Workgroup on Computation Geometry Position Statement
(draft)

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Our workgroup is in the fortunate position of having two recent task force reports with which to work: Theory of Computing: Goals and Directions, from a group chaired by Richard Karp, and Application Challenges to Computational Geometry from a group chaired by Bernard Chazelle. The latter report lists, without attempting to be comprehensive, many resources for computational geometry and many applied problems on which computational geometers, could, should or have worked successfully. In view of these extensive lists, our workshop in Cambridge presents an ideal opportunity to digest these documents and respond to their conclusions and recommendations. We should use these to push the discussion farther, rather than to add problems or resources omitted from the list in the Chazelle report.

Let's start with the assumption that a strong case for the merit[1] has already been made for theoretical computer science and, in particular, computational geometry. Let us use our time to consider in detail the issues already raised, such us: strengthening the communication between computationsl geometry and other fields, between computational geometry and various application areas; ways to arrive at sensible metrics for measuring the quality of implementations (see the algorithm engineering section of the Karp report); communication with the broader community; education of students; ways to make the "pipeline" between theory and application work better (or at all) in both directions of flow.

It seems unwise to continue to consider only the situation of theoretical computer science and computational geometry in the USA alone. Of course education systems and funding agencies differ from one country to another. This means, by considering our community as a global community, that we have the chance to share experiences: what has worked well for education of students in one country might very well be a good idea to consider in another. We see our collegues from far-away places at international conferences, of course. What mechanisms can we develop to share our experiences and ideas (as well as our software) for our mutual benefit? We need global planning. What should be the role of the ACM in such planning for computational geometry? Here are a couple of issues, which are not addressed in depth in the two existing reports, that I believe we should discuss.

0) In what ways can we make the "pipeline" of the Chazelle report flow in the uphill direction, i.e., from practice to theory? Surely it's not enough to tell

[1] meaning fundabiltiy by granting agencies such as the NSF and also, merit in the sense of intellectual accomplishment and depth

acadamic computational geometers that manufacturing problems deserve their attention.
1) What issues and possibilities are we overlooking by having workshops consisting almost entirely of researchers working in North American universities or research labs – few if any participants who are from small start-up companies, who are students, who work in profession societies, who work for funding agencies, who work as scientific journalists, who work as industrial liason officers, etc. We risk extreme myopia without broader participation in our discussions. At some point in the process of improving the strength (in a variety of directions, of which fundability is only one) of computational geometry we should aim for broader participation in the discussion.

2) There's been a lot of discussion on how to encourage computational geometry in more applied directions. What about the other direction? What about our relationships with mathematics, including, for example, topology and combinatorics? Should we consider our subject as a conduit for flow in the pipeline from applications to people who are more theoretically oriented than ourselves, stimulating them to work on problems of interest to us? How might this be done? Let's not forget mathematics in the rush to be more applied.

3) What infrastructure can we propose to universities, professional societies, governments that would make it easier for academics to work on applied problems? Is it just a question of changing the "reward structure" and encouraging conference selection committees to change their tastes? Or can we make some specific suggestions about how to make the process of doing experimental work easier, and easier to evaluate? For just one example, readily accessible data bases of input data on which to try our algorithms and compare their performance with those of people working in other areas, who have solved the problems with own, not necessarily computational geometric methods? Such data bases and the problems they concern would have to be known to computational geometers. How can knowledge of problems from other areas and such databases be made? Certainly the project of David Eppstein is a valuable initiative. Is this a project that should be copied by others, maintained by a professional society, or by people working in research labs who are employed for the purpose of maintaining such information?

4) What are the world-wide demographics of our field? Any chance of a Ph.D. student in computational geometry landing a job in academia in the next 30-40 years, say? What will this do to our field, to have no new input to the academic system in countries where the academic slots in c.g. were filled quickly, during a small span of years? How can we plan for and respond to our demographic situation?

In summary, I propose that we consider what issues may have been overlooked in the existing reports, and that we explore concrete, detailed ways, comming from our global community, to move the recommendations forward.