A Questionnaire for Evaluating Editors

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Abstract: Research evaluation in journals, and all ethical issues surrounding it, are the subject of an extensive debate. We argue that disclosure/non-disclosure policies, partially following recent "open science" trends, bear elemental deficiencies, so that journals must consider alternative accountability procedures. We also motivate that paying reviewers is likely a long-term unavoidable step in many fields to improve review quality, as is restricting the reception of submissions by journals, and limiting the use of short-hand editorial decisions. This particularly applies to the area of mathematics, where expert communities are tight and review is often difficult and prolonged.
To oversee journals' operation, the concept of a database is developed in which authors, and reviewers, can rate submission cases. It leans a lot on the principle of teaching evaluations, although some results must be public, also in order to provide guidance to prospectively submitting authors. We emphasize further aspects/advantages of this facility, specifically how it must differ/improve from previous provisions, and its function in cases of scientific misconduct in publishing. We finish with a proposed database questionnaire.

Keywords: peer review, academic integrity, science publishing, teaching evaluations, responsibility

1. Introduction

The editorial process of journals has been discussed at many places (Ali and Watson 2016, Lim et al. 2019, Horta and Jung 2024). One of the fundamental principles on which this system operates is that authors cannot – unlike employment applications, for instance – submit the same manuscript simultaneously to multiple journals -- and pick the best (acceptance) offer they can get. The main argument for journals' exclusive manuscript consideration is their burden of (detailed) review. This remains an author's basic right, and the question is how a journal can satisfy its duty? This becomes especially relevant as journals deal with increasing manuscript inflow (see e.g. Hanson et al. 2024).

One major source of problems in journal management is that, while review serves as the author's scorecard, an analogous provision for editors and reviewers is lacking. If such an accountability process is not outright neglected, at least attempts to establish it often interfere with review itself.

This has brought up a number of questionable (if not much worse) practices of editors and reviewers (see Magness 2019, Stoimenov 2023, Li and Zhang 2025, among many others).

Various proposals have been made to improve functioning of journals (see e.g. Tennant 2019, Cooke et al. 2024). However, as will be argued, many of them bear deficiencies which protrude especially in my area of mathematics, characterized with both narrow circuits of potential reviewers of a given submission, and large effort of (detailed) review. This applies among others for certain "quick opinion" measures, which are not an equal substitute for review and have other unfavorable consequences.

As part of better oversight principles on journals' operation, an idea was initiated (Stoimenov 2023) to organize a database in which authors, and reviewers, can rate submission cases (openly relating to a given journal and editor, but with author and manuscript title withheld). Similarly to teaching evaluations, they would be asked their opinion about various aspects of the editorial process, and average scores of an editor will be displayed (after a number of records is reached). Together with proper support of review, this may provide a more sustainable approach to various issues raised by, and occurring with, editors (particularly, but not only, in mathematics), as outlined below.

Specifically, we relate to backlog (§2), blinding/disclosure (§3), adjusting journals' finances with regard to paying reviewers (§4), opinions expressed by reviewers (§5), and how the database, as distinguished from alternative web portals, serves in handling those of authors (§6). In more length we discuss (§7) how to act when concerns about correctness of a journal's publications are raised. In this situation, Stoimenov (2023) also proposed an appeals commission for enforcing journals to correct published errors. We detail what is expected from the journal, what are the responsibilities of the database in evaluating whether the journal's duties were observed, and how to divide functionality with the appeals panel. After some short review about the role, relevance, and prospective other analogues of the proposed database (§8), we summarize for convenience its queries considered here and previously, obtaining some preliminary version of a suggested questionnaire (§9).

2. Backlog

One common reference in rejecting papers, especially in mathematics, is backlog. (For some statistical information, see Björk and Solomon 2013.) Some journals in other areas restrict the number of submissions per author per year. The effect of this constraint may not be that strong, since journals seek to receive submissions of a multitude of authors. But temporarily stopping receiving submissions is an imperative measure to be considered. And I know at least one very respectable mathematics journal which has done this in the past. See also Flaherty (2018).

Such suspension may put some authors "out of the habit" of submitting to the journal. This is why it should probably not be done over very long periods. But acute backlog usually means that journals have worthwhile material to publish for (a few) years ahead. Then receiving nothing for a few weeks or months cannot be such a big burden.

Moreover, under backlog acceptance rate would be very low. Allowing for these extra submitting authors, the majority would be compellingly (and probably brusquely) sent away. This experience may motivate most of them to shift attention to alternative journals more than if they had not been al-

lowed to submit at the first place. Thus, in deciding its reaction, the journal would suggestively start reckoning factors like how *promising* is the author to contribute something useful in the future.

A standard theme, which is insidious in dealing with less established scientists, was quoted in Stoimenov (2023, §2, p.5) in the form, from an editor's perspective, that "if his papers are good, some journal would publish his papers". This is one pattern of thinking worthy of recalling Doty's (1991) dire warning that "we risk sliding down toward the standards of some other professions where the validity of action is decided by whether one can get away with it".

