

ON DIFFERENT RELIABILITY STANDARDS IN CURRENT MATHEMATICAL RESEARCH

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ABSTRACT. In this note I describe reliability standards for writing and reviewing mathematical papers; these standards are (in my opinion) vital for the progress of mathematics. I give examples of applying the described or other reliability standards.

CONTENTS

1. Introduction	1
2. Different reliability standards	3
3. Different reliability standards: examples	6
4. Reviewing the peer review system	9
5. Reviewing the peer review system: examples	11
6. Violation of important scientific principles	14
References	30

Mozart was a great composer.
The Barber of Siberia

1. INTRODUCTION

The reader knows the difference between a medical drug that passed the tests and is acceptable for use, and a drug that did not. It is vital to have the tests, the information on passing them, and the information on how competent the tests were. The purpose of this paper is to attract attention to the analogous problem of reliability in mathematics.

This paper will hopefully be interesting not only to mathematicians and scientists, but also to people interested in mathematics as a part of culture, and as an activity in which their taxes are spent. Most of this paper is accessible to non-specialists (although some examples do contain technical details).

By ‘reliability’ of a research paper I understand validity and novelty of results.¹ Reliability standards in mathematics change. E.g. a long time ago, oral proof was considered reliable. As proofs gradually became more complicated, there appeared the understanding (and then the rule) that only written proof can be considered to be reliable or not (and so could constitute a fair claim for a result).

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¹This is closely related, but is not the same, as clarity. For recommendations on clear writing see the references in [Pa18, §2.1], and [Pa18].

‘All times are changing times, but ours is one of massive, rapid moral and mental transformation. Archetypes turn into millstones, large simplicities get complicated, chaos becomes elegant, and what everybody knows is true turns out to be what some people used to think. It’s unsettling. For all our delight in the impermanent, the entrancing flicker of electronics, we also long for the unalterable... Don Quixote sets out forever to kill a windmill.

(U. K. Le Guin, Tales From Earthsea)

Different reliability standards coexist. So it would be nice if mathematicians and math journals publicly and explicitly revealed their reliability standards by publishing their (potentially different) opinions on specific examples. This would allow more competent decisions of the math community, sponsors, and tax-payers to support this trend or another in mathematics.

It is not merely true that a creed unites men. Nay, a difference of creed unites men — so long as it is a clear difference.

I am quite ready to respect another man’s faith; but it is too much to ask that I should respect his doubt, his worldly hesitations and fictions, his political bargain and make-believe.

(G. K. Chesterton, What’s Wrong With The World)

A **reliable reference** is a text whose results mathematicians can use without doing the author’s work: to check thoroughly the validity of the results and to properly describe the relationship to earlier publications.² Being a reliable reference in the first approximation corresponds to ‘accept as is’ recommendation for a peer reviewed journal.³ The peer review system [Pe] is reviewed in §4–§6, see also [Paj] and the references therein.

The reliability standards I share are presented in §2 and §4 below mostly in the form of recommendations. These recommendations are based on traditions I learned, as well as on my own experience as an author, a reader, a referee, and a jury member of scientific contests.

This paper can be considered as a complement of practical nature to the discussion by prominent mathematicians [JQ93, ABC+]. For similar issues on references, on computer science conferences, and on the *abc*-conjecture see [Pan, Fo, K118], respectively.

This paper can make only a modest contribution to revealing and improving of standards for writing, discussing, refereeing, and taking editorial decisions. However, collective work of thousands of researchers on their usual tasks (like writing a paper, a referee report or a Zentralblatt/MR review) can make mathematical research reliable according to the moral and material support that we are applying for.

Remark 1.1. General words on delicate subjects (like how to play piano) are almost useless without examples. It is only by going into substantial details that we can conclude if a referee report (or an author’s reply to such a report) is competent; otherwise we are likely to be distraught by emotions.

Also, it is easy to violate our declared principles, when they come into contradiction with our worldly hesitations and fictions, group thinking [Gr], political bargain and make-believe. (This is usually done unintentionally and unconsciously, and revelation of this is painful to

²We should at least try to understand arguments even from reliable references. However, to understand an argument omitting details on the assumption that they are already checked requires much less effort than to check an argument including all details on the understanding that every detail might potentially be a problem. Everybody who uses a computer program knows the big difference between a computer program and a computer program that works. Everybody who performs work for a user knows that quite some time is required from the developer to test the result.

³Such a recommendation allows further changes that need not be checked by a referee (unless the changes are significant).

us.) The turning point is to practice what one preaches, and this inevitably changes the preaching. ‘*He who practises the Way (Tào) suffers daily loss of its false shine*’.⁴

Thus examples are important. They are presented in §3, §5, §6, [Ha], [Sk16, Remark 5.3, §5.3, §5.4]; references to examples are presented in §2 and §4; see also <https://scirev.org/>.

Conventions. I omit ‘in my opinion’ for brevity. Some *general* statements (‘should be’, ‘clearly’, etc.) are made on the assumptions that no public objections will appear; I am ready to give justification, if public objections appear. All *specific* examples are justified as thoroughly as to possibly annoy some readers with details; such readers can omit the details.

A genius makes his own rules, but a ‘how to’ article is written by one ordinary mortal for the benefit of another... Authors of articles such as this one know that, but in the first approximation they must ignore it, or nothing would ever get done. [Ha74]

By *user* I mean the user (reader, reviewer, etc.) of a paper, who can also be the developer (author, advisor, etc.) of other papers. Often users read papers for developing other papers.

2. DIFFERENT RELIABILITY STANDARDS

Remark 2.1. (a) Among the first steps of checking the proof it is advisable to

(1) write on the first pages the statements of the results, together with all the definitions used;

(2) structure the paper so as to separate motivations from statements and proofs, e.g. by having a separate subsection ‘motivations’ (see e.g. [Sk20o]);

(3) give rigorous proofs of all statements;⁵

(4) study proofs of most closely related results and send the paper to their authors for comments (see more in Remark 2.4.a).

Using unsophisticated language, and sticking to statements and proofs of the main results, helps to find (and correct) mistakes.

(b) It is advisable to explicitly state any non-trivial result, and either prove it or give a reference. This is not only necessary for reliability, but contributes to the unity of mathematics, by making a paper more accessible to mathematicians from other areas. This in turn ensures higher reliability. Lack of explicit statements decreases reliability, and also contributes to artificial splitting of mathematics (and even of its areas) into different subjects whose representatives cannot use each other’s work.

(c) The usual problem related to the lack of explicitly given rigorous definitions / statements is not that it is hard to reconstruct them, but that this can be done in several ways.

⁴This is an English translation of a citation from the Russian translation [Ch, Chapter 22] of *Chuang tzu*. I am grateful to Yan Pan for informing me that the following translation (said to be by Y. Lin) seems more acceptable to Chinese scholars: ‘He who practises the Tào, daily diminishes his doing’. In particular, the words ‘of its false shine’ are not present either in *Chuang tzu* or in English or Russian translations different from [Ch] and available to me. Still, because of the preceding text in [Ch, Chapter 22] I find ‘of its false shine’ a proper commentary.

⁵This can be done either under a head ‘proof of such and such numbered statement’, or in phrases like ‘Such and such numbered statement follows from [a list of numbered statements from this or other papers]’, ‘The proof is obtained from such and such a proof [reference] by the following substitution: [replace this by that, etc.]’.

This should be done instead of, or in addition to, informal phrases like ‘it is somewhat well-known to be’, or instead of some informal observations ended with ‘we obtain the following: [statement]’ (see e.g. [Sk20e, the third paragraph of footnote 4]). Phrases like ‘The proof is analogous to such and such a proof [reference]’ should be avoided when the analogy requires too much time and efforts from a user.

Remark 2.2. (a) *Listeners* [AS: and readers] *are prepared to accept unstated (but hinted) generalizations much more than they are able ... to decode a precisely stated abstraction and to re-invent the special cases that motivated it in the first place.* [Ha74]

(b) *The modern world is full of theories which are proliferating at a wrong level of generality, we're so good at theorizing, and one theory spawns another, there's a whole industry of abstract activity which people mistake for thinking.* (I. Murdoch, *The Good Apprentice*)

(c) See [GKP, Preface] for discussion of similar issues.

(d) It is advisable to postpone technical results and definitions to later (sub)sections and bring bright results to earlier (sub)sections. In my opinion, bright results and their proofs are potentially more useful than technical versions of known constructions which so far did not yield any bright results. Here by bright results I mean non-trivial results whose statements are accessible to mathematicians specialized in this area of mathematics, but not necessarily in the subject of the paper.

(e) When one uses a specific theory, it is advisable to explicitly state results to be proved with the help of this theory, but in terms not involving the theory (see e.g. [Sk16, Sk19, Sk20e]). This makes the *application* of the result accessible to mathematicians who have not specialized in the theory. So one is motivated to study the theory and sees explicit statements which could guide this study.

Instead of the above way, some papers start exposition with details of a specific theory which are matter-of-fact to specialists but are inaccessible to mathematicians from other areas. (E.g. compare [LL18] to [Sk19] and [BZ16, §3.3] to [Sk16, §2.3]; see [Sk16, Remark 5.7.8].) Sometimes this happens because of the false (and possibly not conscious) assumption that the readers will accept artificial sophistication as depth and high non-triviality.

(f) Sometimes originally a complicated proof is invented. (This often happens because of lack of familiarity with simple expositions of the subject.) Freeing a proof from complications appearing in its invention is a way of checking the proof, not only the courtesy of presenting a simplified proof.

In particular, a large part of a theory that authors describe could be superfluous for a proof of the result. It is advisable to give a direct proof without formally referring to all the theory that helped the authors to invent the proof. Relation to the theory can though be mentioned in a remark which is not part of the proof. See also Remark 2.2.e.

Analogous remarks holds with 'proof' replaced by 'statement', etc.

(E.g. compare longer proofs in [KS20, §3,§4], [Pa15], [?] to shorter proof in [KS20, §2], [Sk18o], [PS20], respectively, or proofs in different versions of [Pan15, AMS+].)

Remark 2.3. ⁶ (a) A published paper is for a much wider audience of mathematicians than just referees, close colleagues and Editors. So motivations for main results, details of the proof, etc. should be written in the paper, not in letters to referees, close colleagues and Editors.

(b) A published paper is for users, not for developers. Working on details could be an interesting task for a developer, but is usually not within the intents of a user.

The following are good lower estimates of how hard work on details is:

- the amount of time required for authors (or for other mathematicians) to make the details publicly available upon request of a reviewer.
- the amount of text written by the authors to justify that the details need not be made publicly available.

⁶For example of a different opinion by L. Vokřínek see [Sk20e, footnote 4]

(c) Updating the arXiv version is not considered as an indication that the published version has any serious gaps. Some authors previously updated arXiv versions upon my suggestions as a math reviewer, and we didn't have any discussion about that. See e.g. arXiv:1609.06573v3, arXiv:1209.1170v4 and a forthcoming paper by D. Gugin.

(d) Discussions of a text involving a user of this text should refer (at least upon request of the user) to the text, not to any non-existent text obtained from the discussed text by some changes (see e.g. [Sk20e, the third paragraph of footnote 4]). Of course, this need not apply to discussions of a text between developers (e.g. coauthors) of this text.

Remark 2.4 (important steps to prepare a quality submission). (a) The mathematical community needs the reliability of a paper to be checked *before* not *after* putting the paper to arXiv (see motivations in Remark 2.5). The same is true for *preliminary* checking of the novelty, because putting a paper to arXiv is a means of *further* checking of the novelty. So it is advisable before putting a paper to arXiv to discuss it among specialists in its area. Such a pre-submission discussion usually allows the author to check whether his/her results are clearly stated, new, and completely proved. This allows to improve quality of the paper.

