappropriate proxy metric Proxy metrics can substitute for direct ones only when the substitution is clearly, explicitly justified. For example, it would be misleading and incorrect to report a reduction in cache misses to claim actual end-to-end performance or energy consumption improvement.
	<input type="checkbox"/>  Claims not appropriately scoped The truth of a claim should clearly follow from the evidence provided. Claims that are not fully supported mislead readers. 'Works for all Java' is over-broad when based on a subset of Java. Other examples are 'works on real hardware' when evaluating only with (un)realistic simulation, and 'automatic process' when requiring human intervention.		<input type="checkbox"/>  Fails to measure all important Effects All important effects should be measured to show the true cost of a system. For example, compiler optimizations may speed up programs at the cost of drastically increasing compile times of large systems, so the compile time should be measured as well as the program speedup. Failure to do so distorts the cost/benefit of the system.
	<input type="checkbox"/>  Fails to acknowledge limitations A paper should acknowledge its limitations to place the scope of its results in context. Stating no limitations at all, or only tangential ones, while omitting the more relevant ones may mislead the reader into drawing overly-strong conclusions. This could hold back efforts to publish future improvements, and may lead researchers down wrong paths.		<input type="checkbox"/>  Insufficient information to repeat Experiments evaluating an idea need to be described in sufficient detail to be repeatable. All parameters (including default values) should be included, as well as all version numbers of software, and full details of hardware platforms. Insufficient information impedes repeatability and comparison of future ideas and can hinder scientific progress.
Suitable Comparison Example Violations	<input type="checkbox"/>  Fails to compare against appropriate baseline Empirical evidence for a claim that a technique/system improves upon the state-of-the-art should include a comparison against an appropriate baseline. The lack of a baseline means empirical evidence lacks context. A 'straw man' baseline that is misrepresented as state-of-the-art is also problematic, as it would inflate apparent benefit.	Appropriate and Clear Experimental Design Example Violations	<input type="checkbox"/>  Unreasonable platform The evaluation should be on a platform that can reasonably be said to match the claims; otherwise, the results of the evaluation will not fully support the claims. For example, a claim that relates to performance on mobile platforms should not have an evaluation performed exclusively on servers.
	<input type="checkbox"/>  Comparison is unfair Comparisons to a competing system should not unfairly disadvantage that system. Doing so would inflate the apparent advantage of the proposed system. For example, it would be unfair to compile the state-of-the-art baseline at -O0 optimization level, while using -O3 for the proposed system.		<input type="checkbox"/>  Ignores key design parameters Key parameters should be explored over a range to evaluate sensitivity to their settings. Examples include the size of the heap when evaluating garbage collection and the size of caches when evaluating a locally optimization. All expected system configurations (e.g., from warmup to steady state) should be considered.
Principled Benchmark Choice Example Violations	<input type="checkbox"/>  Inappropriate suite Evaluations should be conducted using appropriate established benchmarks, where they exist so that claimed results are more likely to generalize. Not doing so may yield results that are not sufficiently general. Established suites should be used in context, e.g. it would be wrong to use a single-threaded suite for studying parallel performance.	Appropriate Presentation of Results Example Violations	<input type="checkbox"/>  Tested on training set When a system aims to be general but was developed with close consideration of specific examples, it is essential that the evaluation explicitly perform cross-validation, so that the system is evaluated on data distinct from the training set. For example, a static analysis should not be exclusively evaluated on programs used to inform its development.
	<input type="checkbox"/>  Unjustified use of non-standard suite(s) The use of standard benchmark suites improves the comparability of results. However, sometimes a non-standard suite, such as one that is subsetted or homegrown, is the better choice. In that case, a rationale, and possible limitations, must be provided to demonstrate why using a standard suite would have been worse.		<input type="checkbox"/>  Misleading summary of results The summary of the results must reflect the full range of their character to avoid misleading the reader. For example, it is not appropriate to summarize speedups of 4%, 6%, 7%, and 49% as 'up to 49%'. Instead, the full distribution of results must be reported.
Adequate Data Analysis Example Violations	<input type="checkbox"/>  Insufficient number of trials Modern systems with non-deterministic performance properties may require many trials (e.g., of a single time measurement) to characterize their behavior adequately. Failure to do so risks treating noise as signal. Similarly, more trials may be needed to get the system into an intended state (e.g., into a steady state that avoids warm-up effects).	<input type="checkbox"/>  Inappropriately truncated axes Graphs provide a visual intuition about a result. A truncated graph (with an axis not including zero) will exaggerate the importance of a difference. 'Zooming' in to the interesting range of an axis can sometimes aid exposition, but should be pointed out explicitly to avoid being misleading.	
	<input type="checkbox"/>  Inappropriate summary statistics Summary statistics such as mean and median can usefully characterize many results. But they should be selected carefully, because each statistic presents an accurate view only under appropriate circumstances. An inappropriate summary may amplify noise or hide an important trend.	<input type="checkbox"/>  Ratios plotted incorrectly Incorrectly plotted ratios badly mislead visual intuition. For example, 2.0 and 0.5 are reciprocals, but their linear distance from 1.0 does not reflect that, so plotting those numbers on a linear scale significantly distorts the result. This misleading effect can be avoided either by using a log scale or by normalizing to the lowest (highest) value.	
	<input type="checkbox"/>  No data distribution reported A measure of variability (e.g., variance, std. deviation, quantiles) and/or confidence intervals is needed to understand the distribution of the data. Reporting just a measure of central tendency (e.g., a mean or median) can mislead the reader, especially when the distribution is bimodal or has significant variance.	<input type="checkbox"/>  Inappropriate level of precision Measurements reported at a proper level of precision reveal relevant information. Under-precise reports may hide such information, and over-precise ones may overstate the accuracy of a measurement and obscure what is relevant. For example, reporting '49.9%' when the experimental error is +/- 1% overstates the level of precision of the result.	