This insight protrudes a common perception towards lesser known (and lesser supported) authors. But even if an author has a publishing record in the journal, this does, unfortunately, not mean that the quality of his/her contributions would not decrease. Predictions, regarding virtues like "promise" or "talent", are always a gamble, which becomes increasingly less fail-safe when the choices narrow. I know one extremely renowned mathematician who is said to follow the rule "Once they decline something, do not send there again."

Ultimately, a journal must realize that, especially under the exclusive consideration principle in use currently (and in any foreseeable future), it cannot – and cannot strive to – publish every paper, or every author, that/whom it feels attracted to. This applies especially when the burden on everyone else is increasing. By any means, it certainly seems the authors' plausible right to (officially) say something about their work's reception. And the palpable yardstick to set to them is the journal's recent output (*cf.* §9), rather than its speculative future assets. Without an effective path in for others' opinions, the line between (alleged) quality concern and self-indulgence becomes extremely fine and easy to cross by the editors, even if unconsciously.

In addition, it is welcome if all backlog restriction statements can be made on the submission portal. Email is, in this situation, highly undesirable, since it is a private medium and allows editors to narrate the same matter differently to different people. This can only open the "back door" for preferential treatment of a select few (*cf.* Stoimenov 2023, §5.1).

3. Blinding/disclosure policies, and alternatives

The traditional single-blinding model (author known to the reviewer, but not vice versa) has been challenged in recent years, and changes have been considered by (a) anonymizing authors to reviewers (double-blind peer review; see Ali and Watson 2016, Watve 2023) or (b) publicly naming reviewers (open review; see Haffar et al. 2019, Li and Zhang 2025). Here I reinforce the limitations of either option towards improving quality of review, especially in mathematics. (Post-publication review performs a rather weak selective role, and will not be deemed appropriate by the vast majority of journals, far beyond mathematics as well.)

(a) Particularly with the internet (see, e.g., Sun et al. 2024), and in tight circuits (of which there are many in mathematics), anonymity of authors is a tenuous protection against identification – and, if so intended, inadequate treatment – by reviewers. Avoiding recognizability through preprint servers, records (and even more recordings) of related talks on conference websites, and so on, completely goes against many authors' natural intent to promote their research.

(b) Reversely, “open science” has brought up, in other fields, various policies of public disclosure of review and/or reviewer information depending on additional criteria such as what the reviewer opts for, the editorial decision, and so on (see Haffar et al. 2019, Wolfram et al. 2020). Whether a reviewer deems these conditions favorable or not, they trigger considerations that draw him/her away from (objective) professional scrutiny.

Self-promotion (*cf.* Stoimenov 2025, p. 743 top) is one possible distraction, and verboseness (*cf.* Bornmann et al. 2012, Maddi and Miotti 2024) can also be a protrusion of such distorted attitude. That details about unsuccessful submissions cannot be (uniformly) disclosed poses a greater obstacle than many proponents of such procedures believe (or admit). Making reviews into a creditable asset (even for a CV?) may motivate reviewers to support submissions against journals’ quality restrictions. A journal approaches a reviewer expecting his/her actions to primarily serve the journal -- definitely not him/herself, but neither the author. There is a number of high-standard journals (not only) in mathematics, which can legitly demand far better from an author than to be helped along by the reviewer. Further, can the editor deny the reviewer the “deserved” publicity if disagreeing with to-be-displayed content – even at the cost of influencing the decision to the author? Such degenerate scenarios become, unfortunately, very imaginable...

And unconditional (or unconsensual) disclosures remain inherently risk-prone in conflict situations. Assume, e.g., a less established open reviewer is handed some poor paper submitted to a high-quality journal. This reviewer, fearing career problems from a famous author, may feel coerced to participate in the decline of quality of the journal he/she reviews for. Or he/she may raise concerns about the manuscript, but be overruled by the editor, and later get framed. We may need then also, for instance, “full transparency” (Laxdal and Haugen 2024, p.2 left) into all hiring committees considering this reviewer's applications. As suggests Stoimenov (2022, p.592 bottom; or see Lang 1993, §I.1, §V.2 end), "guilty unless proven otherwise beyond reasonable doubt" appears a standard operating directive there as well.

In short, disclosure experiments are deficient as a review quality/accountability mechanism. In certain branches of science, they may have some advantages. But as the impact factors have taught us (see Stoimenov 2023, §3.3), spreading a concept that *somewhat* works at one place can turn out *extraordinarily* detrimental at another.

Instead, Stoimenov suggested (2023) journals to focus on careful *internal* evaluation procedures. Thus, the journal should maintain extensive (but unreleased) records about every submission and what every editor, reviewer, and author thought about everybody else. Over time such records will give indications which editors and reviewers are worth it (possibly incl. payment; see below).

