Conduct and Correctness in Mathematical Publishing

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Abstract
Little publicity is given in mathematics to journal organization and maintaining correctness of the literature. However, worrisome policies of editorial handling and peer review are exercised in mathematical academia. They originate both from modern trends (like automatization) and from a traditionally and widely spread complacent and idle attitude in academic circuits. This article displays such policies on the basis of specific instances and the reaction these practices lead to when issues about correctness of published mathematics are raised. It is drawn on concrete cases, which are crucial to understanding the cause of problems, and hence also possible approaches to solutions.

Keywords
Publication of research · Peer review · Academic conduct · Journal management · Ethical standards

Introduction: Journal Publishing for Mathematics Research

Research is a part, to a larger or lesser extent, of the life of every academic mathematician. Unlike other sciences, the mathematical research consists in calculations and logical steps which derive a new result (theorem) from previously known ones. Nowadays, mathematics has grown more complex than the expertise of individual mathematicians, and the knowledge needed to understand and apply a theorem often differs considerably from the one needed to understand and verify its proof. However, if a flaw is found in the proof of a theorem (unless other correct proofs exist), work building on that “theorem” is potentially also flawed. More than elsewhere can errors in mathematics be severe yet tiny and hard to detect and to explain. On the other hand, electronic media have facilitated the communication of research, regardless of its quality. This dramatically increases the importance of journals as places to look for reliable material. As is usual elsewhere in academia, mathematics journals

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practice peer-review (see [1] for an interdisciplinary survey). In mathematics, commonly the referee is let to know who the author is, but (of course) not vice-versa. This policy (while also drawing on technical compulsions, like indispensable self-citations) depends on the referee, as a presumably mature, mathematically and ethically responsible individual, not to personalize this relationship. Still, there is, in theory, no restriction on what kind of comment a referee can make on a paper, nor on how an editor chooses which submissions to publish. Authors and readers must rely on the editors’ good will that the material is properly evaluated and selected. While this may generally be the case, and apart from the omnipresent personal preferences and unintentional error, clearly the opportunities to abuse such a system are virtually unlimited. An important one is that secrecy, as highly esteemed to prevent attacks, could turn into a comfort to launch some.

Ultimately, when mathematical correctness issues get involved, the situation raises enough concern to motivate the following report. Its main research question—how to maintain correctness of the literature—and its relation to journal organization, is a fundamental issue which has received so far little attention in mathematics. This is one reason to seek a venue for its discussion here.

Attitude, Methodology and Structure

The account initiates from a concrete case with a reputed American research monograph series M. We will try to portray the case in the context of related events, which link it with the issue of journal publication, and explain the relation between the whistleblower S (at the time partly grant-funded researcher, 5 years after Ph.D.), and the two authors of the monograph, called A (professor emeritus at a prestigious Canadian university) and B (tenured professor at a middle-tier US university). These events include a previous relevant case with a journal C, discussed in a separate section.

Below, we will argue based on documentation for the majority of acts reported. Such sensitive information is crucial to understanding how problems arise, and thus also how to seek solutions. But it naturally faces suppression by (often influential) parties (e.g., [2] V.2, V.5), which also deny statements. (For instance, I approached M’s editor G with an early version of this presentation of his case, but without response.) To reduce bias of personal interpretation, I find it very important to provide direct quotes from the available sources. Some cannot be identified, or are withheld, for anonymity reasons. At inquiry, I will offer them more completely for independent examination (and opportunity to consult and present alternative opinions). This approach at least very substantially complements quantitative studies [3], whose underlying conflict-related statistics can always be deemed to be insidiously underrepresentative.

The later sections place these events in the context of broader issues about integrity of research communication, complemented by somewhat less extensive quotes and references to other occurrences. Some quotes are meant to partially emphasize previously chosen wordings by the parties involved. References are provided to
consult discussion elsewhere (mostly outside mathematics), but without unnecessarily reiterating their content.

A summary of the conclusions drawn from the evidence is given, and an outlook to a follow-up treatment of the problems under more recent developments in the publishing landscape. There we will also address (in more detail) proposed solutions.

