SoCG'21 – Guidelines for reviewing

Thank you for accepting to review for SoCG 2021! Your service is deeply appreciated. Please keep in mind that SoCG is a highly selective conference.

Reviews provide information to help the program committee (PC) decide which submissions are accepted for presentation at the conference. A reviewer usually has more time to look into a paper in depth than a PC member, since each PC member gets about 24 papers to process. So your help in carefully checking and evaluating the paper is very useful.

We need that you honor the deadline to provide the reviews. Actually, early reviews are very welcome: they can help identify possible problematic papers.

This document covers the following aspects:

1. Ethical issues and conflicts of interest
2. Typical structure of a review
3. SoCG scope
4. General criteria
5. Criteria specific to each paper type **(important; quite specific to SoCG)**
6. Score and confidence ratings
7. Main body and appendix of submissions

See also the call for papers (https://cse.buffalo.edu/socg21/socg.html).

1. Ethical issues and conflicts of interest

Submitted papers are **strictly confidential**. We must not distribute them, or use them for our research. Similarly, your reviews, grades, and confidence scores must be kept confidential.

Submissions should be judged solely on the basis of the submitted extended abstract. You may have a personal bias on some papers. Only you can judge such a bias, and decide if you don't feel comfortable reviewing the paper. These conflicts of interest (CoI) can be tricky – they come in various shapes and sizes. We distinguish between strong and weak CoIs. A strong CoI prevents you from reviewing the submission and from handling it as a PC member. A weak CoI does not formally prevent you from reviewing the submission and from handling it as a PC member, but you should indicate the nature of the weak CoI in the confidential comments for the PC, for transparency.

Here is a list of instances of relationships that can cause strong or weak conflicts of interest. These should be viewed as guidelines: Not all cases can be covered, every situation is unique, and there is no obvious scale for measuring ties between two persons.

- current or past PhD supervisor or PhD student (strong: past 10 years; weak: anytime);
- employment at the same institution or company, within the last 2 years (strong) or 5 years (weak);

- close collaboration on some recent projects or papers, within the past 2 years (strong) or 4 years (weak) – this refers to collaboration on joint research, not publication (journal papers often appear some time after collaboration);
- current work in the exact same area, so that accepting the submission would significantly impact a current project of the potential reviewer or handling PC member (typically, working towards obtaining the same result);
- financial, family or close personal relationship;
- in general, any doubt that the potential reviewer or handling PC member can judge the author's work fairly.

Do not declare a strong conflict of interest too quickly, just because you wrote papers with someone recently. You might just be one of the few who can judge the paper well. For unclear cases, the following informal criterion might be helpful: Judging a paper that you would support (or stand up against) for reasons related to **you** and not the content of the paper itself should be avoided.

2. Typical structure of a review

Typically, a review contains the following:

- A concise summary of the paper for the program committee members (which may not have read the paper): In the reviewer's own words, what are the main problems considered? Why are they relevant or irrelevant? What are the main results?
- An evaluation supported by evidence (see criteria below).
- 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, even if they are spot on, are generally unconvincing and thus not very useful. The PC is more likely to adopt your viewpoint if you provide precise and supported arguments.

As we have been authors, we know that high-quality reviews are appreciated by authors. Please, try to provide high-quality reviews and comments. The reviews should be polite to each paper (and of course the authors). Please refrain from using impolite or offensive language or statements. For example, saying that something is garbage is rude and unacceptable.

3. SoCG 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;
- Lower bounds on the computational complexity of geometric problems;
- Mathematical, numerical, and algebraic issues arising in the formulation, analysis, implementation, and experimental evaluation of geometric algorithms and heuristics;

- 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 listed below.

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 its technical depth, the importance of the results, 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 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)	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 by 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 reasonable contribution for SoCG, 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 decent research result, but I think it will not make the cut at SoCG, for example because the problem is not very interesting or the solution is not very innovative. I am willing to be convinced otherwise, but as it stands I think we have enough better papers.
Reject (-2)	This paper is clearly below SoCG level in my opinion: the results are weak and/or the writing is quite poor. I would actively argue against accepting the paper.

Strong reject (-3)	An obvious reject.

The confidence ratings are described below.

Grade	Interpretation
Expert (5)	Expert. Consider me an "expert" on this paper. I understand it in detail. I know the field, and I am perfectly sure about my judgement; I have checked and understood all proofs.
High (4)	High. I am fairly familiar with the area of this paper, and have read the paper closely enough to be reasonably confident of my judgment.
Medium (3)	Medium. I have read the paper carefully and understood the main ideas, but I'm not very confident of my judgment on it.
Low (2)	Low. 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 on it.
None (1)	Null. (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 should be able to stand on its own.** Experimental and implementation results (independent of paper type) must be reproducible and verifiable.

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