For instance, an author is not impartial, and an editor often insufficiently expertized – but a co-reviewer is the ideal party to judge the quality of a review. (This co-reviewer must, of course, be mutually anonymous with, and independent from, the evaluated reviewer, i.e., unlike in the team-work project contemplated by Mehmani 2019. Also, Stoimenov 2025 favors reviewer evaluation being done post-decision.) Thus a rule of thumb for a reviewer would be “Write a review so that your co-reviewer likes it”. Alternatively, one can use a “meta-reviewer”, who reviews all reviews. (I had this experience once as an author prior to the editorial decision; it was also suggested by Li and Zhang 2025.)

4. Three salaries for open access, or: how to finance review?

Many scientists support the benign mindset of review as a community service. But it bears its problems, too. While paid review (see, e.g., Lortie 2011, Laxdal and Haugen 2024, Beecher and Wang 2025) raises (valid) concerns, many reciprocate to unpaid one: how much effort should one invest when doing the work voluntarily? What is a fair -- and working -- reward principle?

Indeed, for papers dealing with social and managemental issues, it is possible to receive very competent reviews returned within "two to four weeks" (Lim et al. 2019, p.182). But in mathematics, "weeks" can be easily replaced with "months" or even "years". (My current "record holders" are 3 years for a rejected paper and 3½ years for an accepted one; *cf.* also Notices 2010.) In addition, the amount of material the reviewer has to grasp during a review is not proportional to what he/she can use therefrom in his/her own research. This can make review not only a daunting, but also scientifically an insufficiently rewarding experience. (*Cf.* Horta and Jung 2024, §3.)

The legitimate question is then how to cover the review costs. It is (sadly) well known that the large commercial publishers capitalize on the reputation of journals to drain money out of science -- money that, for instance, could be used to pay reviewers. (See, e.g., Jackson 2007; Scarcella 2024; Beecher and Wang 2025, p.839.) Thus pressuring these publishers to provide for such costs seems reasonable. Evidence like the "three salaries" OA charge (Stoimenov 2025) raises huge doubts in appreciating review as a charitable activity. (See also Aczel et al. 2021.) In particular, resistance to innovation on incentivizing review can be increasingly taken up by certain reviewers as a (self-)excuse for mediocre effort. And this is certainly not a development to support either.

The question then translates to (low-cost) academic publishers. They may be compelled to price hikes which libraries are sensitive about. How significant these price hikes would be depends on how generous such journals would like to be to their reviewers. Even if this way the price rises somewhat above what librarians (and editors) would be happy to see, this is likely still far below a commercial publisher's rate. The reader may find in Stoimenov (2022, p.595) examples of how a low budget impedes a journal's basic functionality. Indiscriminately cutting costs cannot be a path to quality, and every reasonable reader must comprehend this.

To sum up, whether subscriptions are an agreeable cause or not, economizing on review gradually transforms it from an author's prerogative into some type of a privilege. And editors largely tolerating whatever they can receive as reviews is, among other problems, exploitable by reviewers, and contributes nothing to the quality of their work. And yet, this so-pronounced "moneyless" policing has financial implications: authors apply for jobs and grants and so on (see further §5 and Flaherty 2018).

Alternatively, charging authors can be considered (see Dickey 2019, p.11). But given author payment models (OA, page charges, etc.) already in place, the question should reduce to academic publishers and to whether to charge authors for every submitted paper, not every accepted one. Paying for excessive submission (beyond a limit of manuscripts per author per period) is one option. If journals insist that authors pay regardless of limitations (and editorial decision), then it is very natural that authors demand a right to actively participate in evaluating the service they received. The database could thus indeed play an active role (also) paving one way out of the review-funding slump, especially for low-cost publishers: they can start attracting authors' financing, saying "you can rate us". With this opportunity, such payment modus would become far more convincing.

Of course, author-paid submission should not restrict journals' right to dismiss material they deem obviously improper. But, at the opposite end, most authors would not contemplate payment when they do not assume that their work at least merits a serious consideration. Submission fees may thus motivate editors to tone down the perfunctory (and often templated) statements (Stoimenov 2023, §3.2) leaving their reasoning largely to the author's imagination. Apart from its evaluative lapses, the system is also inherently lacking in mitigative provisions like matching manuscripts to mutually agreeable venues (Stoimenov 2023, §3.4 top). For instance, when the journal was put to the author's attention by the publisher himself in a transfer option, then do the editors (pretend to) believe that the author did not know well where he was submitting to? Authors then perceive the process rather as a bureaucratic hurdle to overcome than as a constructive direction (*cf.* also Dickey 2019, p.8).

Ultimately, paid review is an option, not a compulsion. Similarly to the various author payment models, it is perfectly fine if paid and unpaid review coexist within the same field (or even within the same journal; see Stoimenov 2025). However, especially if a reviewer struggles to make a living, he/she may of course choose a reviewing offer that rewards his/her effort (best).