The Interaction Between Referees and Authors

Some fields of mathematics have a rather small community of experts, and certain individuals are frequently called upon to serve as a referee. B exemplifies a result of this circumstance. His initially good relationship with S deteriorated when he refereed and rejected a paper S had submitted with the argument that a different proof of the result was written in a (at that time) seven-year-old unpublished draft of his. (B also suggested a lower-profile journal to submit to.) He had circulated a copy of his draft, but only after S had submitted his paper for review (and with no ability to properly add a reference), and moreover, without figures, B’s draft was hardly readable. S then strongly objected to B, thereby explicitly suspecting him as the referee. (His assumption was later gradually confirmed, explicitly by a colleague K mentioned below.) Instead of a reasonable response, S only started receiving a series of scathing comments about other papers from various journals. After this continued for a while, and S saw no other way to defend himself, he put some of these reports on his website in an attempt to make others stop B. (At the time the dispute broke out, B had allegedly declared to K that he would never again referee S’ papers. We will reexamine this and related details in context later.) Despite some conciliatory responses, S’ attempts to openly violate anonymity and object to various referees’ actions drew offense, not only from B. A very famous mathematician L wrote a confidential letter to another world-famous mathematician H who, in supportive spirit to S, nevertheless advised him about the content. Among others, L was disapproving S’ open complaints about his reports, pointing out that the next time a referee can simply reject the paper without elaborating. Another editor D reacted to S’ website more straightly. He told S he is ‘satisfied that the referees consulted were honest and did professional jobs. If your comments are criticisms of the refereeing process in general, then you should find some other forum in which to debate them. If they are specific criticisms of [D’s institution], and you genuinely believe them, then you may do better to find some other journal to submit your articles to.’ D then ‘consider[ed] this correspondence terminated.’

In a follow-up case, S solved a problem formulated by B himself in a published problem book and had a co-author who—like S—was continuously struggling for jobs but—unlike S—was completely impartial to all this conflict. The report was downright negative. S and his co-author then received from another journal a similar report, largely copied word-by-word from the previous one. S had experienced “copy-and-paste” attacks he did not attribute to B, trying hard to tolerate them as a “feature” of the reviewing system. Ultimately, there will be some editor who will not choose B as a referee. Among others, S’ paper that initiated the dispute with B
appeared five years later, with additions and improvements, in a much better journal. However just or not this outcome, a tour-de-force across editorial offices ceases to be a mere private matter when the careers of third parties get affected, and definitely when readers of B’s own papers are misled.

In a conversation, A addressed the conflict saying to S that B ‘is now your enemy’ and ‘you cannot afford more enemies.’ What can be regarded as advice about personal relationships is attached a suspicious subverse with A’s involvement in the monograph case. Exercised indiscriminately, private hostilities can easily turn into a device for transgressing whatever standards of conduct exist—be it through intimidation, attacking at will, or ignoring scientific dissent. Academia does not traditionally support many common types of confrontation with ‘enemies.’ Taking advantage of one’s position to spread scientifically baseless allegations about on others’ work, or their objections to one’s own work, is among the least that such a term could aim to advocate.

For the online database X of a leading mathematical organization E, collecting accessible overviews of published articles, A himself showed S some inclination to a comment like ‘This paper looks wrong’ [emphasis added]—but none whatsoever to clarify what is actually wrong, and whether (and how) it can be corrected. Hoax bears detrimental potential, no matter how unseriously initiated (and intended). S already felt uncomfortable over A’s advice that, before using some paper, ‘you better check’. With exhaustive (and exhausting) verification usually beyond most readers, charging such task to the entire math community quite arguably means invoking, if not provoking, a hearsay hodge-podge.

Questions of Editorial Conduct

Working for the database X, once S received a paper published in a journal C. What was suggested as a ‘main result’ had a proof of eight lines. The idea was, roughly speaking, to observe that when a ≤ b, b ≤ a, and b = c, then a = c. S himself had nine papers previously rejected by that journal (of which eight were returned without a referee report), but he knew of other authors whose much more serious submissions were “processed” in a similar way. S sent an angry letter to the editors, asking how a traditional top-thirty journal could accept something like this. He deemed this a more adequate option to writing an open—and compellingly offensive—review. (He had also other, mostly personal career concerns to raise the issue formally.)

The editors, however, showed little appreciation. As S had declared that he might go more public in a following case, they referred to S having ‘threatened’ them as a pretext to collectively refuse to handle his next submission. (Only some years later S learned from an editor of C that his initial referee reports were strongly negative.) Around that time, X (which had a copy of S’ letter to C) stopped its regular review requests to S, and has not clarified status inquiries over a period. D also advised S that ‘[a]ny potential referee will be alerted to the existence of your [review-criticising] web page’. After S then took it down in reconciliatory intention, D further cautioned that he would consider a ‘betrayal of trust’ any attempt by S to reinstate it.
So while the editors could appear to be comfortable with their doings, the disruptive image was placed on an outsider.