Improving quality may involve a disillusion and dissatisfaction (as a part of learning, cf. Remark 1.1). A dissatisfaction which might appear during such work is a natural part of improving quality of the paper and qualifications of the author. Such work is interesting if authors (=developers) recognize the importance of learning and fulfilling the wishes of their readers (=users). Such work is annoying only if authors write under the assumption (however unconscious) that their work need not be useful. Improved quality of the paper publicly available on arXiv improves the author's reputation, while low quality damages it.

It is advisable to put a paper on arXiv before submitting it to a journal, cf. Example 5.1. Without this simple procedure the possibility remains that the results of a paper are already (partly) known. What is unknown to one group of mathematicians can be known to another group. Putting a paper on arXiv allows including into a pre-submission discussion (described in the previous paragraphs) people who work in related areas, but are not in contact with the author. Improved quality of a paper published in a journal improves the author's reputation, while low quality damages it even more significantly.

In a less formal pre-submission discussion it is easier to help the author, to share ideas with him/her, and to minimize the critical part of such help. Publication of a paper on arXiv without prior discussion with a colleague means that the author expects a public, not a private approval or criticism of this colleague. The colleague might still prefer private criticism, but could be compelled to criticize publicly if the arXiv text contain flaws which obstruct the progress of mathematics, cf. Remark 2.5.

Writing a referee report on a paper is a responsible task involving double-checking (see Remark 4.1). In this time-consuming form it is much harder to help the author than via informal discussions.

The above steps do not absolutely protect against significant flaws, see e.g. [Sk08, footnote 3 in p. 2]. However, with the above steps done, the responsibility is shared with the math community.

(b) During pre-submission discussion, specialists in the area might send their specific suggestions/criticism which they consider important (below this is shortened to just 'suggestions'). Then it is advisable to put on arXiv (or submit to a journal) a revised version approved by specialists. Of course this might not be easy to do. E.g. the authors can receive no comments from somebody; they can disagree with some suggestions (and they cannot be sure that they would not receive another stupid or essential suggestions the day after they

finalized their work, based on previous suggestions). Hence the authors can decide to submit their paper to arXiv/journal even if they did not

- (1) receive a feedback from some persons,
- (2) take into account some suggestions, or
- (3) give a chance to a person who sent feedback to learn his/her opinion whether his/her critical remarks are properly taken into account.

There is no need to mention (1), but it is advisable to mention (2) or (3) in the submitted text. See Example 3.3.

Remark 2.5 (claims and responsibility). Here I present some motivations for the recommendations of Remark 2.4. A natural reaction to Remark 2.4 is as follows: *The opinions of people vary on how to use arXiv. It is good if there is some diversity. Some people would put on arXiv only a finished paper which appeared in a journal, some people would put there very early preprint. Overall, a paper on arXiv may be both: already a solid work or something just very early. Many mathematicians, if they find some problems in an early preprint on arXiv, would either ignore this, or send their remarks to the authors.*

A research paper (on arXiv or elsewhere) can have both positive and *negative* impact on the development of mathematics. A usual example of a negative impact is as follows. A paper claims an interesting result but the proof does not form a reliable reference (see §1). Then this result cannot be used. Also, colleagues are discouraged from providing a reliable reference because its publication might involve hard and unpleasant work of justifying that the existing text is not a reliable reference. So the paper obstructs the appearance of a reliable reference (by authors of the paper or by other authors). So the paper may involve unfair competition (cf. footnote 9).

Mathematicians often have interesting ideas but no time to develop them into reliable references. Sharing preliminary ideas in the form of claiming results has a negative impact described above, but could also have positive impact. Sharing preliminary ideas as preliminary ideas is useful and not harmful.

A clear diversity in arXiv papers, when a paper in its first lines explains whether the authors think it is a finished paper or a very early preprint, certainly stimulates progress of mathematics. Lack of clear diversity allows the attempt, however unintentional, to both make a claim and not have the responsibility for making it. With some administrative support, this attempt can well be successful in terms of having paper published, getting a grant or a job, etc. So without clarity (provided either by authors or by their critics) the style of not caring for users has a significant advantage of development comparative to the style of caring for users. This has negative impact on the development of mathematics.

3. DIFFERENT RELIABILITY STANDARDS: EXAMPLES

Example 3.1. (a) Different standards concerning Remarks 2.1.a.(3), 2.1.b, and 2.3.ab are illustrated by the discussion of whether the paper [Ad18] is a reliable reference (see explanation in §1) for a proof of the Grünbaum-Kalai-Sarkaria (GKS) conjecture (cf. [Fa20]).⁷

An attempt to prove GKS is made in [Ad18, (3) in p. 7], where the implication ‘[Ad18, Theorem I(1)] \Rightarrow GKS’ is attributed to [Ka91], and it is written ‘*We will give a simpler,*

⁷The most important letters are presented in (d). Some letters are omitted. In those letters Karim neither stated that according to his reliability standards the paper [Ad18] is a reliable reference for a proof of the GKS conjecture, nor provided a modification that would make the paper [Ad18] such a reliable reference. The letters will be published here only if the judgement of this footnote is questioned, so that a justification by their publication is required.

self-contained proof of this implication in Section 4.6. This implication is not explicitly proved

- in [Ka91] (it is even not explicitly stated there) because the statement of [Ad18, Theorem I(1)] is not present in [Ka91] (even as a conjecture), and the paper [Ad18] does not mention that [Ad18, Theorem I(1)] was not known in 1991 (even as a conjecture);

- in [Ad18, §4.6] because [Ad18, §4.6] does not explicitly mention this implication.

(b) In his letter of 22.01.2019 (but not in [Ad18], see Remark 2.3.a) Karim Adiprasito writes ‘...it [AS: GKS] follows from Theorem I and Corollary 4.8. (I also remark that after that corollary).’ This is wrong for the following reasons. First, [Ad18, Corollary 4.8] is not a rigorous mathematical statement because

- an object cannot satisfy a theorem (as in arXiv version 1 of [Ad18, Corollary 4.8] to which Karim’s letter refers; I also could not find any statement named ‘hard Lefschetz theorem’ in [Ad18, §1-§4]).

- the definition of ‘the hard Lefschetz property’ is neither presented nor referred-to in [Ad18, §1-§4].

Second, after [Ad18, Corollary 4.8] it is not remarked that GKS ‘follows from Theorem I and Corollary 4.8’.

(c) In spite of (a,b) it is still possible that [Ka91] or [Ad18, §4.6] contain something very helpful for writing a reliable proof of the implication ‘[Ad18, Theorem I(1)] \Rightarrow GKS’ (and that the implication should be attributed to [Ka91]). A reliable reference would explicitly prove the implication (and explain why the implication is attributed to [Ka91], with references to particular places in the 25-page paper [Ka91]).

In January 2019, when I found the gap described in (a) in version 1 of [Ad18], I thought the gap is very minor. As of March 2022, this is a 3-year gap and a half-a-page gap in the sense of Remark 2.3.b. But I do not assert that the gap cannot be filled.

(d) (AS to KA, 16.01.2019)

Dear Karim,

Thank you for writing an interesting paper [v1 of [Ad18]]. I am interested in learning your proof of the Grünbaum-Kalai-Sarkaria conjecture, but I could not find it in §1 or §4. If I am missing something, could you let me know in which page(s) the proof is written? If not, could you update your paper adding a head ‘proof of the Grünbaum-Kalai-Sarkaria conjecture’ and such a proof under this head?

Best, Arkadiy.

(KA to AS, 22.01.2019; I am grateful to Karim for allowing me to publish this letter)

Hi Arkadiy,

this is a corollary of Theorem I, and it is somewhat well-known to be (Kalai showed this first in his paper [Ka91]). I did not list all corollaries of that theorem explicitly, because there are too many and the derivations assuming my main theorem are done better by others before me, but the Grünbaum is actually derived explicitly, as it follows from Theorem I and Corollary 4.8. (I also remark that after that corollary).

Best, Karim

(AS to KA, 5.04.2020)

Dear Karim,

Thank you for your renewed interest in my critical remarks on arXiv:1812.10454. I sent you all those remarks in January, 2019. I’m afraid they are not taken into account in [[Ad18]]. The proof of GKS seems to be contained in (3) in p. 7, where you attribute the implication ‘Theorem I(1) \Rightarrow GKS’ to [Ka91] and write ‘We will give a simpler, self-contained proof of this implication in Section 4.6’. Your paper suggests that Theorem I(1) was not known in 1991, even as a conjecture. Thus implication ‘Theorem I(1) \Rightarrow GKS’ could not be proved in [Ka91]. Section 4.6 contains no head

‘Proof of the implication Theorem I(1) \Rightarrow GKS’. Section 4.6 has only one formally stated result, Corollary 4.8, which is not the implication ‘Theorem I(1) \Rightarrow GKS’.

It would be nice if you could update arXiv version adding a proof of the implication ‘Theorem I(1) \Rightarrow GKS’ under the name ‘Proof of the implication ‘Theorem I(1) \Rightarrow GKS’’. If you send me a project of such an update, I am willing to read it and tell you if I have further questions.

Best, A.

Example 3.2. Different standards concerning Remark 2.4.a are illustrated by the following example. The paper [FV21] refers to [MW16] without indicating unreliability of the proof in [MW16] justified in [Sk17o], [Sk17-3, §5] (and without referring to the different proof of [Sk17-3]). A discussion with colleagues (or an internet search) would yield [Sk17o, Sk17-3] before arXiv submission of [FV21]. The authors of [FV21] were informed about [Sk17o, Sk17-3] and the criticism of [MW16] in June 2021 at Moscow Conference on Combinatorics and Applications. As of February, 2022, arXiv version of [FV21] is still not updated.

Example 3.3. Remark 2.4.b is illustrated by the following examples:⁸

- because of the way my name and work is mentioned in [MW16], I find it misleading that [MW16] does not mention that a pre-arXiv version of [MW16] was sent by the authors to me, I liked the idea of proof and had important specific criticism on its realization, but [MW16] was put to arXiv without fulfilling the recommendations 2.4.b.(2),(3); cf. [Sk17o].⁹

- because of the way M. Skopenkov’s work is mentioned in [FK17], I find it misleading that [FK17] does not mention that pre-arXiv versions of [FK17] were sent by the authors to M. Skopenkov, who liked the idea of proof and sent the authors important specific criticism on its realization, but [FK17] was put to arXiv without (3); the authors kindly sent some updates to M. Skopenkov, he answered with more critical remarks and encouragement, and the arXiv updates of [FK17] still do not mention that M. Skopenkov did not confirm that his criticism is reasonably resolved.¹⁰

- mathematicians would not find it misleading that [Ad18] does not mention that I had some specific criticism on the argument (their opinion might or might not change if they have learned of the criticism, see Example 3.1).

⁸When I saw the arXiv papers [MW16, FK17], I informed the authors of my opinion expressed in the bullet points and suggested to update the papers. When the corresponding updates will appear, I would be glad to remove these examples.

⁹ The papers [Sk17-2, Sk17o] are not results of a research competitive to [MW16], but are results of several-years attempts to help the authors recovering their gap. See the following letter of A. Skopenkov to S. Avvakumov, U. Wagner, I. Mabillard, from January 15, 2017.

When I wrote a remark to part III [added in 2020: to [AMS+]], I realized that the idea of recovering the gap in part II [added in 2020: to [MW16]] by smoothing, which I suggested early in 2015, works:

The smooth version of the Local Disjunction Theorem 1.16 [added in 2020: from version 2 of [AMS+]] is correct. Moreover, the smooth version implies (by approximation) the piecewise-smooth and then the PL version. The same holds for the non-injective version of the metastable Local Disjunction Lemma [MW16, Lemma 10]. Thus by proving the smooth non-injective version of the metastable Local Disjunction Lemma (using vector bundles), both the gap (of inaccurate use of PL block bundles) in [MW16] can be recovered, and the proof can be shortened.

¹⁰I am grateful to Rado Fulek for many fruitful discussions in spite of our differences on this point.