There are diverse criteria that can be reasonably taken into account when devising a payment scheme. (It is highly recommended that this information remains journal-internal, also to thwart certain manipulatory businesses; see below.) In particular, payment should increase with volume/quality of performance, and probably could be informed within certain limits to the reviewer in advance. (Range within these limits could depend on how the review itself is evaluated; see previous section.) Standardizing overall is difficult, but it can regulate itself by choice-and-competition, similarly to journal pricing. Subscriptions still work, despite that there is no global agreement between different publishers, even less taking into account criteria such as what is the perceived scientific level of the journal (or poor numerical indices thereof; see, e.g., Brembs 2018), etc.

Then, certain reviewers' efforts may be concentrated around (better) paying journals. And journals that pay may have some advantage in ensuring review quality. Some similar situation occurs not only in science, without anything abnormal with it. Certainly, free-review journals cannot demonetize the (fairly money-heavy) science publication business altogether, thus blaming review-paying ones for doing something immoral really seems out of line. Teaching instructors receive a salary and, along with reviewers, editors definitely can – and often *also do* -- fund themselves as much they deem necessary to keep up a good work.

And there is one more point to keep in mind: it is very easy to design reviewer payment completely independent from the editorial decision (*cf.* §3, (b)). No matter how strongly criticized, such a reward policy will always retain this significant advantage over any closure/disclosure model. (See Zaharie and Seeber 2018 for the deficiencies of alternative bonuses.)

Of course, there are some potential reverse consequences of payment. For instance, there are "mill factories" (Else 2022, Skórzewska-Amberg 2022), which charge authors to find them a journal to publish in, and to some extent "write" (meaningless) papers on their behalf. Similarly, journals may then start getting offered by some companies a (bulk) supply of "cheaper" reviewers (see Moustafa 2022). But this is part of the now omnipresent "background noise" that reasonable scientists (who are still supposed to run any serious journal, instead of accountants or the like) should be able to safely sort out. Certainly, editors must grow increasingly vigilant whom they trust. But with reviewers fully at their own designation (and, of course, own responsibility), they can be content as long as

the communication between the two parties remains reliable itself. And this is a global (cyber-security) concern not restricted to the profession.

To address the issue of cronyism (Moustafa 2022), review expenditure should be managed by the entire board. But such a problem is, again, far from journal-specific. For better or for worse, one can hardly externally prescribe to someone his/her spending preferences. One can, though, expect work properly done, and this is what the database aims at regulating.

5. Reviewer opinions

Quoting to the author what the reviewer said may be perfectly fine, especially if independent reviewers agree. But a reviewer is always an assistance to the editor, and should not be sought by the editor as an excuse for his/her actions. Keeping the reviewer unidentifiable leaves editors with virtually unrestricted discretion at a reviewer's choice. The reviewer could be the editor themselves, or at least someone familiar enough to reckon how they would react to a particular submission. Again, the database is supposed to allow authors to say how well they think review was organized by the editor (with the premise that they are entitled to be informed sufficient detail; Stoimenov 2025, p.746).

However, instead of making efforts, for instance through payment, to ensure better quality of review, many journals now believe that certain "short-hand" (d)evaluation must be increasingly practiced (incl. by electronic "minds" such as "plagiarism checkers"; see Hosseini and Resnik 2024) to cut an unrestricted inflow of submissions. This *modus operandi* is a double-edged sword, for several reasons.

Journals cannot keep internal actions top-secret -- or control what speaks around and whether it does the authors justice. (What happened with whose submission at which journal is one popular rubric for places like conference breaks.) This means that every additional decision process, no matter how uninvolved, can disadvantage authors beyond the standard amount of frustration and seeking other places to submit. Of course, journals are notorious to care little about authors they send off. But more importantly, making effortless judgments into a routine experience for many can undermine (especially voluntary, and in-depth) reviewing commitment, and along with it skill, in a state where editors already face difficulties finding reviewers (*cf.* Flaherty 2018; Hanson et al. 2024, §1; Beecher and Wang 2025). In effect, this "quick opinion" trend does not short-cut, but short-circuits the review activity.

Ultimately, whatever is deemed a reasonably elaborate opinion, there seems nothing wrong to have its exchange more properly controlled through the database. (In general, the standard option "N/A" should be sufficient to accommodate for an author/reviewer feeling provided too little information regarding a specific question.) This applies the more that the flow of criticism in review is far from uniform. Due to many factors (qualification, political stature, etc.) it is common that the same scientists are regularly requested opinion on the same others, but far less often vice versa. This iteration can allow expression style to slip all the way down to "copy-paste" derogation (see Stoimenov 2022), and tensions will build up which, in last instance, the database can help mitigating.

Especially (but of course not only) in mathematics, an editor-reviewer trust relationship (*cf.* end of §4, and also Lim et al. 2019, p.187) is often very close. An editor typically works with a rigid handful of reviewers. The fact that "good" reviewers are, to many editors, in shorter supply than teaching

instructors, does not mean, though, that no control is warranted. To the reverse, how to ascertain that reviewers do not leverage their position to their advantage?