A was following up close S’ outrage about the paper in C and their treatment of his own. From A’s own experience, he describes a possible editor’s attitude that ‘if his papers are good, some journal would publish his papers,’ and ‘once I decided, it is usually final.’ Thus, ‘what kind of response do you expect from the editors? Apology?’ Still, A admitted that if a decision is made ‘by a political reason’, then ‘it is hard to prove it.’ This remarkable concession offers some imminent conclusions about in how far editorial action results from or addresses the—insistently brought up—job’s difficulties. However, in defense, an editor might appeal to a universal (because hard to argue against) justification: difference in opinion. Indeed, any perception of quality or importance of a paper, no matter how widely supportable, is subjective—and hence can always be disputed by a particular editor or referee. In the long run, if published material is technically correct, there is no irreversible harm done to mathematics. This is less certain, though, when errors are present. Such a situation arose a few years later—and became the occasion for the present discussion.

The Monograph Case

During his research, S discovered an error in the monograph of the two authors published in the series M. The problem was largely repairable, but it needed some explanation. An eight-page note was sent to the managing editor G to present the issue to the authors. Instead of some serious response—that either they would refute the objection or write an erratum based on the explanation—S received just a very brief comment from B, sent on behalf of both authors. B merely stated that they ‘never claimed’ what S was referring to, and ‘[i]t seems to us that [S] may not understand details of our paper.’ It is true that the claim does not appear explicitly, but it enters into the proof of one of the main results. B also sent a “revision” of this proof, but it had corrected only a few typos and did not address any of S’ concerns. S replied, listing again one-by-one the questions and objections, but he has not witnessed any further mathematical comments from the authors addressing this proof.

In a first attempt to publish a correction himself, S had submitted to the same publication M a long manuscript including the discussion of the error. Seven months later, a (transpiringly pre-formatted) message stated that M could not consider the manuscript, partly due to backlog, which had in fact been pointed out in advance by the managing editor G. Then S’ short note was submitted to M, intended as an erratum. (S had not witnessed any effort for a correction a year and nine months since the initial communication regarding the error.) The managing editor G, however, declined to handle S’ correction, too (in particular to have it examined by an independent party). He insisted that ‘corrections are by the same authors. So [S’ note] must be considered a new article. It certainly is not appropriate for [M] by length considerations. It certainly seems that you and the authors of the original [monograph] do not agree on many things. I think that if you want to publish this, you should submit to some independent journal and have it refereed. So we cannot
consider this for [M].’ G did not explain on what issues the authors disagreed with S, and did not ask his position on these issues. S received no response to a further query asking in what form errata to M can be published. Later the same month, meeting at a conference, A told S that B would contact him regarding the matter. Also, a common colleague T had told S that the authors planned to put a corrected version of the monograph on the preprint server V [4, 5]. But after a further ten months waiting, still S saw nothing had happened.

S then turned to a senior colleague with similar recent experience, who advised him to contact the managing editor G one more time directly. Despite being informed in detail, among other things, of the conflict with B, G reaffirmed his determination not to handle S’ correction. He reiterated the old (and inadequate) response of B, and declared that ‘the decision was not to submit an erratum to [M].’

Only at that point S started sending his correction to other journals. After M, at least eight other journals officially rejected or declined to seriously consider the erratum. The editor of the fourth journal made the first “authorized” admission that ‘there is indeed a mistake in the [A-B] paper.’ However, according to ‘a couple of experts,’ ‘repairing that mistake is not such a big issue.’ This is possibly so, but it is hard to judge (and dispute) by others with insight withheld into both what is evaluated and who evaluates. (Are the ‘experts’ disjoint from the set {A,B}, and conclusively unbiased?) One of the later referee reports acknowledges ‘[t]his paper describes an error in a [monograph] that appeared in [year]. Given that the [monograph] has been cited [number] times (according to the [X online database] count), it is not a major publication. Nonetheless, it is of interest to the [specialized] community and has generated further research. Thus a paper correcting an error in it should be published. But does [this journal] publish papers of that sort, or should there be an audience outside of [this particular] community?’