4. REVIEWING THE PEER REVIEW SYSTEM

All professions are conspiracies against the laity.
B. Shaw, The Doctor's Dilemma

Remark 4.1 (Why high quality of peer review is vital). Publication in a peer reviewed scientific journal is the main test for recognizing a result as reliable (see §1). Thus reliability standards are mostly set up by such journals. Besides, publications in such journals are used for jobs and grants distribution. Consequently, such journals practically rule the mathematical world.

Sometimes ‘official’ peer review standards differ from practice. See Examples 5.1, 6.4, and §6, where this is easy to see even to an outsider who does not has a vast experience of reading and/or writing referee reports. See also [CLM, Ab21]. Consequently, it is vital to recognize and correct such situations, see Remark 4.3.

Dissemination of research is not a reason for the existence of a peer reviewed journal (because research can be disseminated by the authors via the internet). However, scientists might want to attract attention to some results, but have lack of time and energy to follow the principles of Remark 4.2. Then it is important that the web page of a journal (or a publication) announces that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation. Publications in arXiv and computer science conferences are examples of this kind [Fo].

Without all that a journal degenerates to an instrument of redistribution of jobs and grants in a way obstructing the progress of science.

Remark 4.2 (Some principles of scientific discussion). (a) **Scientific truth is established by justification and reasoning**, not by authority, majority or administrative pressure. This is what makes science different from other respectable activities of thought (like religion, intelligence service, etc.). Thus criticism and responsibility for criticism are vital for scientific discussion. Without them reliability standards could not be kept. Suppression of criticism or irresponsible criticism contribute to the degeneration of an activity to a non-science. Therefore suppression of criticism or irresponsible criticism must be identified as such, and their negative influence on science must be overcome.

In particular, the following *impartiality principle* should be maintained: decisions ought to be based on objective criteria, rather than on bias, prejudice, or preferring to benefit one person over another for improper reasons.

Since technically the scientific truth is (or should be) established by publications in peer reviewed journals (see Remark 4.1), the same principles should be maintained for editorial decisions in peer reviewed journals.

However, there is nothing wrong if scientists/referees/editors present unjustified statements, when they explicitly write that these statements do not affect practical issues (like editorial decision), and are presented on a ‘take it or leave it’ basis.

An example of a competent justification that a paper does not meet the high standards of a particular journal (*Fundamenta Mathematicae*) is [Rep] (publication of this report is allowed by the Editor H. Toruńczyk). See examples of unjustified/wrong statements/judgements in referee reports and surveys (including unjustified statements that proofs in published papers are incorrect) in Examples 5.6.b, 6.4, 6.2 and [Sk16, Remarks 5.4, 5.5, 5.6, 5.9].

(b) **Confidential decisions should match public discussions.** Every substantial scientific argument affecting a confidential decision should be publicly available.¹¹ An example

¹¹Here a ‘decision’ is a decision by an editorial board, dissertation/diploma committee, etc. The *quantity* of persons publicly supporting some point of view need not be matched by a confidential decision.

on dissertation defence in Russia: if there are no public objections to a dissertation, but a dissertation committee votes against the dissertation, then the committee is dissolved. (I hope there are analogous rules or traditions in other countries.)

This is required for maintaining (a). In particular,

- criticism and different opinions should not be suppressed by administrative means (e.g. misusing the anonymous review system by writing biased negative reports on papers whose authors propose criticism and opinions different from the reviewer's);
- references to criticism and to different opinions should be cited but not suppressed (even if one disagrees with them).

I disapprove of what you say, but I will defend to the death your right to say it.

(The Friends of Voltaire, E. B. Hall)

See [Sk16, Remark 5.1.b]; e.g. compare [Sk16, §1] citing [BZ16], and [Sh18] citing [Sk16] to [BZ16, §1], [BS17, FS20] not citing [Sk16].

The anonymous review system gives a convenient frame for suppressing criticism, for irresponsible criticism, and for promotion of unreasonable opinions which do not withstand open discussion. See Examples 5.2, 5.5, in §6, and in [Sk16, Remark 5.3 and §5.4].

(c) **Decisions should be made carefully and critically studying the texts in question**, not blindly believing (charming or influential) people we know, cf. [Gr]. The texts in question are

- referee reports and authors' responses, for editorial decisions,
- research and survey papers, for distribution of credits.

This is required for maintaining (a,b).

(d) See examples explicitly showing that the principles (a,b,c) are maintained (sometimes in spite of attempts to violate them) in Examples 5.2, 5.4, 5.5, 5.7. These examples represent high standard of impartiality and freedom of scientific discussions maintained by the Editors.

Remark 4.3 (Mistakes in editorial decisions). (a) With all our concern for reliability, mistakes are unavoidable. Thus a characteristic of a peer reviewed journal is that the Editors recognize and correct their mistakes. Since most of the Editors' work is not immediately visible to a reader, failure to recognize and correct their mistakes even in a few cases indicates that the degeneration described at the end of Remark 4.1 takes place.

If a mistake involves *acceptance* of a paper containing unreliable result/proof, then the correction involves publication of errata in the same journal (whether by authors, or by Editors, or by a third party).

(b) If a mistake involves *rejection* of a paper because of an unreasonable referee report, I suggest the following procedure for keeping (or restoring) the journal's status of a peer reviewed journal. The rejection decision should be canceled, and consideration of the paper should be renewed. The paper should be accepted or rejected only upon a referee report (justifying its conclusions) sent to the author, not upon the Editors' opinion without sending a referee report.¹² Indeed, even a great expert can either make a mistake, or be biased, or be emotionally dependent on a less qualified/honest person (see Remark 4.2.c and [Sk16, §5]).

¹²Sometimes the initial submissions are rejected upon the Editors' opinion that the paper is not suitable for the journal. This policy has its advantages (referees do not waste their time on written explanations that the paper is not suitable; authors can soon send the paper to a different journal) and drawbacks (decision without a report is potentially unreasonable or even corrupt). It is outside our purposes to discuss this policy here, we only state that such a policy is clearly inappropriate after rejection of a paper because of an unreasonable referee report. If one report is unreasonable, and other reports are hidden, a scientist has to suppose that the other reports are also unreasonable.

So without sending a referee report to the author, the decision is way too unreliable for the status of a peer reviewed journal (described in Remark 4.1).

The conclusion that a referee report is unreasonable has to be ignored if it is not justified by specific criticism of specific phrases of the report. The same holds for any other conclusion in the discussion described below. If the conclusion is justified by specific criticism, then it should be sent to the referee, so that he/she could provide a revised report (taking into account the criticism), or explain why he/she disagrees with the justification, or refuse to reply (in the last case the conclusion is recognized to be correct). The referee's letter is sent to a person who justified that the referee's report is unreasonable (usually to the authors), and so on. The Editors can invite alternative referee for the paper or for the particular questions discussed. When the discussion converges, the Editors make their new rejection/acceptance decision. For an effective means to make the discussion responsible (and hence short) see Remark 4.4.b. If the Editors feel lack of time and energy to moderate such a discussion, *'it is important that the web page of a journal announces that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation'*, see Remark 4.1.¹³

Remark 4.4 (Transparency helps). (a) The transparent anonymous peer review policy [Skt] (perhaps introduced gradually and partly) would be helpful for maintaining the principles of scientific discussion (described in Remark 4.2). Transparency allows to diminish the gap between official information and practice, see §5 and §6.

(b) The transparent anonymous peer review policy [Skt] allows not to waste our valuable time on replying to incompetent texts. Those who write incompetent texts usually are competent enough not to allow publication of these texts on the internet, and such a policy involves ignoring texts not put on the internet.

(c) When a discussion is an argument rather than a collaboration, we strongly need it to be responsible. In such situations we usually do not have enough time to discuss premature ideas, whose invalidity becomes clear when their publication (or a mental experiment of publication) is suggested. (Such ideas usually, though not always, are euphemisms for 'I have more administrative power than you'.) Then it is useful to have a transparent discussion, in which letters are (allowed to be) published on the internet. If a participant of such a transparent discussion receives a letter not stated to be public, then he/she deletes it unread (to avoid confusion). If a part of such a public discussion becomes obsolete, participants can delete that part (only) by mutual consent.

(d) *'Public shaming is the only thing that can really work against groupthink. To spread the word, please LIKE this post, REPOST it, here on WP, on FB, on G+, forward it by email, or do wherever you think appropriate.'* [Pan]

5. REVIEWING THE PEER REVIEW SYSTEM: EXAMPLES

Conventions for §5 and §6. In letters and in citations from referee reports the references are updated. In spite of that, the references are given to the very version of a paper cited in

¹³The main purpose of this procedure is keeping (or restoring) the journal's status as a peer reviewed journal. If the authors wish that their paper (rejected because of an unreasonable referee report) should be reconsidered in the same journal, then this purpose is achieved together with the reconsideration. If the authors wish to submit their paper to a different journal, the above procedure still makes sense (because of its main purpose). Then the formal result of such a procedure is 'honorary rejection' or 'honorary acceptance' of the paper. The journal publishes the information on the result but not the paper, in order to comply with the copyright. The result is 'honorary reconsideration after revision' if the authors do not submit a revised version after a reasonable amount of time specified by the Editors.

the source. Otherwise sources are not changed (the grammar is not corrected). Quotations from referee reports are given in italics and in quotation marks. We quote all parts of the reports related to the rejection recommendation. We omit e.g. quotation of referee's description of current situation in the area before the appearance of the paper under review. For those referee reports that are both incompetent and play a crucial role in the Editors' decision, the complete texts are available upon request (in particular, to confirm that we did not omit any critical remarks justifying the rejection recommendation). If an unreasonable report did not play a crucial role for the Editors' decision, there is no need to justify my criticism by publishing my reply to the report. I omit cases before 2017 and those cases which did not involve a non-trivial discussion (i.e., those cases in which a paper was accepted after a reasonable amount of changes required by the referee).

Example 5.1. 'Official' peer review standards include reasonable checking of novelty of results. However, I do not know any peer reviewed journal which requires submitted papers to be publicly available on arXiv. If a paper is not publicly available on arXiv, then I consider novelty of its results as not being reasonably checked, see Remark 2.4. Observe that checking of novelty in the form putting a text on arXiv is necessary (although not sufficient) for a paper to be awarded with a *scientific prize* of the Moscow Mathematical Conference of High School Students, see https://mccme.ru/mmks/index_eng.htm.

Example 5.2 (the report on handling of (a previous version of) [Sk16] in Russian Math. Surveys). One of the referee reports misused the anonymous peer review system to promote an unreasonable opinion which does not withstand open discussion, see [Sk16, §5]. *I am grateful* to Editor S. Shlosman for *publicly criticizing* in [Sh18] the survey [Sk16] because this allows to see that the criticism exists and is incompetent (see [Sk16, Remark 5.6]). *I am grateful* to the Editors for publication of [Sk16] without correcting the passages criticized by S. Shlosman, and publishing S. Shlosman's criticism in the same volume [Sh18].

Example 5.3 (the report on handling of [CS16] in Algebr. and Geom. Topol.). To appear.

Example 5.4 (the report on handling of [CS16] in Moscow Math. J.; A. Skopenkov). One of the referees (Alexey Zhubr) kindly disclosed his identity to facilitate the discussion of the paper. He presented important criticism and justly asked for a major revision.

In response to his report made after such a revision the authors wrote to the Editors, 'The referee of our paper Alexey Zhubr kindly forwarded to us his report of 19.09.2019. We are grateful to him for a thorough reading of our paper, many specific suggestions he made in earlier reports, and a recommendation (however reserved) to publish this paper.'

The critical conclusions of the latest report are not justified by references to the paper and suggestions what could be done in a more clear way. So we can only state that we disagree with that conclusions, that sections 3 and 4 are mostly hard to read because of complexity of the matter (not because of poor exposition), and that we invested several years to write this paper in a clear way. In our opinion, a referee should be requested to justify his critical conclusions only if the Editors think the conclusions could affect the acceptance of a paper. Otherwise the matter could be dropped.'