In smaller subject areas, the editor may know only one reviewer, who then acquires extensive decision power over what the journal publishes in that area. But this reviewer's motives may not always be genuine. The editor (possibly at part wishfully) thinks that this is a "good" reviewer, because he/she has worked with that reviewer for a long time, and maybe because it is difficult and/or bothersome to seek an alternative. (Such liaisons have led to the jargon "old-boys networks"; *cf.* Dickey 2019, p.8 l.9; Horta and Jung 2024, p.6 l.17.) But this reviewer also deals with a growing group of authors. It is understandable that a journal must treat reviewers and authors with different priority. But currently a generic editorial office does not deem authors' opinions worth hearing at all! And if such an internal channel does not exist (or does not function), then the database can be used. Generally, no matter how highly esteemed, everyone must be kept accustomed to critical feedback.

For instance, instead of leaving an author's displeased opinion to the public, an engaged journal could extract insight from internally consulting his/her responses (and text comments) to inquiries like those of §9. (Similar could apply for reviewers' mutual evaluation; §3.) This can be, in fact, encouraged through the database itself, explaining its question about journals' self-improvement efforts. It can be further reinforced by (slightly) rating up more recent database records. Of course, it is solely the journal's responsibility to build up authors/reviewers' trust to provide information to help its self-maintenance.

However, be aware that identifying problems with reviewers *by name* (like in an "opposed reviewer" provision; see Stoimenov 2022, p.594 bottom) bears hazards for the author. Editors' disregard (or, worse, abuse) of author-reviewer conflicts warrants its own question in §9. Note also that such practices can only very tenuously be linked to correctness (see §7, and also Resnik 2019). Unlike exceptions like the monograph case (Stoimenov 2022), far more commonly they concern new research (which, if rejected, the journal is clearly not responsible for). This is another reason the conflict inquiry to be clearly separated from a "misconduct" probe.

6. Author opinions

The concern is suggestive that the database can be easily (ab)used to pile up complaints and attacks on editors, probably with little or no argumentation. However, there are several reasons why a potential overtly grave outcome should less question the examination process, but rather manifests underlying causes that have been left to aggravate for too long.

Academic platforms such as Peeref (2019) allow publicly writing about submission experiences, but there is no stricter accounting how many submissions, proof of submissions, what are evaluation criteria, etc. (Of course, Peeref enables other useful features, like rating published articles themselves – similar to what was proposed by Watve 2023, but on a journal-independent basis.)

SciRev's (2013) evaluation procedure remotely relates to ours (§9). But instead of statistical details to survey (Huisman and Smits 2017), we must focus on examining actions necessary/fair for an editor/reviewer to have performed, and time is one relevant parameter among several (Stoimenov 2023, §5.1).

Sped-up "review" (§5) somewhat diffuses the concept of "desk rejection", and an author is often not unambiguously informed/let to conclude at what step the decision (and/or a delay) occurred. More precise (among others) timing information may also incite editors' inferences about the author's identity (and, consequently, cautious authors' defensively misleading input). What the database projects instead is an overall claim "The editorial handling/review process was timely", which can be responded to (like any other query) by some choice ranging between "strongly agree" and "strongly disagree". Still both parts can be separate questions: the author should be allowed to track/obtain from the editor information about reviewer interaction periods (see Stoimenov 2023, p.14).

Moreover, when considering accountability, the editorial decision is largely misleading, and better be left out completely: it mostly biases a record away from the basic principle that the duty of the journal towards a submitting author is uniform – and hence should also be uniformly evaluable. In analogy, individual class evaluation records, if accessible, are usually displayed without the student's grade: effects like the instructor paying less attention to complaints of weaker students are deemed unfavorable.

Here, (visibly) admitting unsuccess, especially if felt disagreeable, will always rouse the author's repulsion, however unidentifiable he/she remains. It can also trip up condoning certain subsurface editorial actions which, as the above remarks regarding the quote in §2 show, are generally unreliable (and unverifiable). Another advantage of detaching from the final outcome is that the submission confirmation letter is a simple uniform validation of case input (Stoimenov 2025; compare with Huisman and Smits 2017, p.639). Contrarily, a question whether *the author thinks* that the editor properly justified his/her decision (certainly with the bail-out "N/A" option offered) is worth considering for the database (see §9).

What really matters is the author's opinion on various aspects of review provision. And again, the ratings better reflect the review process as a whole: this is the editor's duty, that the database should inquire about. It is thus not essential to score separate reviewers, separate review rounds, and so on. (Such details can be left to the author's optional text comments, if enabled.) Likewise, the reviewer's comments measure his/her performance much better than the author's revision effort. An instructor is requested to consider students' level, because they are a locally confined group, and class content is rarely cutting-edge innovative. However, the author's deficiencies are not any reviewer's responsibility – although his/her credit if discovered.

The possibility that the editor withholds reviewers' comments addressed for the author is one further situation motivating the proposal (Stoimenov 2025) reviewers also to be included in evaluating editors. A sample query would be "Are you convinced that your comments were duly forwarded by the editor to the author?" If the reviewer saw them being addressed, then probably so, but otherwise? Review reveals an increasingly precious resource to suffer from editors' negligence, or worse, to be tampered with by certain editors' agendas.