**Mathematical Lit(t)erature?**

Lack of author and editor enthusiasm about dealing with flaws is hardly novel ([6], pp. 305–306). One colleague responding to my letter [7] quoted an editor telling him that his paper is not suitable for publication, because it corrects another paper. The reasons one can attach some significance to the problem with the monograph are that

- The problem is one of the main results of the monograph;
- Research monographs are not reprinted (unlike, e.g., undergraduate textbooks, to which one may tend to compare M rather than to research journals), which would provide an opportunity to correct the error;
- The error is subtle (it had not been noticed for about fifteen years since the manuscript was written);
- This is the only proof available;
- Other work (published in other papers) depends essentially on this result; and there is at least one other (published) paper which uses the argument and may (without that details were examined) encounter the same problem.
Of course, disputes about personal views on the importance of corrections are difficult to win. But while suggestively an erratum is not needed for every typo, evidence that the explanation of the error was not carefully read and understood is hardly convincing. A traditional view of publishing responsibility in the scholarly community is that the mere existence of ways for someone to communicate corrections does not justify publishing, or letting stand one’s flawed research. There is the custom that an erratum is written by the original authors, but this critically depends on their sense of responsibility for it. The argument that something (here, a correction) does not merit publication because it presumably does not have wide appeal might be valid in tabloid journalism, but it has little defense in science: should we write no proofs because the few read them? Some other authors find in their great expertise confidence to downplay scientific objections or, worse, caution if (as A and G formulate it) ‘you and [they] do not agree on many things’, that ‘you cannot afford more enemies.’ Lang writes regarding such a stance in [2] (end of V.4, commenting on a quote by Davis): ‘I do not recognize being an “exceptional scientist” as a license to throw one’s weight around to avoid answering scientific criticisms.’ Whatever its polemic, the statement has a basis: it is hard to imagine some authority to administer over who is privileged to litter how much in his publications, and who will clean up.

It must be acknowledged here that the advancement of computers did help to clear up a proof like the long-disputed case of the Four Color Theorem [8, 9]. But such projects remain very substantial (both scientifically and financially), and usually target individual important results. Specifically on the background of the broad discussion about the hazards of mass robotization (see, e.g., [10–12]), the future role of machine verification platforms remains to be seen. For now at least, their capacity of globally supervising mathematics literature is difficult to imagine. Responsibility must obviously be sought somewhere else.

One further point to keep in mind is that, even if a journal does not remunerate its contributors (or vice-versa), publishing is not a charity enterprise. There is little justice in leaving it to benevolent efforts to maintain the literature, while editors and publishers levy subscriptions and authors seek employment, tenure, or financial support based on it. B had regularly government grants, and the monograph was one of his (if not his most) noteworthy research asset. Publication [13] and funding issues [14] often exercise pressure which becomes a major cause of misconduct. However, as Lang notes ([2], section V.4(b)), in reality, notorious cases throughout science [15] have beheld at questionable practices reputed, influential individuals. They face far less existential pressures and risks than those—students or young researchers—seeking to report [16], exposed to acquire (see [17]), or directly affected by these practices. S had gone several years without employment, also during the time A was prodding him to condone various higher-ups’ lapses, and B in reviews (and his advisor also in letters of “recommendation”) vocally took on the theme about S’ paper writing deficiencies. While A had expressed confidence that the reviewing system is generally fair, reporting on a hiring discussion he did bring up the paradigm ‘When you are nobody, everything is tough’.

Beside addi(c)tional aspects ([18], p.331), scientists skyrocketing (or being skyrocketed) may increasingly perceive attention to common problems and rules not
only as unwarranted, but as unappreciated. Others may target the science community at large as the debtor for their own past difficulties. Such individuals are well aware that, whatever they set off to, effective resistance is hard to organize, or count on their connections in science circuits to stonewall, or steamroll, it. Most people do not willingly enter others’ disputes, or investigate anyone’s merits or stakes. Some outright defer to a scientist’s prominence, or like S’ advisor, his good personal opinion about B. T’s words that ‘I do not want to be too openly critical about [the monograph]’ and ‘I just want to be friends with all involved parties’ exemplify this natural reluctance to active (op)position. (Compare also the quote of Dingell in section IV.2 of [2], and those of Davis in V.2 and V.4 ibid.)