Another referee presented minor but valuable critical remarks. The paper was accepted without further revision.

Example 5.5 (the report on handling of [Sk18] in Arnold Math. Journal). There were two initial reports on the paper. The first was positive, it contained important criticism and asked for a major revision. The second was negative, it misused the anonymous peer review system to promote an unreasonable opinion which does not withstand open discussion, see

[Sk16, §5]. In my reply to the Editors, I justified the latter judgement by considering the referee's comments one by one. The Editors suggested a major revision (even before receiving my criticism of the second report). I do not know whether the Editors sent my criticism to the second referee or not. I received no reply to my criticism from the second referee. The paper was accepted after several iterations of work upon suggestions of the first referee.

Example 5.6 (the report on handling of [PS20] in Discr. Comp. Geom.). Our review is presented in the form of our letter of 19.12.2021 to the Editors J. Pach and K. Clarkson.

(a) *Dear Janos, Dear Kenneth,*
Hope you are fine and healthy.

Please find after our signatures [added later: in (b)] our report to review #2 on our paper [PS20] recently rejected from DCG. The report shows that the review is incompetent.

This paper is not among our best papers. Knowing how busy you are, we are not asking you to reconsider the rejection decision. However, it is important to recognize incompetent reports as such, and nullify their influence. Now that papers can be distributed via the internet, the only reason for the existence of scientific journals is high-quality peer review. So it would be nice if you could

- (1) put the incompetent review to databases (used by other journals) only together with our report.
- (2) be more careful with the reviews of the same referee to other papers.
- (3) send our report to the referee so that he/she could see that the incompetence is checked (or could object to our report, if he/she could).

We will not object if you would choose to reconsider the rejection decision. The other review #1 is positive. It leaves to the Editorial Board the judgment whether the paper is sufficiently related to DCG. This judgment is done by regular publications in DCG of paper on algebraic and geometric topology (related to discrete mathematics), e.g. arXiv:1302.2370, arXiv:1307.6444, arXiv:1512.05164, arXiv:1703.06305, arXiv:1808.08363. Please also observe that our paper was cited (even before publication) in the paper arXiv:2108.02585 on discrete geometry and computer science.

[A technical paragraph omitted]

Best Regards, Salman and Arkadiy

(b) *A report to the review #2:*

The first sentence of our paper is

'We present short and clear proofs of Theorems 1 and 3.b which first appeared in the unpublished paper [MS06, (iv) \Rightarrow (i) of Corollary 4.4, Theorem 4.5].'

The second sentence of the review *'The paper presents a proof of a particular instance of a result by Melikhow-Schepin'* repeats the above first sentence of the paper in a misleading form. (The 'short and clear proofs' [of the current paper] and 'unpublished' [paper of Melikhow-Schepin] are missing.)

[a paragraph on 'minor errors' in another paper is omitted]

The third sentence *'Furthermore, the paper looks like a "piece/subsection taken out of an actual research paper"'* instead of justifying the judgment of the review, gives the paper a negatively sounding name ('piece/subsection') insinuating that the paper is not a research paper. However, the third sentence does not explicitly state that the paper is not a research paper (because it clearly is), does not explain why the paper is not a research paper, does not describe what is 'piece/subsection', and does not explain why the paper is such. The reviewer did not refer to the actual paper of which our paper is a subsection. Every short (or long) paper could be in principle a part of a larger paper. Thus the third sentence is a logical fallacy.

The remaining three sentences of the review are (partly repeated) judgements based on the above-discussed second and third sentences. Thus the review attempts to justify that the paper does not meet the standards of this journal, but fails to do so.

Example 5.7 (the report on handling of [PS20] in Topol. Appl.). The initial report was incompetent. This was shown in our reply to the report, and this was not questioned (cf. Example 5.6). The Editors considered different opinions on the paper. The paper was accepted after work upon suggestions of an alternative referee.

6. VIOLATION OF IMPORTANT SCIENTIFIC PRINCIPLES

Example 6.1 (introduction). (a) In this section I justify that principles of scientific discussion (described in Remark 4.2.ab) are violated in Israel J. Math. and in Proc. of the Amer. Math. Soc. The papers [Sk17-2], [AKS-s], and [AKS-2] were rejected from Israel J. Math., Israel J. Math., and Proc. of the Amer. Math. Soc., respectively, based on incompetent reports. The incompetence is justified in Example 6.2, 6.4, and 6.7. The justification was sent to the Editors and not questioned by them.¹⁴ This violates the principles of Remark 4.2.ab.¹⁵

The papers [Sko, Skm] and Example 6.10 present examples of suppression of criticism by administrative means, and of the difference between official information and practice, violating these principles. So

- Israel J. Math. and Proc. of the Amer. Math. Soc. are not *peer reviewed* journals,
- Department of Differential Geometry of Faculty of Mechanics and Mathematics of Moscow State University is currently not a *scientific* department, and
- the Möbius contest is currently not a *scientific* contest.

These organizations are instruments of redistribution of jobs and grants in a way obstructing the progress of science.

It is important for the community to know that. Also, this would hopefully be useful for the Editors of Israel J. Math. and Proc. of the Amer. Math. Soc., either

- to restore the status of a peer reviewed journal (see Remark 4.3), or
- to publicly explain why they disagree with specific parts of Remarks 4.1, 4.2, 4.3, or
- to announce at the web page of the journal *‘that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation’* (see Remark 4.1).

For analogous reason this would hopefully be useful for the chair and members of the department, and for the president and members of the contest.

(b) *‘Violation of important scientific principles ... is a consistent policy of an influential group in the community of topological combinatorics’* [Sk16, Remark 5.3] (see justification in [Sk16, Remark 5.3]). Material which is reliable, openly unopposed, and referring to the different (however misleading) point of view is not published, while material which is misleading, publicly criticized, and failing to refer to public criticism, is published, see [Sk16, Remark 5.3]. This jeopardizes the peer review system.

¹⁴ The rejection decision on [Sk17-2] was confirmed, referring to other experts’ opinion, but without presenting any new referee report; no report was sent upon my request of Example 6.3.b. A private Editor’s reply to our letter of Example 6.8.a did not demonstrate understanding of this problem, and did not suggest or accept any way of resolving the problem.

Additionally, the impartiality principle of Remark 4.2.b is violated for [Sk17-2] because a similar paper [AMS+] containing a *weaker* result was published in the Israel J. Math.

¹⁵No public reply to our letters of Examples 6.3, 6.6, 6.8 are available by May, 2022. Therefore these examples are gradually moved from §5 to §6. Public replies, if available, will be presented in this paper.

Examples 5.2, 5.5, in §6, in [Sk16, §5] show that this group (or mathematicians blindly believing private judgements or publications by this group) attempts to misuse journals as instruments of redistribution of jobs and grants in a way obstructing the progress of science. The examples are different: Russian Math. Surveys and Arnold J. Math. are not such instruments, some journals have a chance to correct mistakes exhibited to Editors (see Remark 4.3.a), while Israel J. Math. and Proc. of the Amer. Math. Soc. remain such instruments even after mistakes are exhibited to the Editors.

Example 6.2 (the report on handling of [Sk17-2] in Israel J. Math.; 29.01.2021). [Added later: The paper was rejected on the basis of a referee report shown to be incompetent below (cf. footnote 14 and Example 6.3). The referee's specific critical remarks are either

- attempts to misuse the anonymous peer review system to promote an opinion which does not stand an open discussion (and thus violating principles of scientific discussions recalled in Remark 4.2.b), see quotations in (A,E) below, or
- misleading judgements based on incomplete quotations of the manuscript, see quotation in (D) below, or
- statements and judgements irrelevant to the rejection recommendation, see quotations in (C,F) below.]

Most important replies are (A)-(F); more technical replies are (F1)-(F5) and (G).

(A) ‘1. The current manuscript does not give any reasons why the author finds the proof in [MW16] unsatisfactory. Instead the reader is pointed to reference [Sk17o], an arXiv preprint that quotes emails between the author and Wagner. In [Sk17o] the author gives a list of reasons why the proof of [MW16] should be regarded as incomplete. Even if the treatment in [Sk17o] was satisfactory and well-reasoned (it is far from that), since it is in a different manuscript that presumably is not undergoing peer-review, the reader is simply asked to take the author's word for the incompleteness of [MW16]. This is unacceptable...’

The referee does not justify the judgement that the treatment in [Sk17o] is far from being satisfactory and well-reasoned (presumably because the treatment is satisfactory and well-reasoned). This is too strong a judgement to be used without justification for rejection recommendation. The more so because an author of [MW16] wrote ‘I agree with many of the specific criticisms ... I think these are helpful and valid criticisms, and we will address them in the next revision’ [Sk17o, Remark 4], and then no next revision appeared in 3 years. Cf. arXiv:2101.03745v1, Remark 1.3.b.

‘The reader is simply asked to take the author's word for the incompleteness of [MW16]’ is wrong because

‘In [Sk17o] the author gives a list of reasons why the proof of [MW16] should be regarded as incomplete.’

In any case, these remarks of referee cannot justify the rejection recommendation because of (B) below.

I did not include the relevant part of [Sk17o] into the submission because of (B) and (F3) below.

(B) The manuscript makes the priority questions irrelevant to its publication by naming Theorem 1.2 Metastable Mabilard-Wagner Theorem.

Remark 1.4 of the manuscript [Sk17-2] explains in mathematical terms (i.e. without an attempt to distribute credits) the relation of the manuscript to earlier papers (including [MW16]). This remark is not questioned by the referee.

A reader is mostly interested in having a reliable reference, not in distribution of credits. In my opinion, the proof in the manuscript is so short (comparatively to the proof of [MW16])

that it is interesting to a reader independently of his/her judgement on how serious are the gaps in [MW16] described in [Sk17o].

No part of [Sk17-2] concerns the question whether [MW16] is a reliable reference or not. Only §5 of [Sk17-3] concerns this question. I added of [Sk17-3, §5] upon criticism of the referee (see (A)) and I am ready to remove it upon request.

(C) ‘1. If the current manuscript is not supposed to be regarded as an alternative proof but instead as the first complete proof of the main result, a critical treatment of [MW16] and its perceived gaps is unavoidable.

2. There is a possibility that the current manuscript takes credit away from the authors of [MW16]...’

These remarks cannot justify the rejection recommendation (and the second sentence is incorrect) because of (B).

(D) P. 1 [added in 2022: of the report]. ‘The proof given in the present manuscript follows roughly the same ideas as in [MW16] but is different:...

2. ... The manuscript under review borrows proof ideas (and the statement of the main result) from [MW16]. For example, in Remark 1.4(b) one reads “Proofs in this paper and in [MW16] are similar...”.’

These judgements are misleading (at least without reference to detailed explanation in Remark 1.4(b)) and are based on incomplete citation. Indeed Remark, 1.4(b) reads

‘Proofs in this paper and in [MW16] are similar because they use and extend known methods. Among these methods are

- for Theorems 1.1 and 3.1: Surgery of Intersection Lemma 2.2 (in [MW16] this method was used without the references presented in the proof of the Surgery of Intersection Lemma 2.2 but with a reference to the ‘predecessor’ paper [Mi61]); ...

The new parts of proofs in this paper and in [MW16] are essential and are different.’

See also [Sk17-3, footnote 5]:

‘...In [MW16, §4.1] this surgery of double intersection was used with a reference to the ‘predecessor’ paper [Mi61], but without the above references... Lack of the above references in [MW16] may result in exaggerating the overlap of methods of [MW16] and of this paper.’

(E) ‘2. ... Thus this will have to be handled differently, so that the authors of [MW16] may receive due credit.

The referee’s justification that the current manuscript does not give due credit to [MW16] is incorrect, see (B) and (D) above. However, I am flexible in terms of credit distribution (although I am keen on mathematics behind credit distribution). So I am willing to give more credit to [MW16] upon referee’s suggestion of a specific phrase (if he/she thinks that [Sk17-2, Remarks 1.4.ab] are not proper or insufficient). The referee did not state any such suggestion.