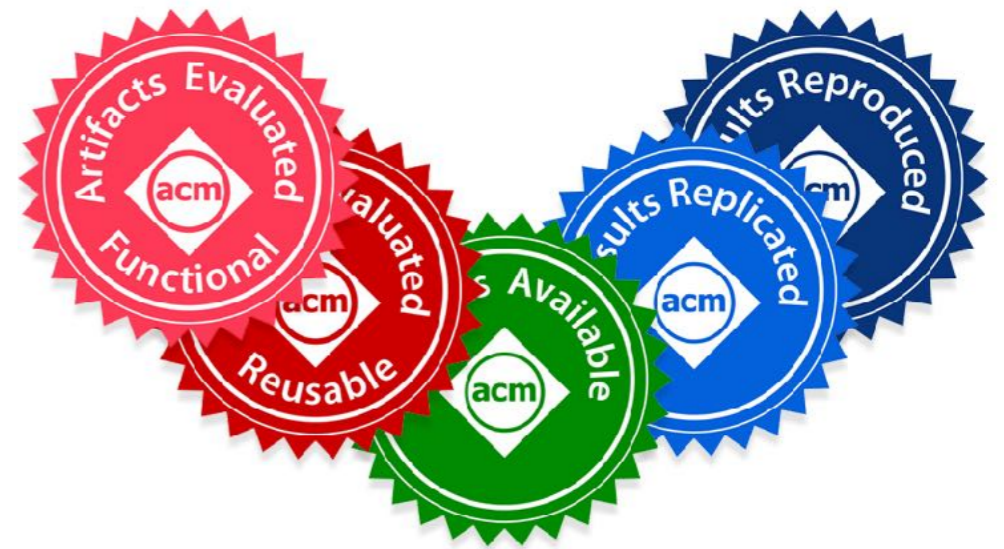
PDF: <http://www.sigplan.org/Resources/EmpiricalEvaluation/>

October 2018. E. D. Berger, S. M. Blackburn, M. Hauswirth, and M. Hicks for the ACM SIGPLAN EC

* <https://www.sigplan.org/Resources/EmpiricalEvaluation/>, 2019-02-25

Mise en avant des qualités

- Evaluer le niveau de reproductibilité des publications
et rendre cette information publique.



“Pre-registered study”

- Publication de pre-rapports définissant
l’objectif de l’étude,
la méthodologie d’obtention des résultats,
la méthodologie d’évaluation des résultats;
 - avant même de connaître les résultats.
- ➔ La publication est garantie indépendamment des résultats.

Inciter la jeune génération

- Favoriser les essais de répliquions plutôt que des études originales lors des cours

e.g., CS 244 Reproducibility Project @ Stanford.

- Faire reproduire des travaux déjà publiés dans le domaine en début de thèse

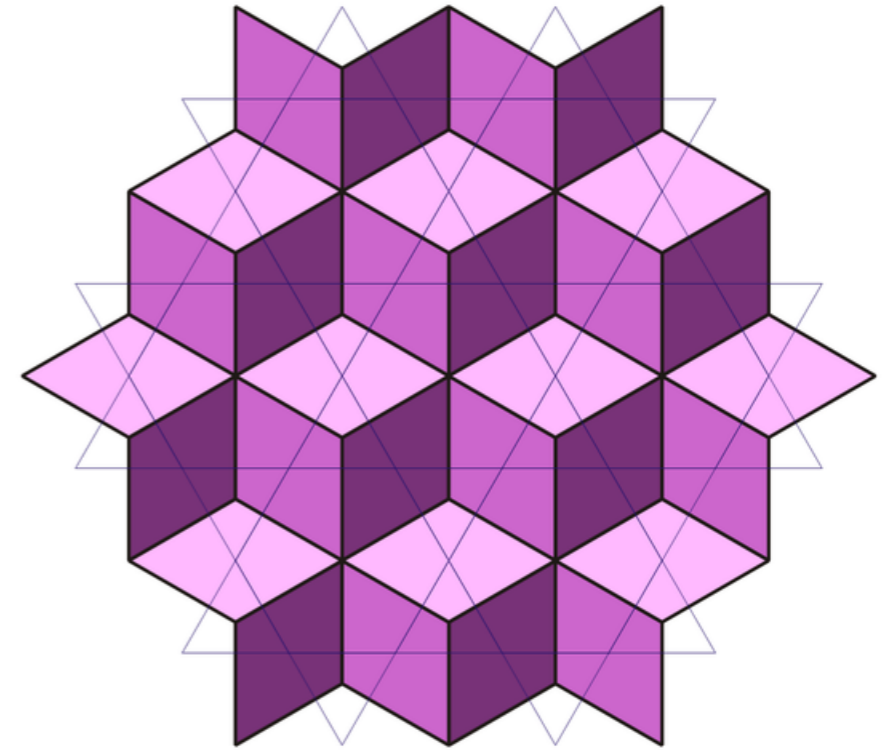
e.g., essai OpenRF [8].

[8] Mahfoudi MN, Turletti T, Parmentelat T, Dabbous W. Lessons learned while trying to reproduce the openrf experiment. In Proceedings of the Reproducibility Workshop 2017 Aug 11 (pp. 21-23). ACM.

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Une rencontre informelle
autour d'un sujet scientifique.



Notre science peut-elle être reproductible ?

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