Guidelines for SoCG Reviewing

The CG Steering Committee
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Abstract

This document describes guidelines for reviewing submissions to the International Symposium on Computational Geometry (SoCG). They apply to everyone who reviews submissions for the SoCG, may it be as a member of the program committee (PC) or as an external subreviewer. Collectively, this target audience is referred to as *reviewers* here. Reviews serve two main purposes: (1) they provide information to help the PC decide which submissions are accepted for presentation at the conference and (2) they provide useful feedback to the authors about the quality of their submission—independently of whether or not it is accepted. To serve both of these purposes, high-quality, detailed reviews are required. So, what does a high-quality, detailed review entail? The goal of this document is to present our view on this matter, along with a number of issues related to the review process overall. The guidelines are structured as follows:

1. Ethical issues and conflicts of interest
2. Typical structure of a review
3. Scope
4. General criteria
5. Criteria specific to each paper type
6. Score and confidence ratings
7. Main body and appendix of submissions

1 Ethical issues and conflicts of interest

Submitted papers are *strictly confidential*. Reviewers must not distribute them, nor use them for their research. Similarly, the reviews, grades, and confidence scores must be kept confidential.

Obviously, a paper should not be assigned to a reviewer who has a conflict of interest (CoI) with the paper. In general, a reviewer should declare a CoI when they may be biased towards (one of) the authors, or when there is some other reason that they cannot review a paper objectively. In particular, a reviewer should declare a CoI when they have one of the following relations to an author:

- Family member, (ex-)significant other, or close friend.
- PhD advisor or advisee (no time limit), or postdoctoral or undergraduate mentor or mentee within the past five years.
- Same institutional affiliation.
- Involved in an alleged harassment incident. (It is not required that the incident be reported.)
- Reviewer owes author a favor (e.g., recently requested a reference letter).

Note that even though a reviewer may believe they can write an objective review in some of the above cases, they should still declare a CoI to avoid the appearance of bias.

Another reason for a CoI is the following:

- Frequent or recent collaborator whom you believe you cannot review objectively.

If a reviewer feels that they can write an unbiased review about a paper co-authored by a recent collaborator, they can still review the paper. In such cases they should mention a "soft CoI" in the "Comments for PC" in Easychair. This should also be done if the reviewer has obtained competing results, or in other cases that can be perceived as a CoI by others. Of course if a reviewer feels uncomfortable to review a paper for such reasons, they can simply declare a (hard) CoI and decide not to review the paper.

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2 The review

Typically, a review contains the following:
- A concise summary of the paper, the problems considered and the main contributions. If a paper is written well, then it is easy to identify the main contributions—the abstract describes them. But some authors might overstate their results or might not realize the significance of certain aspects of their work, and so a reviewer may give a different summary of the main contributions than the authors.
- A list of the strengths and weaknesses, supported by evidence (see criteria below).
- An overall evaluation based on the strengths and weaknesses.
- Some constructive and professional feedback to the authors for improving the paper.
- Optionally, in a separate section, some confidential feedback to the program committee. Please only put information there that should not be disclosed to the authors (typically, information that would break anonymity of the reviewer).

Please note that short reviews are not very useful. Precise and supported arguments are crucial to help the PC to arrive at a well-reasoned decision. Specifically, if you believe that there is an error in a statement or a gap in a proof, provide a clear description of the problem so that other reviewers can check and verify. In a similar vein, a review should not simply state "the solution is trivial" without explicitly giving an argument (should be easy if it is trivial, indeed), or pointing to previous work or textbook material that essentially implies the result. Analogously, this applies to positive comments: Simply stating "great result" or "elegant proof" without any further explanation is not very helpful and insightful; so such a statement should always be accompanied with a few more words that detail what exactly is great or elegant there. At the end of the day an evaluation is always subjective, but it should be well-founded and well communicated.

Always be polite and constructive. Reviewers should assume that the authors have put their best efforts into their submissions. Hence any comments that could be interpreted as hurtful or condescending should be avoided. Be mindful of cultural differences and try to abstain from the use of negative adjectives and sarcasm. As a reviewer you should feel comfortable to sign your review with your name (even if there are good reasons not to do so).