Generally, any information about the journal entered by the editors can be biasing, and should be disabled. This includes reviewer records (if editors review a submission themselves), but also author records. Even if (at any serious journal) editors are excluded from considering own submitted work, one can hardly assume their treatment (or their reporting about it) will be neutral.

Moreover, for instance, if the journal raises its desk rejection rate exceedingly, the database manifestly aims to allow a proportionate number of authors to report review provision as insufficient.

They could use “N/A” (on review-related queries), but also explicitly express dissatisfaction – it is entirely up to them. Paper-shredding is a(n unpleasant) bi-product of a journal’s aspirations and limitations, but should not be turned into an intrinsic purpose.

A further issue with web platforms is that more of them makes each one less unequivocal. A natural editors' posture is "Do we need to care about what people write about us on [...]?" Overcoming this barrier must become one of the database’s fundamental objectives.

7. Corrections and Integrity

The issue of corrections has preeminent importance, because there scientific responsibilities (as discussed by Lang 1993 at length) and administrative ones (as primarily dealt with by the database) meet head-on. There are many reasons why journals block corrections (see Lang *ibid.*, §I.3). Despite some findings (Strothmann 2018) that corrections can actually increase citation, they are often perceived as unattractive, and some authors and editors resolutely battle them from publication (see Lang *ibid.*, §I.1; Trebino 2009).

When reporting about a journal rejecting corrections, the reservation is reiterated that one does not know how often such cases occur, with whom, etc. However, motivating doubt on action against such behavior, this type of rhetoric is highly worrisome. (As a “pioneering” of this thinking, see the -- meanwhile notorious – editorial by Koshland 1987, as quoted by Lang 1993, §V.5.) Correctness of published science is a fundamental tenet that nothing should touch on (Doty 1991, Nature 1991). In addition, there are many factors (see Lang 2000, §I.5; Rothwell and Baldwin 2006) that hinder the proper report of such incidents. In social life, no one (decent) nowadays surveys crime rates to decide on law enforcement, or sees in weaknesses of law enforcement a justification of criminality.

That said, not every provision is suited to perform such a task in science, and the database has indeed limited capacity. My paper (Stoimenov 2023, §5.3) contains another suggestion in this regard, of an “appeals commission”. Nevertheless, it is in the spirit of the database to judge (for itself) the journal’s actions, and for this a board of scientific advisers was suggested. For instance, that the author has left academia, is struggling with career, is seriously ill, or deceased, all of this explains as excusably difficult (or impossible) to expect much corrective cooperation. But a journal is a community enterprise, among others, also precisely in order to withstand relatively unharmed such individual hardships. There is every reason to expect substantial responsibility, and responsiveness, from the journal when issues are raised with material it has published (see also Edsall 1994, p.241 bottom).

This suggests that even a severe criterion should be considered for the case-evaluation questionnaire like “Do you think that the editorial process involved misconduct?” This query should be granted to reviewers as well, for instance, if the editor pushes through faulty research (*cf.* §3, (b)). But if the answer is “yes”, the case-filing author/reviewer will be demanded further evidence. This also raises the dilemma that the journal’s side must be heard – here, since this is too serious a charge to outright go along with the (potential) whistle-blower – while still keeping the latter concealed.

A solution is to ask the journal for bulk internal documentation that it wishes to provide about a large number/long period of its submissions entered into the database. (Such queries can also be used to spot fake records; *cf.*, e.g., Skórzewska-Amberg 2022.) Of course, the journal may not coop-

erate, but then the database advisers rule by the evidence at hand. No further repetative or detailed explanations or inquiries regarding misconduct reports should be made to the journal: it can be assumed tidy internal book-keeping, and as usual in a conflict situation, it takes much much lesser risks than any individual who decided to put forward (in this case to the database) such charges (*cf.* Edsall's self-quotation 1994, p.240).

Furthermore, in the same way in which an editor or reviewer rarely embraces writing guidance to the author (*cf.* §3, (b)), the database advisers are not in the position to debate on integrity principles with the editor. Many of such cases occur in rather high-profile journals. World-leading standards of editorship imply world-leading standards of editorial integrity. And as pointed out by Lang (1993, Introduction), judging the way one responds to criticism can be done with far less expert knowledge than judging (the scientific content of) the criticism itself.

In this sense, evaluation of corrections is not the advisers' main focus – similarly to other journal submissions entered into the database (§6), but *unlike* the intended function of the aforementioned appeals commission (Stoimenov 2023, §5.3). This also explains why it is rather redundant in general to submit manuscripts to the database when filing cases – unless responding to some explicit (but presumably very rare) direct request by the advisers.

An example of objectionable behavior is when an editor enforces onto a correcting author that only original authors can write their own errata (see Stoimenov 2022). This is indeed a custom, but its weaponizing "cooperates" with complacent authors suppressing the discussion of their published flaws.