(F) ‘3. The expository quality of this manuscript is low. For example,

— Several notions are not defined, and the reader is not directed towards a suitable reference. Examples include “stably parallelizable manifold” or “ Σ_r -equivariant map,” where the notation Σ_r is not introduced.

— The reader is not guided through proofs in a coherent manner. The proof of Lemma 2.2 ends with “So the lemma is proved analogously to [HK98, Theorem 4.5 and appendix A], [CRS, Theorem 4.7 and appendix], cf. [Ha63], [Ha84, Lemma 4.2], [Me17, proof of Theorem 1.1 in p. 7] [AMS+, The Surgery of Intersection Lemma 2.1]; for detailed descriptions of the above references see [AMS+, Remark 2.3.a].” This is not helpful exposition.’

‘— On p. 5 one reads “By the Hirsch-Smale-Gromov density principle [Gr86, Corollary in §1.2.2.A]...” This should be stated explicitly.’

‘— On p. 3 one reads “The following result implies the existence of a polynomial algorithm for checking almost r -embeddability in R^d for fixed k, d, r such that $rd \geq (r + 1)k + 3$, cf. [MW16, Corollary 5].” This should be explained. It seems the work in preparation referenced in [MW16] never appeared even in preprint form.’

‘— Abstract and p. 2: “The r -fold analogues of Whitney trick were ?in the air? since 1960s.” What does that mean? The r -fold analogues of the Whitney trick were formulated and proved by Mabillard and Wagner.

[The above citation of the report is added later]

The suggestions on exposition in item 3 of the report are reasonable or at least acceptable (except the last one, see (F5) below). However, these suggestions cannot contribute to the rejection recommendation because

- the referee did not describe any problems with applying well-known argument for proving Lemma 2.2;
- the suggestions are easy to fulfill, see the attached version;
- of (F1)-(F5) below.

In [Sk17-3] I added all the details the referee asked (including a proof of Lemma 2.2). I am willing to remove them upon request.

The referee’s critical remarks from item 3 should be compared to [Sk17o, Remark 3]. The referee’s using item 3 for rejection recommendation should be compared to his/her judgement that the criticism in [Sk17o, Remark 3] is far from being satisfactory and well-reasoned, see (A).

Items (F1)-(F5) below reply to the suggestions on exposition in item 3 of the report.

(F1) The notion of a stably parallelizable manifold is a textbook notion.

(F2) The notation Σ_r is well-known and/or easy-to-guess to mathematicians working in the area.

(F3) Journals usually have restrictions on lengths of papers. One of my papers was accepted only on the condition that some details of the argument will be left out and kept only in arXiv version. Referee’s recommendations to give more (or to give less) details are welcome. Recommendation to give more details can contribute to the rejection recommendation only if the referee justifies that the missing details are important (which is not so in the current report). There is nothing wrong that different referees in different journals have opposite recommendations, but only if such recommendations are not used to justify the rejection decision. The rejection decision should be based on the mathematical quality of a paper, not on the author’s ability to predict the referee’s wish to have more (or to have less) details.

(F4) The sentence citing [MW16, Corollary 5] does not affect the main result.

(F5) Why ‘the r -fold analogues of Whitney trick were in the air since 1960s’ is explained in the survey [Sk16] cited at the end of the relevant paragraph of [Sk17-2], see [Sk16, Remark 3.6]. In 2017 U. Wagner kindly confirmed that he thinks this remark is proper.

(G) ‘2. ...If, however, there is a serious flaw in [MW16], then the as of now unpublished [MW16] should not appear in its current form.’

This is unclear because this is not a correct English sentence. Presumably this phrase refers to an update of [MW16]. Since such an update is not available to math community, the phrase is not relevant to the review.

Example 6.3 (Letters to the Editors of Israel J. Math. on [Sk17-2]). (a; 29.01.2021)

Dear Editors,

Hope you are fine and healthy.

Below [added in 2022: in Example 6.2] please find my reply to the (attached) referee report on the manuscript

‘Eliminating higher-multiplicity intersections in the metastable dimension range’
rejected from Israel Journal of Mathematics (IJM).

This reply shows that the report is incompetent. Remark 5.3 of [Sk17-3] shows that the report misuses the anonymous peer review system to promote an opinion which failed to be substantiated in an open discussion. So could you please consider if the following would allow to maintain high reputation of IJM (see arXiv:2101.03745v1, Remark 2.2.Sk17 and <https://scirev.org/>):

- (1) abolishing the rejection decision;
- (2) sending the manuscript (whether the version submitted in 2020 or the attached one [added in 2022: [Sk17-3]]) to an alternative referee;
- (3a) sending my reply to the referee and asking him/her to present a revised report taking into account critical remarks presented in my reply (the new version of the report can be written either on the version submitted in 2020 or on the attached one);
- (3b) disregarding the referee report in case he/she would not provide such a revised report within time range corresponding to the standards of IJM;
- (3c) if the Editors consider misusing the anonymous peer review system to promote an opinion which does not stand an open discussion to be offence serious enough to disregard the referee report, then I do not insist on (3a) and (3b).

I do understand that moderation of a potential dispute between the referee and the author could be outside of purposes of the Editors. So observe that most part of this dispute is irrelevant to publication of the submitted manuscript, see Example 6.2.B below.

I am open to compromise, e.g. to treat the attachment as a new submission if this would not result in delay of its refereeing.

Perhaps I should inform you that I am completing analogous reply to the referee report on the paper [AKS-s] rejected from IJM. In that reply I justify that the report misuses the anonymous peer review system to promote an opinion which failed to be substantiated in an open discussion.

Sincerely yours, Arkadiy Skopenkov, <https://users.mccme.ru/skopenko/>.

(b; 26.03.2021) Dear Editors,

Hope you are fine and healthy.

Thank you for your letter. Could you send me the referee report upon which your rejection decision of March 18 is based?

Recall that your previous rejection decision is based on an incompetent report that misuses the anonymous peer review system to promote an opinion which failed to be substantiated in an open discussion. This judgement is justified in my letter of January 29 and is not questioned in your reply of March 18. In your letter of March 18 you did not state that your previous rejection decision is retracted (so that your new rejection decision is based only on the new report). Could you please consider if making such a public statement would allow maintaining a high reputation of IJM (see arXiv:2101.03745v1, Remark 2.2.Sk17 and <https://scirev.org/>). For the math community it is important to know whether the anonymous peer review system is misused in IJM (cf. arXiv:2101.03745v1, Remarks 2.2.Sk16 and 2.2.Sk18 on Russian Math. Surveys [added in 2022: and on] Arnold J. Math.). This is important independently of the journal’s final decision on a particular paper.

Sincerely yours, Arkadiy Skopenkov, <https://users.mccme.ru/skopenko/>.

(c) 17.03.2022, to Michael Temkin, Editor in chief, and the Editors

Shalom Michael, Shalom Editors,

Hope you are fine and healthy.

Michael’s letter of 29.03.2021 is delightful in a personal sense, because it sincerely attempted to resolve the problem in a friendly way, and Michael spent his valuable time to convince more experts to give their opinion.

Michael’s letter of 29.03.2021 is disappointing in a professional sense, because it shows that Israel J. Math violates important principles of scientific discussions exposed in Remarks 4.1, 4.2, 4.3. This is justified in (a,b) and in Example 6.2. In particular, the more experts confirm the

editorial decision based on an incompetent referee report without producing a new report sent to the author, the farther is the journal from a peer reviewed journal.

Your reaction suggested in Example 6.1.a would be important. E.g. you can

- restore the status of a peer reviewed journal (as suggested in Remark 4.3), or
- publicly explain why the Editors disagree with specific parts of Remarks 4.1, 4.2, 4.3, or
- announce at the web page of the journal *‘that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation’* (see Remark 4.1; this can be done implicitly by presenting no public reply to this letter).

Please find the pdf of this message at https://www.mccme.ru/circles/oim/rese_inte.pdf. This message is a part of transparent public discussion as described in Remark 4.4.c. So in order to avoid confusion, could you read that remark before replying to this letter.

Sincerely Yours, Arkadiy Skopenkov.

Example 6.4 (the report on handling of [AKS-s] in Israel J. Math.; R. Karasev and A. Skopenkov; 17.03.2022). The paper was rejected on the basis of a referee report shown to be incompetent below (cf. Example 6.6). The referee’s specific critical remarks are either

- attempts to misuse the anonymous peer review system to promote an opinion which does not stand an open discussion (and thus violating principles of scientific discussions recalled in Remark 4.2.b), see (c,e1,t1,v1) below, or
- wrong or unjustified statements and judgements, see (a,d,e,f,g,e1,f1,j1,k1,l1,o1,p1,q1,r1,s1,t1,u1,v1,w1) below, or
- attempts to create an impression of presenting a justification, without explicitly calling a justification something which does not stand the test of being called such,¹⁶ see (b,g,e1,l1,p1,w1) below.
- statements and judgements irrelevant to the rejection recommendation, see (d,b1,c1,f1,g1,m1,u1,x1,y1) below, or
- valid critical remarks that cannot contribute to the rejection recommendation, see (d1,n1,w1) below.

The most important critical remarks to the report are (a)-(g) below; more technical remarks are (b1)-(y1) below (the numeration starts with b1 not a1 for technical reasons).

The reaction of Israel J. Math., whenever available, will be published here. This reaction will hopefully contribute to showing that taking editorial decisions on the basis of incompetent referee reports is not a consistent policy of the journal. See §4 and §6.

Below the notation and references in the referee report are changed to agree with the notation of [AKS-s], so as to avoid confusion; otherwise the report is not changed. Page references are to pages of the report. In spite of our criticism, in some places we would agree to change phrasing, but the referee does not provide any suggestion (except for suggestions violating the principles of scientific discussions recalled in Remark 4.2.b, see e.g. (c) below).

(a) P. 3. *‘The main result of the paper, Theorem 1, for certain class of parameters improves the gap (1) as a function of ‘dimension’ and ‘number of intersections’ compared to the gap given in [BFZ]. This improvement does not give any new insight on [BFZ, Conjecture 5.5]. Hence, the level of the results of the paper does not meet the high standards of the Israel Journal of Mathematics and so, based only on this fact, I suggest to the editors of the journal to reject the paper.’*

¹⁶This could be not an intentional attempt to mislead the Editors, but lack of responsibility, and so lack of understanding the necessity of properly structuring the report, explicitly writing which judgements are justified by which specific remarks.

The main judgement of the referee report ‘does not give any new insight’ is not justified. The report calls this *judgement a fact*, which is a logical fallacy. In [AKS-s, Remark 2.b] we do explain why we do give a new insight:

‘We think counterexamples of Theorem 1 are mostly interesting because their proof requires non-trivial ideas, see below’.

The referee suppresses the reference to this explanation in the above paragraph of the report, presumably because his judgement ‘does not give any new insight’ does not withstand the test of being read next to this explanation.

(b) The paragraph of the report quoted in (a) states that the rejection recommendation is based *only* on the (unjustified) judgement ‘*does not give any new insight*’. After that, three pages of critical remarks are presented, starting with ‘About the evaluation of this paper’. Thus the relation of these three pages to the rejection recommendation is unclear.

(c) P. 1. ‘*Only recently, combining the work of Mabillard-Wagner [MW14] and Blagojević-Frick-Ziegler [BFZ14], Florian Frick in [Fr15] presented counterexamples to topological Tverberg conjecture in the case when n is an arbitrary non prime power.*’

This is misleading, and is publicly criticized in [Sk16, Remarks 1.9, 5.3 and §5] (most of the material of [Sk16] cited here is available in earlier arXiv versions of this survey, and was even sent to the main ‘players of the game’ before putting it to arXiv.) For the reader’s convenience, we quote [Sk16, Remarks 1.9] as Remark 6.5. Justified criticism of the description of references to the counterexample presented in [Sk16] is not publicly available, see details in [Sk16, Remark 5.3].