Remember that your assessment is only an expression of your personal opinion (in particular when it comes to whether the problems or results are interesting). Try to phrase your assessment as much as possible as an expression of this opinion and not as facts. In short, write reviews with the care and professionalism that you would want to be applied to reviews of your own papers.

3 Scope

From the call for papers: We invite submissions of high quality that describe original research on computational problems in a geometric setting. Topics of interest include, but are not limited to:
- Design, analysis, and implementation of geometric algorithms and data structures;
- Computational complexity of geometric problems;
- Implementation and experimental evaluation of geometric algorithms and heuristics, including mathematical, numerical, and algebraic aspects;
- Discrete and combinatorial geometry;
- Computational topology, topological data analysis, and topological combinatorics;
- Applications of computational geometry in any field.

4 General criteria

When evaluating a paper, one should address the following (interrelated) issues. A paper with a high score should in general score high on several of them. These criteria are general and apply to all types of papers: theoretical, applied, or experimental. Additionally one should address issues specific to the paper type, as discussed in Section 5.

Relevance. In what respect is the paper relevant to computational geometry? Is it directly relevant for the design, use, analysis, or implementation of geometric algorithms? Does it have indirect implications for the development or the theory of geometric algorithms? Does it contribute to the mathematical foundations of discrete or combinatorial geometry and/or algebraic geometry and/or computational topology? (See "SoCG scope" above.)

Foundational/conceptual contribution. Does the paper introduce a new model, new notion, new definition, new approach, novel implementation, novel application? Note the significance and reasons for this novelty (and note the absence of such a novelty, if applicable).

Technical development. Does the paper involve

- an introduction of a new technique?
- a novel use of known technique?
- a talented use of known technique?
- a traditional use of known technique?
- a trivial use of previous technical knowledge?

Relation to open problems. Does the paper solve completely/partially an open question? How important is this question (central/important/interesting/legitimate/unimportant)? How much effort has been invested previously in solving it and by whom?

Social interest in paper. Is it potentially interesting to the whole community of computational geometry, to a major field, to everyone in a restricted area, or interesting only to the authors?

How will it contribute? Does it have the potential to influence or affect future work? Does it have the potential to have an impact on application domains? Is it/can it be important in other fields/subjects or have a wider influence?

Type of contribution. Is it a

- first step (opens a new area)?
- last step (closes an important area)?
- giant step (makes essential progress)?
- none of the above?

Clarity of presentation. Is the paper well written, or in a too preliminary form? Have the authors made an effort to make it accessible, or does the way the paper is written make the paper unnecessarily hard to read and/or narrow down its interest to specialists?

5 Criteria specific to each paper type

When evaluating a paper, it is important to keep in mind that there are different types of papers and that the criteria by which papers of one type are evaluated may be different from those used for papers of another type. Moreover, the importance of a common criterion may vary from type to type. The strengths and weaknesses of a particular paper may be diverse in nature. Among the paper types that we expect to encounter are papers focusing on:

- mathematical foundations,
- algorithmic design and complexity and/or lower bounds,
- experimental & implementation issues, and
- applications.

Hybrid papers, which consist, for example, of both an algorithmic design and analysis part and an experimental part are of course also common.

Below is an attempt to characterize these paper types and to specify the main criteria by which they should be evaluated.

Mathematical Foundations. A typical paper will contain theorems and proofs describing new results in discrete or combinatorial geometry, and/or in algebraic geometry, and/or computational topology. The paper will primarily be evaluated on the importance of its results, its technical depth, the elegance of the solution, the connection of the problem studied to computational geometry and topology, and the potential future impact on algorithm development.

Algorithmic Complexity. A typical paper will contain algorithms, theorems, proofs and/or lower bounds describing new results on computational geometry problems. The paper will primarily be evaluated on the (mathematical or computational) relevance and importance of the problem studied, its technical depth, the elegance of the solution, and the potential future impact of the results and/or the proposed new methods and techniques.