Another archetype of editorial handling usual with original research but unacceptable with a correction is that it is reviewed anonymously to the author submitting it and rejected based on that review. Similarly to the previous case, such an author is forced into the position to, even if potentially, stand by someone covering up his own (or his best friend's) official falsehoods. As outlined at the beginning of §5, an editor has a great deal of discretion to direct review making his/her favorable outcome more likely. But corrections are not there to fit or misfit his/her, or anybody else's, personal taste. They have a special role in the fundamental principle of opinion exchange about validity of science (see Lang *ibid.*, §II, or Edsall 1994, p.241 bottom).

One suggestive option that closely observes this principle is if journals have temporary posting boards for submitted corrections, where everyone, including perhaps directly invited experts, can give an opinion. (Leaving such content to be discussed *somewhere* on the internet is not an attitude. But an organized debate will also discourage seeking undue publicity for insubstantial amendments such as typos.) More than post-publication review (see Ali and Watson 2016), though, the journal should still take official action, when some consensus about the merits of the correction is reached.

Note that this same way to proceed would be also very appropriate for the appeals commission, that can be enacted in case the journal refuses such due course. Thus, such a forum is intended for scientific exchange devoid of, and prodding journals to overcome their political vagaries. It must not be pushed towards some court-like structure where certain "accused" are examined against some "clear definition of fraud" (see Lang 1993, §V.3). Such style of process has also proved prone to mounting controversies on a highly unprofessional level (see, e.g., the "fray-and-prey" rhetoric in Lang 2000, Appendix I).

Or, if correction review remains journal-internal, then the correcting author must at least be informed suggested reviewer(s) by name, and be given the right to object to certain choices of such reviewer(s). These are minimum requirements to grant him/her a fair involvement (and not some authoritarian display).

It must also be emphasized that these arguments rather strongly relate to the journal that published the to-be-corrected article. The status of other journals is far more delicate. A correction would effectively request them to deprioritize their own policies to serve maintaining the literature on a global scale. And, however benign, this is hardly convincing in practice. (*Cf.* Lang 1993, §I.3, V.1(a,f) and Stoimenov 2022, p.591.) This is why installing the appeals commission (or some equivalent institution) is that compelling as well. There are exceptions, though: for example, some journals share common editorial boards and split to-be-published papers by more formal criteria such as length.

Of course, under misconduct charges, it becomes especially crucial that conflict of interest is avoided at the database as well, i.e., that those who examine the case are not implicated in it. Following up on some discussion in Stoimenov (2023), we can postulate, say, that (1) a case is filed no later than half a year after the editorial decision, and (2) any adviser of the database retires from editorial boards at least half a year before assuming his/her advisory role. (He/she can be withheld records about journals he/she was editor of. And of course, advisers will not have access to cases initiated before they joined, even if probes on such cases have not yet been concluded.)

For reviewers, this timeline needs slight adjustment. Currently, they are rarely informed about the continuation (and conclusion) of the editorial process, although it should be their legitimate right. In some cases, reviewers were unsatisfied that the paper later appeared in the journal and/or in what shape it appeared. This can be accounted for by allowing a reviewer to file a case within 6 months after submitting a review, but with a one-time option to revise the record later. (This update also, for instance, facilitates the input of his/her opinion on co-reviewers; *cf.* §3.)

It goes without saying that only scientists with good integrity opinions from their peers be invited as advisers. The database will display them openly, but still must rely on them to turn down accidental review requests (which are nearly impossible to trace from outside the journals). It is also important that advisers represent various regions and cultural backgrounds: both to gain insight into diverse working practices, and to evade centralized influence. There is now a notorious example of (failed) investigations confined to single institutions (and, in one of them, to a single investigator; see Lang 1993, §I.2, Doty 1991), which should serve as a valuable lesson. However, to maintain credibility (which is absolutely crucial; see end of §6), *the database better run separately in separate fields*, with advisers being rather known and respected people in these fields. There are enough differences in detail in the to-be-evaluated process depending on the discipline, say, for mathematicians not to fully commit to an auditing mainly operated by biologists.

Unlike other questions, within a case record, the status of misconduct may better be left as a binary decision. (The editor's overall score can be displayed as "(number of positive records)/(number of all records)".) For assessing professional qualities, grave offenses are not very properly quantifiable on a multi-degree or continuous scale: there is nothing (meaningful) such as "slightly criminal" in civil conduct either. And since any author/reviewer's allegations are vetted by a group of other scientists, the risk of a "false positive" can be reduced to a minimum. Also, the delays of (safely) requesting information from journals would mean that some charges will be pending for a long time. This

is, at the end, what the database can do: it is not a prosecutive authority, it cannot be expected to engage neither in swift nor in elaborate investigations...

It makes some sense, though, that the option “N/A” is retained. This is in order not to push an author/reviewer into (escalating, at least inner) tensions. But his/her lack of confidence to answer “no” must raise suspicions. Thus, while “N/A” records should not count towards anyone’s (misconduct) score, their frequency, in particular for a fixed journal and editor, must be strictly (internally) monitored by the advisers.