Thus the report attempts to misuse the anonymous peer review system to suppress the clear description of references concerning the topological Tverberg conjecture, presented in [Sk16], presumably because that description is way too sound to be publicly criticized. (See below description of further such attempts.)

We made the paper [AKS-s] independent of that discussion, by citing in [AKS-s, Remark 2.a] the surveys on the topological Tverberg conjecture containing different description of references, including [BZ16] which contains a misleading description [Sk16, Remark 5.7]. In the new version [AKS-2] we added references [BBZ, BS17, Sh18], although they do not give anything new to that description comparatively to [BZ16], and contain misleading descriptions [Sk16, Remarks 5.4, 5.5, 5.6].

P. 5. ‘*the reference [Sk16]: only published version should be quoted, otherwise this reference contains 4 or 5 different manuscripts,*’

It is usual to have many versions on arXiv and to cite a paper meaning the latest version.

P. 3. ‘*In Remark 2(a), which follows after Theorem 1 and offers motivation, the authors, for the topic of the counterexamples to the topological Tverberg conjecture, quote ‘surveys [BZ16, Sk16]’. The proper way would be to cite the original publications of the counterexamples [Fr15] (announcement) and [BFZ] (journal version).*’

The papers [Fr15, BFZ] are not the original publications of the counterexamples, at least not the only ones, as explained in [Sk16, Remark 1.9, 5.4.abef, 5.5abcd,].

P. 4. ‘*In the paragraph after Remark 4, as an explanation for Theorem 5, counterexamples to the r -fold van Kampen-Flores conjecture are just mentioned and not explained. Three papers are quoted in relation to this topic, ignoring the papers where the first counterexamples to the r -fold van Kampen-Flores conjecture appeared.*’

The counterexamples are not just mentioned, but are explained, i.e., are stated together with proper references. The referee does not name ‘*the papers where the first counterexamples to the r -fold van Kampen-Flores conjecture appeared*’, presumably because if they would be

named, it would be clear that it is unfair to name them as original references, see [Sk16, Remarks 1.9.b, 5.4.e, 5.5.b].

We added in [AKS-2] as a further explanation, the reference [MW14, §1, Motivation & Future Work, 2nd paragraph] to the extended abstract well-known to the people in the area and cited by the referee.

(d) P. 2. ‘*This paper has only one main result stated in Theorem 1.*’

This statement is irrelevant to the rejection recommendation (and is also wrong because there are [AKS-s, Theorems 5 and 6]).

(e) P. 2. ‘*Theorem 6 is a particular case of [Ba93, Theorem 3.6 and paragraph afterwards].*’

This is wrong. Indeed, [Ba93, Theorem 3.6] takes a group G from a certain class and proves that there exists *some* representation W of G , for which there exist G -equivariant maps $X \rightarrow S(W)$ for certain G -spaces X . However, $G = \mathfrak{S}_n$ does not satisfy the conditions of [Ba93, Theorem 3.6]. Also Theorem 6 states that for a specific group $G = \mathfrak{S}_n$ there exist G -equivariant maps from X to a *specific* representation of G , possibly not the one given by [Ba93, Theorem 3.6].

(f) P. 2. ‘*The result of Lemma 10 is known, ...*’

Since the report presents no reference to the result of [AKS-s, Lemma 10], one has to treat this statement as incorrect.

P. 2. ‘*...and can also be deduced from the existence of a Σ_r -map $S_{\Sigma_r}^{2r-3} * \Sigma_r \rightarrow S_{\Sigma_r}^{2r-3} \dots$* ’

The existence of a Σ_r -map $S_{\Sigma_r}^{2r-3} * \Sigma_r \rightarrow S_{\Sigma_r}^{2r-3}$ is trivially equivalent to non-trivial [AKS-s, Lemma 10]. The referee neither proves the existence nor gives a reference for it. This should be compared to the referee’s critical remarks on [AKS-s, proof of Lemma 10], see (w1) below.

(g) P. 3. ‘*The paper is written in a very unprofessional and unclear way, with incomplete proofs, and missing relevant references. The state of the paper under review was to me particularly puzzling after considering the paper, referenced by [AK19], authored by Avvakumov and Karasev (2/3 of the present authors). The difference in quality is stunning.*’

These judgements are not justified. Neither it is explicitly written that they are justified by critical remarks presented below in the report, presumably because it is too clear that they are not justified. Observe that [AKS-s] presents a detailed (however imperfect) proof of [AKS-s, Lemma 10], comparatively to the reference to a picture in an analogous situation in [AK19]. Thus this critical remark of the referee is inconsistent with his/her criticism from (w1) below.

(b1) P. 2. ‘*First, Theorem 1 does not solve any case of the [BFZ, Conjecture 5.5] — as the authors state in the sentence which breaks from the first to the second page of the manuscript.*’

Even if a paper does not completely solve any case of motivating conjectures or problems, the paper could be an interesting or significant contribution to them. The referee does not justify that [AKS-s] does not contain such a contribution. See (a,b) above.

(c1) P. 2. ‘*Second, in the case when $\frac{d+3}{r} < \lceil \frac{d+2}{r+1} \rceil$ the result of Theorem 1 is an elementary fact, any affine map in general position yields and almost n -embedding even without any condition on n In some other situations the gap given in [BFZ, Lemma 5.2] is greater than the one given by Theorem 1.*’

The existence of trivial / known particular cases does not contribute to making a theorem uninteresting. See the following phrase at [AKS-s, bottom of p. 1]:

‘Theorem 1 provides even stronger counterexamples to the topological Tverberg conjecture: **for d large compared to r ...**’

See also the phrase of [AKS-s, Remark 2b] cited in (a) above.

(Note that [BFZ, Lemma 5.2] does not explicitly state any counterexamples, so the phrase ‘the gap given in [BFZ, Lemma 5.2]’ is meaningless. No examples of ‘is greater’ is presented either in [BFZ] or in the referee report.)

(d1) P. 2. ‘Lemma 9 is a special case of [Ba93, Lemma 3.9] where Y is taken to be the empty set.’

P. 5. ‘After Lemma 9, a reference towards [AK19, §5] is given for the proof and relevant information. As explained Lemma 9 is a special case of [Ba93, Lemma 3.9].’

We are grateful to the referee for bringing [Ba93, Lemma 3.9] to our attention. We have added the following sentence to [AKS-2]:

‘In particular, this lemma follows from [Ba93, Lemma 3.9], although to read the direct proof in [AK19, §5] is simpler than to find the notation required for [Ba93, Lemma 3.9] and make such a deduction.’

Since [AKS-s, Lemma 9] is not a result of [AKS-s], but is attributed to [AK19], the above remark of the referee cannot contribute to the rejection recommendation.

(e1) P. 3. ‘I would like to point out that authors systematically quote their own papers for the results of others, which cannot be seen as professional.’

This is wrong, and is not justified by any reference to the paper under review.

For the counterexamples to the topological Tverberg conjecture, we quote the surveys [Sk16, BZ16]. The surveys contain both original references and different description of them. This is done in order to make this paper independent of the discussion of such a description, cf. (c) above. We do not exclude from our citations the survey [BZ16] written by other authors, and containing a misleading [Sk16, Remark 5.7] description. This cannot be called ‘authors systematically quote their own papers for the results of others’.

(f1) P. 3. ‘The title of the paper says ‘Stronger counterexamples. . .’. It is unclear what do you mean. As already explained one way to talk about this is to analyze the gap (1), but only as a function of ‘dimension’ and n . Otherwise there is are new ‘stronger’ counterexamples.’

What we mean is the usual ‘stronger than previously known’. This is explained more precisely in the following sentence of [AKS-s, Remark 2a]:

‘Theorem 1 provides even stronger counterexamples to the topological Tverberg conjecture: for d large compared to r we have $N > (d + 1)(r - 1)$ and even $N > F$.’

The referee’s phrase “Otherwise there is are new ‘stronger’ counterexamples” does not make sense to us.

(g1) P. 3. ‘Thanks should be given in a standard way, not by appending ‘*’ to the title.’

This could be fixed during the copy editing process if the paper is accepted.

(h1) P. 3. ‘The abstract should concisely present what is done in the paper and what is actually new...’

This is done, and the referee does not justify that this is not done. The Theorem stated in the abstract is new. In [AKS-2] we added the following phrase to the abstract:

‘This was improved in 2015 by P. Blagojević, F. Frick, and G. Ziegler using a simple construction of higher-dimensional counterexamples by taking k -fold join power of lower-dimensional ones.’

(i1) P. 3. ‘... In particular, please indicate precisely in what sense your counterexamples are new and stronger than done in previous work.’

Since the relation involves technical formulas, we present it in [AKS-s, Remark 2a], not in the abstract.

(j1) P. 3. ‘Please unify the notation used in the first paragraph of the paper, the abstract and the statement of Theorem 1.’

The referee indicates no conflict of notation, and we could not find it.

(k1) P. 3. ‘It is unclear what K and Δ_N are.’

This is explained in the first two sentences of the paper (see also Remark 4).

(11) P. 3-4. *'This presentation of fairly standard notions is very confusing. In Matoušek's book [Ma03] these notions are presented well.'*

The judgement of the first sentence is not justified. The fact that some exposition is good does not show that an alternative exposition is 'very confusing'. We wrote the paper assuming that the reader of Israel J. Math. need not have studied the book [Ma03].

(m1) P. 4. *'In Remark 2(a), you talk about [BFZ, Conjecture 5.5], but do not fully state it or explain it.'*

We do fully state the unknown part of [BFZ, Conjecture 5.5] in Remark 2a. We omit the known part of the conjecture (which is thus not a conjecture, but a result), i.e., the first line of [BFZ, Conjecture 5.5].

(n1) P. 4. *'In the first sentence of the second paragraph of Remark 2(a), you claim: 'This problem was considered in [BFZ, §5] cleverly using the pigeonhole principle.' This is inaccurate, the idea is different.'*

In [AKS-2, Remark 2a] we changed our phrase to

'This problem was considered in [BFZ, §5], where higher-dimensional counterexamples were constructed from lower-dimensional ones'

and added a short construction accessible to non-specialists.

(o1) P. 4. *'In Remark 2(b), you explain why you do not compare the results you prove with the already known results.'*

This is wrong. We do compare in [AKS-s, Remark 2a] the results we prove with the already known results. In [AKS-s, Remark 2b] we explain a different thing: '...We do not spell out even stronger counterexamples which presumably could be obtained by combining the procedure of [BFZ, §5] with Theorem 1.'

(p1) P. 4. *'The argument says 'We think counterexamples of Theorem 1 are mostly interesting because their proof requires non-trivial ideas, see below.' I dislike the term 'nontrivial idea' which is subjective, and implicitly implies that previous work is trivial.'*

The words 'nontrivial idea' are common usage in introduction to research papers. For a reader, it is important to know the authors' understanding of what is simpler and what is more complicated. It is correct that the proof of Theorem 1 of [AKS-s] is more complicated than [BFZ, Lemma 5.2], see a short exposition of [BFZ, Lemma 5.2] accessible to non-specialists in [AKS-2, Remark 2a].

The referee did not explicitly state the opposite (only hinted that), presumably because the opposite does not withstand the test of being explicitly stated and looked upon.

(q1) P. 4. *'In Remark 4 (motivation) a definition of a simplicial complex and its geometric realization is given. First, a definition is called 'Remark' and in addition '(motivation)' is added for complete confusion.'*

Remark 4 of [AKS-s] is not a definition but is a remark containing definitions, a convention, an observation, and information. The word '(motivation)' is proper because this remark motivates [AKS-s, Theorem 5].

(r1) P. 4. *'Next, a standard term of 'geometric realization' is substituted with the term 'body', and then nowhere in the paper this term reappears.'*

We mention both terms: 'The *body* (or geometric realization)...'. This is convenient to readers working in different areas who might be more familiar with one or with the other term. This is unimportant because indeed nowhere in the paper this term (or this notation) reappears, which is explained in the last phrase of the remark:

'We abbreviate $|K|$ to K ; no confusion should arise.'