Experimental & Implementation. A typical paper will make a clear contribution to the implementation and evaluation of geometric algorithms, such as exact, approximate, and/or algebraic computation, algorithms engineering, and/or the experimental evaluation of competing algorithmic approaches. The paper will primarily be evaluated on the completeness and the expected impact of the proposed implementation, the soundness of the experiments, the quality and quantity of testing, and on the general amount of knowledge gained.

Application. A typical paper will describe the modeling and algorithmic choices made when developing or adapting computational geometry techniques for an application area. The paper will be primarily evaluated on the soundness of the modeling decisions, the ingenuity of the solution, the effectiveness of the proposed method, and the expected impact in the application area. One might also consider the lesson learned regarding the applicability or suitability of computational geometry tools to the specific area.

6 Score and confidence ratings

Each review comes with a score rating and a confidence rating, which are **not** sent to the authors. As for the score rating, please keep in mind that SoCG is a highly selective conference. Typically about 30% of the papers submitted will be accepted; the ratings given by each PC member should take this constraint into account. These ratings are used as a reference or guide, but the contents of the reviews are more important and meaningful than the numerical scores.

Score	Interpretation
Strong accept (+3)	An enthusiastic accept. An excellent paper, advancing the field in an important way, and well written. People should definitely attend the talk. I think this paper will be in the top third of the accepted papers (so roughly in the top 10% of the submissions) and is a possible candidate for the best paper award. I would fight strongly for this paper.
Accept (+2)	A strong contribution. I would definitely like to see this paper at the conference. I feel I learned something worthwhile from this paper, and believe that people will be interested in the talk. I think it will be in the middle third of the accepted papers (so roughly in the top 20% of the submissions, though perhaps not top 10%).
Weak accept (+1)	A good contribution, probably in the bottom third of the accepted papers. Not a stellar result, but I think it should be accepted, and hope there is enough space for it.
Borderline (0)	If you really cannot make up your mind between +1 and -1, then you can give this score, but try to avoid it.
Weak reject (-1)	A contribution with some merits, but probably below the cutoff (that is, not within the top one-third of the submissions): I think the problem is not very interesting, the solution is not very innovative, and/or the writing is not so stellar. I am willing to be convinced otherwise, but there are probably enough better papers.
Reject (-2)	This paper is clearly below the cut in my opinion: the results are not strong enough, the writing is quite poor, and/or there are serious concerns about correctness or novelty. I would actively argue against accepting the paper.
Strong reject (-3)	An obvious reject.

The confidence ratings are described below.

Grade	Interpretation
Expert (5)	Consider me an “expert” on this paper. I understand it in detail. I know the field, and I am perfectly sure about my judgment; I have checked and understood all proofs.
High (4)	I am fairly familiar with the area of this paper and have read the paper closely enough to be confident of my judgment.
Medium (3)	I have read the paper carefully and understood the main ideas. I am moderately confident of my judgment.
Low (2)	I am not an expert. My evaluation is that of an informed outsider. I have some idea of what this paper is about, but I’m not all that confident of my judgment.
None (1)	(to be avoided...!) Please do not use this except in extreme circumstances.

7 Main body and appendix of submissions

From the call for papers: **All details needed to verify the results must be provided.** Supporting materials, including proofs of theoretical claims and experimental details, that do not fit in the 500-line limit should be given in an appendix. If more appropriate, the full version may be given as the appendix. In both cases, however, the authors should include in the main part specific pointers to the relevant locations in the appendix. The appendix will be read by the program committee members and subreviewers at their discretion and will not be published as part of the proceedings. Thus, the paper without the appendix must be able to stand on its own. Experimental and implementation results (independent of paper type) must be reproducible and verifiable. Any aspect that negatively affects reproducibility, such as lack of availability of data or code etc., for whatever reasons, should also negatively affect the evaluation of the submission.

History of this document. The first public version of these guidelines was written by the 2020–2022 CG Steering Committee, consisting of Mark de Berg, Sándor Fekete, Michael Hoffmann, Matya Katz, Bettina Speckmann, and Yusu Wang, and published at cg.org on November 26, 2021. The document is based on the SoCG'21 reviewing guidelines by Kevin Buchin and Éric Colin de Verdière, which in turn drew inspiration from unpublished versions of similar guidelines that were circulated among SoCG PCs in earlier years.