# Viewpoint Column

by


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# Discussion Panel on the Future: Highlights 2025

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For several years, I have been thinking about the idea of a general discussion about the future of our area. An ideal place for such an event would be an annual meeting embracing several specialised conferences, like Highlights of Logic, Games and Automata, shortly Highlights. This event attracts computer scientists in the so-called Track B of theoretical computer science: people publishing at conferences like LICS, ICALP B, CONCUR, PODS, CAV, POPL, etc.

In 2025 we had the opportunity to organise a discussion panel at Highlights about the future research in the automata, games and logic community and this text is a report from that event. I'll write this report in a very personal style and share with you my perspective on the organisation and the panel itself, as such a personal perspective may be more interesting to read than an attempt to show things objectively.

First a few words about the Highlights conference, as maybe not everybody is familiar with this concept. It arose from the idea to have an annual meeting which brings together most of the community. In short it allows people to come to this single event, meet everyone and hear about every important result which was shown that year. The Highlights conference started in 2013 in Paris and is a very successful event ever since.

The format of the debate was rather simple: first each speaker gives a 5-10 minutes statement about her or his opinion on the future of our area. A few weeks before the panel I sent the panelists a few guiding questions: about the direction we should go as a community, criteria for distinguishing a right research question, or the relation between our discipline and the AI revolution. In the plan, after the statements there was a possible short debate between the panelists, some panel member may want to refer to what the other one said. Finally, a time for a longer discussion with the audience was planned, when everybody could express her or his statement, add a remark or ask a question. This form (statements + short discussion + remarks from the audience) is simple and does not contain a moderated debate between the panelists, which one could expect. I made such a decision based on the observation that it is often extremely challenging to moderate a debate between just a few panelists, make it interesting and not chaotic. Instead, eliminating moderated discussion and just letting people express their thoughts in an unmoderated framework seems better in my opinion. Anyway it is usually the most common form of debate in the real world. For all of that we had 90 minutes.

When choosing the panelists I took care about certain diversity among them in a few dimensions (like stage of career or subfields of our area), but my main purpose was to find people who enjoy long discussions for their own sake, sharing reflections and philosophy. I'm very grateful to Joël Ouaknine, Sophie Pinchinat, Szymon Toruńczyk and Georg Zetzsche, who kindly accepted my invitation. I'd like to particularly thank Szymon with whom I've discussed already in advance, in Warsaw many possible aspects of this debate and with whom I casually like to discuss topics like foundations of mathematics. I'm also very grateful to Christel Baier, who was PC Chair of Highlights this year and who constantly helped me on different stages of the preparation of this event.

During the debate a lot of various topics were touched upon and it would take very long time to describe all the thoughts brought by the panelists and by the audience. Therefore, I'll mainly focus on the ideas, which particularly resonated with me personally. The first statement was given by Szymon Toruńczyk. I particularly liked his opinion on what should guide us to find valuable research topics. Szymon emphasised that this should be mathematical beauty and depth and also interactions with other fields of mathematics. Even though it might seem quite a strong statement, I particularly agree with his opinion about the future with respect to the interaction with artificial intelligence: "There will be an increasing number of papers which claim relevance for AI, but I think in our community, we should always evaluate the mathematical depth and beauty, and not the claim of practicality or impact on other areas. I think this should be judged by experts on the other areas.". Szymon also expressed a few thoughts about interaction of verification and AI. I found interesting his prediction that the area of verification may get very useful to verify what AI will produce, i.e. to get sure that the proofs, code, or other texts produced by AI are not hallucination or some erroneous or mistaken statements.

The next speaker was Joël Ouaknine and he has formulated a very interesting thought about open problems in our area. He emphasised that having flagship open problems helps very much a community. Researchers working in algorithmics or complexity theory have their famous problems like for example P vs NP or improving the exponent of matrix multiplication. Such problems guide the research and increase visibility outside of the small area. Joël postulated that it would be very valuable to establish some list of important open problems from our community. This thread was later continued during the discussion. I liked the idea very much and have asked how can we create such a list, which is proposed by Joël. Should we have a new Hilbert, who proposes a set of fundamental problems, or maybe should we establish some committee, which will choose the problems? In response to that Georg Zetzsche answered that maybe we should create on our own homepages lists of favourite open problems. I like this idea, I have such a list on my homepage, but I personally think this way of promoting open problems is too weak. Joël responded by a reference to the list of Millennium Prize Problems. He said that similarly as it was done with Millenium Prize Problems it would be good to create a committee consisting of researchers representing various subfields in order to propose a list of open problems. Moreover, it would be encouraging to fund a prize, which would be given for solving any problem from the list. To me personally this idea is the most interesting result of the debate and I think it would be absolutely fantastic if such a list could appear in next few years.