(s1) P. 4. *'Reference to [Sk16, Theorem 3.3] is inappropriate since this is just the original statement of Özaydin result.'*

This is wrong because the paper [Oz] does not contain the statement [Sk16, Theorem 3.3] (although there is no question of attributing it to Özaydin, which is done in [Sk16]). Reference to

[Sk16] is the more appropriate because this survey provides a simple and accessible to non-specialists proof of the Özaydin result.

(t1) P. 4. *'... but with incorrect details in the sketch of the presented proof [Sk16, §3.2].'*

The referee's statement that there are incorrect details in a proof (in a published paper) is inappropriate because the referee does not indicate which phrase(s) of [Sk16] exactly is (are) incorrect.

(u1) P. 4. *'There is no reason to mention [BG17] at all.'*

We do give the reason in the phrase mentioning [BG17]. The referee does not explain what is wrong with it. The suggestion to remove the mention of [BG17] is inconsistent with the referee being touchy about not citing other papers, and is a minor issue which cannot contribute to the rejection recommendation (we agree to remove the mention of [BG17] if the referee or editors insist).

(v1) P. 4. *'The statement of Theorem 7 quotes three papers without proper credit to precedence.'*

The statement of [AKS-s, Theorem 7] quotes three papers in alphabetical order. The referee does not explain what is wrong with that, and what he/she means by 'precedence', presumably because his/her objection does not withstand the test of being explicitly stated and looked upon.

The following historical details are not used in [AKS-s], and so are not presented there. The paper [Sk17o] (and [Sk17-3, §5])

- contains a Skopenkov-Wagner description of the problems with the proof [MW16];
- attributes the result claimed in [MW16] to Mabillard-Wagner, in order to concentrate on mathematics, not on priority questions.

(w1) P. 5. *'Proof of Lemma 10 is very hard to follow, especially in the final part 'construction of f'_+ '. You are talking about homotopy between a map with degree -1 and degree 1 — more care is needed. Then 'the equivariant Borsuk Homotopy Extension Theorem' is used, with no reference, no assumptions checked for its application. Thus, the proof of Lemma 10 is on the level of a sketch.'*

The second sentence is wrong, we do not talk about homotopy between maps of degree -1 and degree 1 . The referee does not refer to specific sentence where we do (because there are none). (E.g. before the second display formula in p. 4 we write that the map between spheres is defined for $t = 0, 1$, not for $t \in [0, 1]$.)

The third sentence is a valid criticism. However, the referee indicated no significant problem, and this criticism is easy to take into account [AKS-3, pp. 5-6, Proof of the assertion: construction of $f_{1,+}$].

Thus this paragraph of the report cannot contribute to the rejection recommendation.

(x1) P. 5. *'For the references:*

- the reference [BFZ] should be updated, the paper is published,
- the reference [BG17] should not be used,
- the reference [BZ16], the last name of the first author is misspelled, the year of publication and the name of the publisher are missing,
- the reference [Fr15] should be updated, number, year, and pages are missing,
- the reference [Oz], the year is missing,
- for the references [AK19, MW15, MW16, Sk17o] the years of posting should be added as well as the number of pages, even some date can be deduced from the arXiv reference of a choice of quotation label.

This could be fixed during the copy editing process if the paper is accepted. However, we did update the references in [AKS-2].

(y1) P. 5. *'At the end, in the last sentence of the paper: '*' should not be used to classify the papers in the situation when all the relevant surveys and/or books are not quoted.*

Some of the survey papers and the books related to the topic which are not quoted are [Ma03, Zi11, BBZ, BS17, Sh18].'

We do cite the surveys on the topological Tverberg conjecture containing different description of references, including [BZ16] which contains a misleading [Sk16, Remark 5.7] description. We did not cite the surveys [BBZ, BS17, Sh18] because they do not present any new description comparatively to [BZ16]. However, we added citations of the surveys [BBZ, BS17, Sh18] in [AKS-2].

We did not cite (and did not add to [AKS-2] citations of) the book and the survey [Ma03, Zi11] because they do not mention counterexamples to the topological Tverberg conjecture (these references appeared before the counterexamples). So these nicely-written references are too far afield to be mentioned in a research paper on stronger counterexamples.

Remark 6.5. This is a quotation of [Sk16, Remarks 1.9.abc]. We omit the footnotes containing details of justification; the numbers refer to numbered statements of [Sk16].

(a) ‘*The topological Tverberg theorem, whenever available, implies the van Kampen-Flores theorem*’ [Gr10, 2.9.c, p. 445, lines –1 and –2]. This is the Constraint Lemma 1.8. What M. Gromov called ‘theorem’, we call ‘conjecture’. See (c) for the rediscovery of this lemma.

This lemma was proved in [Gr10, 2.9.c, p.446, 2nd paragraph] by a beautiful combinatorial trick reproduced in the current paper.

(b) *A counterexample to the r -fold van Kampen-Flores conjecture for r not a prime power* (Theorem 1.7) was implicitly obtained by I. Mabillard and U. Wagner in [MW14, MW15] by mentioning the Özaydin Theorem 3.3, and proving that the Özaydin Theorem 3.3 implies Theorem 1.7. Indeed,

- the Özaydin Theorem 3.3 states that some equivariant map exists for r not a prime power;
- [MW14, Theorem 3] states that for codimension at least 3 the existence of the above equivariant map implies the existence of an almost r -embedding as in Theorem 1.7.

Failure of the r -fold van Kampen-Flores conjecture for r not a prime power was first explicitly stated by F. Frick [Fr15].

(c) *Since O and $O \Rightarrow \overline{VKF}$ and $TTC \Rightarrow VKF$ imply \overline{TTC} , by (a) and (b) papers [Oz, Gr10, MW14] together give counterexample to the topological Tverberg conjecture* (so that there remained the non-trivial task of writing versions of [Oz, MW14] that could be accepted to peer-review journals). However, neither Mabillard and Wagner nor the topological combinatorics community were aware of (a) before 2016. This is surprising because the next part [Gr10, 2.9.e] of Gromov’s paper was discussed during the problem session at 2012 Oberwolfach Workshop on Triangulations. So this community saw a serious problem with the Mabillard-Wagner approach to a counterexample to the topological Tverberg conjecture: maps from the $(d + 1)(r - 1)$ -simplex to \mathbb{R}^d do not satisfy the codimension ≥ 3 restriction required for [MW14, Theorem 3]; these maps actually have negative codimension. F. Frick [Fr15, proof of Theorem 4] realized that this problem can be overcome by (a). He did this by rediscovering (a), not by finding the reference [Gr10, 2.9.c]. In fact, (a) was implicitly rediscovered earlier by Blagojević-Frick-Ziegler [BFZ14, Lemma 4.1.iii and 4.2].

Example 6.6 (Letters to the Editors of Israel J. Math. on [AKS-s]). (a; 17.03.2022)

Dear Editors,

Hope you are fine and healthy.

Please find in Example 6.4 justification that the paper [AKS-s] was rejected from Israel J. Math. on the basis of an incompetent referee report. In our opinion, this violates important principles of scientific discussions recalled in Remark 4.2.ab. Your reaction to this justification would be important, e.g. you can:

- restore the status of a peer reviewed journal (as suggested in Remark 4.3), or
- publicly explain why the Editors disagree with specific parts of Remarks 4.1, 4.2, 4.3, or
- announce at the web page of the journal ‘*that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation*’ (see Remark 4.1; this can be done implicitly by presenting no public reply to this letter).

Cf. Examples 6.1.ab.

Please find the pdf of this message at https://www.mccme.ru/circles/oim/rese_inte.pdf.

Sincerely Yours, Roman Karasev and Arkadiy Skopenkov.

(b) (A reply of 18.03.2022 to a private letter from M. Temkin of 17.03.2022)

Dear Michael,

Could you provide your public replies to Arkadiy's, and/or to Roman and Arkadiy's messages of March 17, so that your replies can be presented in an update of arXiv:2101.03745? See Remark 4.4.c.

If not then https://www.mccme.ru/circles/oim/rese_inte.pdf (and eventually arXiv:2101.03745) will state (as a part of the public discussion) that no public reply is available. This will be as informative to the math community as a public reply.

Sincerely Yours, Roman Karasev and Arkadiy Skopenkov.

Example 6.7 (the report on handling of [AKS-2] in Proc. of the Amer. Math. Soc.; R. Karasev and A. Skopenkov; 20.03.2022). The paper was rejected on the basis of a referee report X shown to be incompetent below (cf. Example 6.8). The referee's specific critical remarks are either

- attempts to misuse the anonymous peer review system to promote an opinion which failed to be substantiated by an open discussion, see (1) below, or
- wrong or unjustified statements and judgements, see (2)-(5) and (7) below, or
- valid critical remarks that cannot contribute to the rejection recommendation, see (6) below.

The most important critical remarks to the report are (3), (4), and (7) below.

(1) '*... Recently, in 2015, Frick presented counterexamples to the topological Tverberg conjecture for non prime powers...*'. (We do not quote from the report the rest of the introductory paragraph.)

In Example 6.4.c it is explained that *this is misleading*, and that *the paper under review is independent of the discussion of references on the counterexample to the topological Tverberg conjecture*.

(2) '*The paper under review studies further the 'dimension gap' of counterexamples. The main and the only result of the paper is [AKS-2, Theorem 1] which gives improved "dimension gap" of counterexamples to the topological Tverberg conjecture.*'

Here '*the only*' is incorrect (because the paper has [AKS-2, Theorems 4 and 6]) and is irrelevant to the rejection recommendation. Cf. Example 6.4.c.

(3) '*The authors do not properly evaluate and compare their result to the other known related results. The claim in [AKS-2, Remark 2.a] "Theorem 1 is a partial result on [BFZ, Conjecture 5.5] stating that ..." is not correct. Indeed, if [AKS-2, Theorem 1] would address any instance of [BFZ, Conjecture 5.5] the result would definitely deserve publications in a good journal.*'

Even if a paper does not completely solve any case of motivating conjectures or problems, the paper could be an interesting or significant contribution to them. The referee does not justify that the paper does not contain such a contribution. Cf. Example 6.4.b1 as well as the above quotations from report Z and quick opinion Y. (E.g. ' $f(n) \geq n - 1$ ' is a partial result on a conjecture ' $f(n) = n^2$ ', even though $f(n) \geq n - 1$ does not imply that $f(n) = n^2$ for any instance of n .) Thus the claim in [AKS-2, Remark 2.a] is correct, and the referee's conclusion '*do not properly evaluate and compare...*' is incorrect. Still, we do not object to slightly changing the correct phrase of [AKS-2, Remark 2.a] to emphasize that our partial result on [BFZ, Conjecture 5.5] does not prove the conjecture for any instance.

(4) '*The rest of the paper gives the proof of [AKS-2, Theorem 1] is a way which is very hard to follow and even harder to verify.*'

This conclusion is not justified by references to the paper and suggestions what could be done in a more clear way, except for a single minor issue (see below our replies to the following phrases). So the report does not show that the paper is hard to read because of poor exposition, not just because of complexity of the matter. Cf. the above quotations from report Z. We wrote this paper in a clear way according to the high standards of §2, including sending it to colleagues and working on their critical remarks before journal (and even arXiv) submission.

(5) *‘The main technical ingredient of the proof of [AKS-2, Theorem 1] is [AKS-2, Lemma 10]. It constructs degree zero equivariant self-maps of odd dimensional spheres. The proof of [AKS-2, Lemma 10] is not well presented. It is given in a paragraph on [AKS-2, page 4], but relies on a sequence of claims which are presented on the remaining pages of the paper.’*

The last sentence describes what we believe a *well-structured proof* is.