Also, for instance, a reviewer may decide to allege misconduct only in a record update. Then he/she will still be demanded explanation, but it is not necessary to consult (again) the journal's side, if the aforementioned one-time inquiry had been already made regarding the submission in question. This does not disadvantage the journal, since it is not supposed to know anyway who when makes what statements about it at the database.

In closing this topic, an explanation is in place about how "misconduct" is to be understood. The database can, of course, give hints to authors/reviewers what the advisers would regard as such. However, it should ultimately be *solely the opinion* of the ones alleging and of those vetting the allegations to decide. The grass-root question to focus on is whether the involved’s actions *support researchers in good faith to trust* at face value the content of a published paper, without that they “better check” (Stoimenov 2022, p.589 l.20) all they plan to use therefrom. Attempts to formalize (Resnik 2019; and legalize, see, for example, Lang 2000), as well as, more recently, to mechanize (Hosseini and Resnik 2024) the notion of "misconduct" do poorly address this question; they have led to “fudging and contradictions” (Lang *ibid.*, §I.2) in correctness-related inquiries.

8. Summary and outlook

The database aims at implementing a concept similar to what reviewers, students, etc., have been involved in for decades: if evincing deficient performance, *to prod editors to be self-critical and self-corrective*. As such, it needs strong eloquence, and to strictly depart from statistical intentions. Impact factors (see, e.g., Brembs 2018, Waltman and Traag 2020) hardly serve in this regard, and obviously neither do well the open/closed models. Even less brings it leaving authors to stand-alone objectional initiatives (which bear hazards especially if review content is revealed; see Rejecta 2009). The legal system is totally unhelpful, because (again, unlike employment) judges will hardly be convinced of publication as an act of deep financial or life-sustaining gravity (*cf.* Lang 2000, §I.4(b), Stoimenov 2022, p.593). And while gossip (along with social media and the like) quickly absorbs a plethora of curious stories, it is generally least harmful – and, effectively, most beneficial – to those who can most easily get away with it (*cf.* Stoimenov 2022, *ibid.*). Thus, another path is needed. While some drawbacks and limitations are apparent of what is proposed here, it certainly seems a worth considering beginning.

Of course, journals’ compulsions to invest more care with their submissions may lead to the creation of more new journals, and better competition between different journals for publishing quality research. But I believe this is exactly what, it its present state, the field would mostly benefit from.

On a final note, the introduced concept is not restricted to publishing. It can be used for someone’s work in teaching [1], job recommendations, and organization of conferences (Stoimenov 2025). In all cases, in seems worth thinking about a gateway for (protectively) expressing professional opin-

ions about a person, for the sake of both this person's self-improvement, and orientation to others having/planning to work with him/her in the future. If such an evaluation framework spreads, this may – hopefully – help enforcing more responsibility in various scientific activities.

9. Sample database questions

For better overview, the following collects various suggested database queries proposed in this and the preceding papers, giving a prototype of a database record. As outlined at the end of §7, it may be useful to adjust both numerical parameters and text formulations depending on the discipline. I am open to further revisions/suggestions.

To the author: give an opinion according to the below criteria globally on all editors/reviewers involved. You have the option to rate one editor, or the entire board. In the latter case, each editor will receive (say) ¼ of of the weight of your vote. Please, not more than 2 cases per author per journal per year, and not more than 100 cases per author per year (only case-filing author counting).

1. The editor's tone was friendly and his/her letters meaningfully composed.

2. The editor responded to status queries (or allowed to track progress).

3. The editor made an honest effort to avoid reviewers having a conflict of interest with me.

4. The editor navigated the revision process well (in particular in presence of incoherent reviewer suggestions).

5. The editor justified his/her decision properly.

6. The editorial handling was timely.

7. The editorial process was reasonable, considering other papers on similar subjects the journal published recently.

8. Did the editorial process involve misconduct?

9. My overall impression of the editor was good.

10. I was (made) aware of, and satisfied with, efforts of the journal to improve its editorial process (for instance, by seeking feedback from those involved).

About the reviewer (as part of the editor's evaluation):

11. The reviewer made a serious effort to understand the scientific content of the work.

12. The reviewer's evaluation arguments were sound.

13. The reviewer's remarks were helpful.

14. The reviewer's tone was polite.

15. The reviewer's response was timely.

16. The reviewer's alternative journal suggestions (if applicable) were appropriate.

17. My overall impression of the review process was good.

To reviewers:

1. The editor's tone was friendly.

2. The editor's choice of co-reviewer(s) (if applicable) was suitable.

3. The editor duly forwarded my comments to the author.

4. The editor navigated the revision process well.

5. The editor informed me of the submission's progress until the final decision (in particular responded to status queries).

6. The editor justified his/her decision properly (taking into account my and possible other reviewers' comments).

7.-11. Same as Questions 6-10 to author.

Notes

[1] Stoimenov (2025) suggested students to oversee their own rating process, since faculty making decisions based on the data are often themselves involved in it, and could have a conflict of interest.

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