Easily earning A’s predicate that ‘it is hard to prove it’, sensitive accounts meet most official venues shrugging their shoulders. (I.3 of [2], specifically Reviewer Y, provides one archetype among many.) Other attempts like S’ website lead to jeopardizing relationships and less assistance than retaliation. This very much limits the opportunities for public record ([19,38]), the more it protrudes affected identities—a colleague even used the term ‘academic suicide (for you)’ for deanonymizing the present cast. Instead, gossip becomes a commonplace to vent dissatisfaction yet avoid accountability ([6], p.302). But it arbitrates conflicts poorly, while favored to discredit at convenience (and draw critical support away from) someone involved, even on unrelated matters—as S found out about one of his job applications. And so it comes that some scientists attracting public money for an alleged quest for truth are left failing to address, and even to admit falsehoods, while others’ struggles with corrections (and career) become more entertaining than entertained.

Another way that some authors or editors defend their conduct is by referring to the existence of courts of law. Lang ([2], section V.3) writes that ‘such a point of view undermines the exercise of scientific responsibilities, as distinguished from legal responsibilities.’ This line of thought suggests that practices like sloppy writing, refusal to correct errors, and oppressing junior colleagues are acceptable because they are not illegal. A’s conception that ‘it is hard to prove it’ how one possibly acts or what one intends may go over as testimony of evasiveness whitewashing spreading immorality. If not implying indoctrination, it mandates focus on what to ‘prove’ to face a gradual shift from mathematical problems to behavioral ones.

In many conversations, A (and not only he) discouraged S from (and criticized others for) settling scientific disputes legally. But his optimism about academic authorities’ self-management appears increasingly untenable. It is their ongoing, and in certain cases grotesque, failures that have at all brought up such external, even if deficient, regulatory mechanisms. But they not only allocate an inordinate amount of resources (see [20], p.184); no matter how much justice they provide, they will, by nature, turn further away from scientific policies. We will have then (more) situations of the sort where the exposition of research is advised by attorneys and its credibility and applicability ruled by judges [21].
The Role of Journals

There seems no definite code of editorial conduct, and so editors have some freedom to set their own policies. But whatever these may be, they should not oppose basic scientific principles. For instance, in the Hales-Hsiang case [22, 23], the journal of Hsiang’s article did publish Bezdek’s counter-example to one of Hsiang’s main claims. It seems reasonable that, if the journal cannot guarantee the author’s responsible involvement (also due to his, understandable, changing life circumstances), it should grant the right to others to publish corrections, and certainly have them carefully—and independently—considered. This basic tenet of science about the dissemination of criticism is also seen in the principle that an author should not referee his own paper, as he can never be deemed objective about his own work. But similarly, he can hardly always be objective about its correctness. If solely an author should write, or judge over errata to his paper, is such a rule there to manifest truth, or authority? In fact, A himself advised S against complacency as an author, commenting regarding his experience with journal C that ‘you should be modest. You believe your paper is excellent, […] but others may not think so.’ This is unquestionable, but it adds that much more stark contrast to A’s reaction over his own error. There is, though, indeed a crucial difference: publishing status, and how it influences expressing and reacting to scientific criticism draws further beckoning from the general attitude of journals.

No journal is responsible for an unpublished manuscript, and in fact journals seek ways to dispose of submissions ([24], p.336). Often without detailed expertise, and with a narrow circuit of competent referees to preserve, editors have, whether unable or unwilling, grown increasingly averted to concerns about review critiques. Moreover, unlike the author, a reviewer is the editor’s choice of trust, so that challenging a reviewer can be regarded by editors as a credibility issue to themselves. Such editorial partiality has reinforced certain referees’ view that an acceptable critique is also to put up mathematical pretenses while aiming at unfavorable colleagues.

Given that a submission’s reviewing (and reviewers’) history is generally of little concern to editors, the process is not only secured through discretion, but open to iteration. For Lang ‘[i]t’s like the video games: one can’t shoot fast enough’ ([21], p.797). B’s so-planned review abstinence prompted K to faithfully suggest to S not to suspect B for further reports. B’s turnaround—which is a sordid habit of skilled politicians at closed doors—can only benefit, however unintendedly, from K’s defense. L was directly consulted by one referee quoted on S’ website, and later writing to H eventually admitted he found the report’s narrative unstyleful. L’s posture then to reassert (defend?) reviewing laissez-faire and instigate S’ conformance to the status quo, if nothing else, helps little about referees’ manners.