(6) *‘A problematic issue occurs is the use of the so called “equivariant Borsuk Homotopy Extension Theorem”. The authors give no reference to such a theorem but instead give a comment in [AKS-2, footnote 3] by stating the needed result and claiming “The proof is analogous to the non-equivariant version [FF89, §5.5].” The statement given in [AKS-2, footnote 3] is obviously not true.’*

We are grateful to the referee for finding a flaw in our argument. This flaw is easy to fix, see the next version [AKS-3]. Of course, a referee need not think how to fix a flaw he/she found. However, a competent refereeing includes revision not rejection recommendation in the case of a flaw in the proof not shown to be serious by the referee.

(7) *‘In summary, the paper considers an interesting and relevant problem in Topological Combinatorics. It presents a partial results on an improved “dimension gap” of counterexamples to the topological Tverberg conjecture, but fails to give a comprehensive and complete proof of the result. For these reasons I suggest to the editor of the PAMS to reject the submitted version of the paper “Stronger counterexamples to the topological Tverberg conjecture”. The only suggestion I can make to the authors is to rewrite the paper completely with much more care for the reader, and, more important, for the arguments in the proofs.’*

Here the judgement *‘fails to give a comprehensive and complete proof of the result’* and the suggestion *‘to rewrite the paper completely’* are unjustified and unsuitable, as justified by (2)-(6) above.

Example 6.8 (letters to the Editors I. Novik, D. Futer, J. Wang, A. Folsom of Proc. of the Amer. Math. Soc. on [AKS-2]). (a; 20.03.2022) Dear Isabella,

Hope you are fine and healthy.

Please find in Example 6.7 our response to report X on our paper [AKS-2] recently rejected from Proceedings of the AMS. *There we justify that the report is incompetent, and violates important principles of scientific discussion recalled in Remark 4.2.ab.*

Let us quote the ‘Conclusion and recommendation’ part of report Z: *‘I enjoyed reading this paper. It addresses important questions in a currently very active area of topological and geometric combinatorics. The results are non-trivial and interesting and the technique relatively new. I gladly recommend this paper for publication in the Proceedings of the American Mathematical Society.’*

Let us quote the last concluding paragraph of quick opinion Y: *‘I recommend proper refereeing. Given that there is a wealth of literature for degrees of equivariant maps, a more careful verification is necessary to determine whether the main ingredient constitutes a new result. If this is the case, then this manuscript is of significant interest.’*

Hence retraction of the rejection decision, and reconsideration of the paper, in our opinion, would allow to keep high reputation of Proceedings of the AMS. This can be done along the lines of Remark 4.3.b. *Could you please let us know if you plan to do that?*

Reviews of handling A. Skopenkov's papers are published in §5, §6. E.g. for handling the previous version of the same paper see Examples 6.1.a and 6.4.

Best Regards, Roman Karasev and Arkadiy Skopenkov.

(b; 26.03.2022) Dear Editors,

We write this letter in great respect to the American Mathematical Society. This respect involves our belief that no AMS journal is an instrument of redistribution of jobs and grants in a way obstructing the progress of science, so that mistakes in handling submitted papers are corrected, see Remark 4.3.

Our letter of 20.03.2022 contains (in (b)) justification that the paper [AKS-2] was rejected from Proc. of the Amer. Math. Soc. on the basis of an incompetent referee report. We thank Isabella for her reply. Isabella's reply does not question that justification. In our opinion, rejection of a paper on the basis of an incompetent report violates important principles of scientific discussions recalled in Remark 4.2.ab. More importantly, leaving such a flaw uncorrected would show that a journal is not a peer review journal. Isabella's reply does not demonstrate understanding of this problem, and does not suggest or accept any way of resolving the problem. (We did not object to rejection based on one *negative* report out of three, we objected to rejection based on an *incompetent* report.) This calls for a transparent public discussion as described in Remark 4.4.c. So your *public* reply to our letters would allow to keep high reputation of Proc. of the Amer. Math. Soc. In order to avoid confusion, could you read Remark 4.4.c before replying.

You can, in particular:

- carry public discussion of our justification (b) that report X is incompetent, as suggested in Remark 4.3 (and thus confirm the status of a peer reviewed journal), or
- publicly explain why the Editors disagree with specific parts of Remarks 4.1, 4.2, 4.3, or
- announce at the web page of the journal *'that the journal is not a peer reviewed journal, and so should not be used as a reliable reference and/or for jobs or grants evaluation'* (see Remark 4.1; this can be done implicitly by presenting no public reply to this letter).

Cf. Examples 5.2, 5.4, 5.5, 5.6, and 6.1.

Sincerely Yours, Roman Karasev and Arkadiy Skopenkov.

Example 6.9 (the report on handling of [KS21e] in Fundamenta Mathematicae). (a) The paper [KS21e] (shortened by deleting sections 2.4.and 2.5) was submitted in April 2022. It was rejected in December 2023 because of

(1) a logical fallacy in the Editors' letter: from the presence of earlier published results (or combinations of those) in the introduction, the letter concludes (ignoring the new results) that 'we are close to ... the main results having already been obtained in other publications'.

(2) a wrong statement in the Editors' letter: 'some of the results of the removed Appendix of [KS21e] still play a role in the proofs of the submitted paper'.

See details in my letter to the Editors presented in (b,c,d,e). No other criticism of the paper was presented by the Editors. No referee reports were presented by the Editors.

(b; 27.12.2023) Dear Henryk,

Thank you for your letters. Could you send me the referee report(s) on which the Editorial decision is based? If yes, could you allow me to publish them analogously to

<http://www.map.mpim-bonn.mpg.de/Template:Karasev%26Skopenkov2020> ?

Hopefully the referee reports would

(1) explicitly name NEW results of the paper (the result stated in the abstract and restated as Corollary 1.3.3a, Corollary 1.2.1b, Theorem 1.2.3, Corollaries 1.3.2ab, Corollary 1.3.5, and the case $k=2$ of 1.2.1a, 1.3.3b, 1.2.2, 1.3.4, 1.3.7c);

(2) explicitly state that the results named in (1) do not meet the very high standards of Fundamenta Mathematicae;

(3) explicitly state where results of the removed Appendix of [KS21e] are used in the proofs of the submitted paper.

This done, I will actually like the reports and the editorial decision made upon such clever reports.

The short version of the reports presented in your letter

* states ‘the Authors themselves say rightly that several of their main results are a combination of those which appeared already in [PT19], [Me06], [FKT], or [AMS+]’;

* ignores the novel part, and

* concludes that ‘we are close to’ ‘the main results having already been obtained in other publications’.

I’m afraid this is a logical fallacy. Presence of earlier published results (or combinations of those) in the introduction does not mean that the paper does not have new results. (E.g. the fact that Theorem 1.3.1 is attributed to [vK32, Sh57, Wu58, Me06, FKT, AMS+] cannot contribute to the lack of novelty in other results.)

The paper does not say that several of its main results are a combination of those which appeared earlier. Results which are combinations of those which appeared earlier are presented as earlier known results, not as our results: we give clear references to earlier publications. (See e.g. reference to Remark 1.3.7 in p. 1, and in Comments on the proof after Theorem 1.3.1: ‘Part (a) is essentially classical ... Part (b) is proved in ... Part (c) is proved in ...’.)

The short version of the reports presented in your letter does not indicate any specific place where results of the removed Appendix of [KS21e] are used in the proofs of the submitted paper. In my opinion, there are no such places; the appendix is only mentioned for introductory purposes.

Best Regards, Arkadiy.

PS If the reports and editorial decision are based on a confusion, then we are ready to revise the paper emphasizing the distinction between old and new results. In some cases this would make the paper less readable, so we need a referee’s opinion on whether he / she wants us to repeat several times that for $k > 2$ a result is known, and for $k = 2$ (2-complexes in 4-manifolds) a result is new (instead of having all this in compact form in Remark 1.3.7.a).

(c; 15.01.2024) Dear Henryk,

Hope you are fine and healthy.

Sending a report takes less than a minute. So one has to conclude that there are no reports on the paper which are up to the high standards of Fundamenta Mathematicae. (Could you send me the reports without delay if the conclusion is wrong?) So one has to conclude that the status of the paper changed back to ‘under review’ (instead of ‘rejected’). Could you please confirm that (or send me the reports without delay if the conclusion is wrong) ?

Best regards, Arkadiy.

(d; 12.02.2024) Dear Editors, Dear Henryk,

Hope you are fine and healthy.

I am sorry I did not receive any reply to the Dec 27, 2023 letter and the Jan 15, 2024 reminder. Unfortunately, this leaves for me only to add to §6 of <https://arxiv.org/pdf/2101.03745.pdf> the following information: [presented in (a); only the sentence involving reference to (b,c,d,e) is slightly updated].

However, I value collaboration with Polish colleagues very much. So I decided to patiently send this reminder, and to wait another week waiting for the proper refereeing process. That is, for either sending me the referee reports, or confirming that the paper (submitted in April 2022) is back at the ‘under review’ status, and setting a timeframe for presenting the (new) reports together with Editorial decision based on these reports.

Best Regards, Arkadiy.

(e; 27.02.2024) Dear Editors, Dear Henryk,

However reluctantly, I updated https://old.mccme.ru//circles//oim/rese_inte.pdf adding Example 6.9. It justifies that the paper arXiv:2112.06636 (shortened by deleting sections 2.4 and 2.5) was rejected from Fundamenta Mathematicae because of a logical fallacy in the Editors’ letter, and a wrong statement in the Editors’ letter. (I am sorry I did not receive any reply to the Dec

27, 2023 letter and the Jan 15, 2024 reminder. I am sorry I did not receive the information I asked for in my Feb 12, 2024 letter.)

I value collaboration with Polish colleagues very much. So I postponed updating Example 6.1 of https://old.mccme.ru/circles/oim/rese_inte.pdf, and updating <https://arxiv.org/abs/2101.03745>. I am patiently sending this reminder, and waiting another week for confirmation of the proper refereeing process. That is, for either sending me the referee reports, or confirming that the paper (submitted in April 2022) is back at the ‘under review’ status, and setting a timeframe for presenting the (new) reports together with Editorial decision based on these reports. I would be glad to either add positive updated information to Example 6.9, or to leave (upon your wish) only positive updated information in Example 6.9 (in both cases moving Example 6.9 to a different section).

Please observe that I am asking for the above confirmation of the proper refereeing process, which could be done immediately. I am not asking for immediate sending me updated referee reports, which perhaps requires more than 2 months passed since my Dec 27, 2023 letter (although the paper is submitted in April, 2022).

Sincerely Yours, Arkadiy Skopenkov.

PS Mathematicians (both Polish and foreign ones) would be grateful if you allow me to publish the Editors’ Dec 8, 2023 letter giving grounds for rejection (perhaps except the first and the last paragraphs not related to such grounds). This will help them to make their own judgement whether my criticism of that letter is just. Lack of your agreement to publish that letter would be a serious confirmation that my criticism is just.

Example 6.10. (a) The Moebius contest <http://www.moebiuscontest.ru> is a prize for scientific papers. Although this is not a peer review journal, this award plays an important role in setting reliability standards for young researches. So Remark 4.1 is applicable. Papers that are not reliable references do receive this prize, see (b) and [Skm]. So it is important either to make disclaimer to this effect, or to remove the word ‘scientific’ from the official information on the contest (<http://www.moebiuscontest.ru>).

(b) In [Skm, §7] I present the referee reports to [Ak07] and to the earlier versions of [Av14, Ru14, Go16] cited in [Skm] (two of the four reports are in English). It was suggested to award these versions, disclaiming that they are not reliable references. However, the jury awarded them without such disclaimers. For the version of [Go16] the jury even publicly stated that it found the proof in that version complete (as a jury member I voted against this statement). A. Akopian was very decent to inform the jury several months after the award that he found a mistake in [Ak07]; no update of [Ak07] is publicly available. The referees’ remarks were later taken into account in [Av14, Ru14] but not in [Go16].

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In this list by stars I marked books, surveys, expository papers, as well as internet materials which I consider accessible to non-specialists.