The third speaker was Georg Zetzsche. Among other ideas he noted that in our community there is an immense emphasis on algorithms. Georg postulated that we should not only focus on the algorithmic aspects of automata, but it is also very important to seek for connections between various notions, like for example the equivalence MSO = Reg. In relation to this remark he recommended to read an essay "The Two Cultures of Mathematics" by Timothy Gowers, a Fields medalist, which discusses the ideas of problem-solvers and theory-builders. I found this observation about our community very interesting and worth reflection, that's why I mention Georg's remark and reading recommendation.

As the last panel speaker Sophie Pinchinat was describing her point of view. She focused mainly on the need for higher recognition of our area in the society. I particularly liked one observation by her. Sophie noticed that probably the general public does not recognise that there is some basic research within computer science. Indeed, it is widely known today that computer science is one of the most important technologies of our times and probably most of the people are convinced that artificial intelligence changes the world. However, I'm not sure how many of these people notice deep connections of computer science to theory, and to math in particular.

After these statements of panel speakers there was a short discussion between the panel members and longer sequence of remarks and observations from the audience. Several people shared their thoughts, I will focus only on three of those, which particularly resonated with me.

The first remark, which seems important to me, was a question by Marcin Jurdziński. He said that as a community we should ask ourselves a question: "What is our identity?". Do we see ourselves as pure mathematicians? Do we see us interacting and cooperating with engineers? What is our ambition and profile of which person we would like to fit? Marcin mentioned here a few names of prominent researchers, he said: "Are we Euler? Are we Turing? Are we Kanellakis?

Are we Shelah? Are we Wiles? I don't know, who are we?". In my opinion this question is fundamental in our situation, of community situated somehow at the border of several paradigms of doing science. Even people sitting in the same office and co-authoring many papers may have different philosophical approaches to that issue. I've later talked in person with Marcin and he emphasised that his perspective on this question is very pragmatic. It is not only a philosophical question. The answer to it implies our actions.

Another high level thought was formulated by Lorenzo Clemente. He noticed that in the papers we write we often formulate our results in the form: problem $X$ has complexity $Y$. However, when we talk with our colleagues about our recent research we rather share an interesting lemma we have proven or a novel technique, which we have discovered. A conclusion is: maybe we should formulate differently our results? I think this is a very interesting idea. I'll take this opportunity to share my personal opinion on a similar matter. Indeed, we very often study questions of the form: does problem $X$ have complexity $Y$. However, why do we do that? In my opinion the right answer to it should be that these questions are not our goals by itself, these are the questions which guide our research into the right direction. Our true goal in fundamental research should be finding new techniques, new natural notions, new deep connections between mathematical structures. However, it is hard to ask ourselves: find a new technique. Instead, we need more concrete, measurable goals, which guide us towards search for right notions. In my opinion this is exactly the role of the algorithmic questions we ask. They are not the goal, they are the path. Coming back to Lorenzo's question: how should we formulate them? I don't think there is an easy answer to it. Algorithmic formulation is probably easier to appreciate for people a bit further from the problem. Imagine you solved P vs NP problem by a novel, insightful technique. The technique is the core of the new understanding, but you need to say that it solved P vs NP question. Otherwise only a tiny fraction of the community will appreciate this breakthrough. However, at the same time, deep understanding was done by the technique and you will explain the technique to your colleagues. I don't know the answer to Lorenzo's question, but I really appreciate it.

A final remark, which I'd like to share was formulated by Thomas Colcombet. He asked how the culture of publishing on conferences influences our research. I've heard many times discussions about a nonstandard model of conferences in the community of theoretical computer science, but never this particular question - how does it change our way of performing science. I think this is also a question worth reflection.

Summarising, I'm happy that such a discussion could take place. Personally, I find the idea of creating a list of flagship open problems in our area the most inspiring thought from the debate. I hope that one day there will be a right momentum and such a list, together with a prize, will be created and funded. On top of that, I hope that our meeting may inspire more panel discussions on various events in our community, either on similar topics, or very different ones. In any way, I deeply believe it is important to always simultaneously with the actions think where are we heading and why there.