Reacting to such incidents, some (in mathematics comparatively few) journals now allow specifying undesired reviewers at submission. But G demonstrates that conflicts of interest far from certainly meet delicate editorial response, even if one can identify a conflicting party, which is not necessarily easy. D was manifestly displeased when S asked him to exclude reviewers, taking advice by a senior who pointed out this provision at a granting agency. Once a named opposed reviewer was
allegedly contacted there. Similarly, requesting letters of reference (like S’ experience with B’s advisor), one knows the sender but not the content. Alike episodes warrant the least takeaway that especially young scientists could be encumbered from ‘betrayal of trust’ far more than someone such as D. And besides A’s philosophy that ‘[w]hen you are nobody, everything is tough’, how to maintain ethics and fairness in the mires of confidentiality remains a question not less valid.

The situation changes, though, profoundly if a paper is published. On the one hand, some authors see in a publication by a reputed journal, even ‘by a political reason,’ a certification of their ‘excellent’ work. On the other hand, problems arising with its publications directly affect the journal. Thus blame is most conveniently shifted to the new party with least authority: the audience. It is not surprising, therefore, that when authors rebuff an objecting reader, some editors happily join the chorus. The effect of such behavior is then to push toward judging how ‘excellent’ a paper is by whether and where it is published, while discouraging and obstructing its actual study. In turn, authors are increasingly abetted to neglect the content of their work for bargaining about level of its publication.

Numerous problems with various forms of scientific peer review are well-known; see, e.g., [25–29]. Most relevantly, even with the purest intention to examine a paper mathematically—and not politically—a referee is not infallible. Often he sees well his shortcomings and recommends publication relying on authors and journal to take ultimate responsibility: neither can he take such responsibility, when his identity and his work are not publicly disclosed, nor should he, when he acts voluntarily and is not supposed to receive any credit for a publication. (See also Geist et al. [3], Section 4.) Thus pronouncing refereeing a universally vigorous seal of correctness is not more than some editors’ absolutory ‘myth’ ([30], p.108, l.2).

Problems have also arisen when both academia [31] and commerce have pushed for lowering journal costs. In one case, S was compelled to resubmit elsewhere a paper accepted by the journal N, after the editor, typesetting by himself in his spare time, was unknowledgeable (and uncooperative) to handle the figures. In another, after four galley proofs, the requested changes were still not properly done, and an editor (re)tired over such issues called the publisher’s production rearrangements ‘a major disaster’.

**Summary and Outlook**

While mathematics research literature is critically sensitive to correctness, its maintenance though proper journal management is little considered. The particularly intolerant attitude towards submissions in mathematics publishing is readily translated also onto attempts to publish corrections, resulting in cases of ruthless behavior on the part of editors, authors and reviewers. They highlight the distinct possibility of the establishment of a culture of publishing, where responsibility for the correctness of published mathematics can be evaded, while readers finding mistakes in published proofs are stamped as outcasts for the sake of protecting certain (individual or organized) entities in the science press.
Many developments have taken place since the initial case with M, whose treatise cannot be accommodated here for space reasons and will be moved to a follow-up article. There, we focus on the consequences of electronization, with particular emphasis on mathematics (and its consistency), where we hope to contribute more compared to existing accounts. We discuss related features like preprint servers [4], online(-only) journals, impact factors [32–35], editorial infrastructure [36], and so on. The effect of such policies on standards of scientific conduct will be analyzed.

In closing, awareness must be raised to uphold the traditional standards of research and academic conduct in mathematics. It will also be discussed in detail what can be done about these issues. A few resources with suggestions in this regard have been published. In addition to Hill’s accounts [19, 37], we mention the conclusion of physicist R. Trebino’s article [38], as well as Fried’s proposal of a pool of special referees for high-quality journals [39]. We will contribute further ideas after shedding some more light on how recent innovations have influenced the underlying causes.

### An Alphabetical List of Aliases

A  Professor emeritus at a prestigious Canadian university, first author of the monograph
B  Tenured professor at a middle-tier US university, second author of the monograph
C  A journal where S had submissions rejected and an online review to write
D  An editor S had a dispute with regarding reviewers
E  A leading mathematical organization
G  Managing editor of the series M
H  A world-famous mathematician, supportive to S
K  A colleague of B faithfully suggesting to S not to suspect B for any further reviews
L  A very famous mathematician who wrote a confidential letter to H
M  Series which published the monograph of A and B
N  Online mathematical journal quoted for typesetting problems
S  The whistleblower who questioned the correctness of some argument in the monograph
T  A colleague discussing with S about the correction of the monograph of A and B
V  A preprint server popular for (among others) mathematics papers
X  Online review database of the mathematical organization E

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