

An Interview with Fred Wan

Frederic Y.-M. Wan served as director of the Division of Mathematical Sciences (DMS) of the National Science Foundation (NSF) from January 1993 to December 1994. Prior to that, he was a professor in the Department of Applied Mathematics and in the Department of Mathematics and the associate dean for the natural and mathematical sciences at the University of Washington in Seattle. He also served as program director for the applied mathematics program in the DMS from the fall of 1986 to the summer of 1987. Wan is now a professor in the Department of Mathematics and vice-chancellor for research and dean of graduate studies at the University of California at Irvine.

The following is the edited text of an interview held on November 14, 1994, with Allyn Jackson, *Notices* senior writer.

NOTICES: *There has been a lot of talk in Washington in recent years about how federally funded scientific research should aim to meet national goals. On the other hand, recent documents, such as the Clinton Administration's "Science in the National Interest", advocate strong support for basic research. Can you comment on these trends?*

WAN: The two are not inconsistent; in fact, they reinforce each other. Within each of the so-called strategic research areas there is basic research. And no one is questioning that the NSF should focus on basic research, except that that research should be tied in some sense to general areas of national need. Sometimes these could be pretty specific, but NSF support is still for

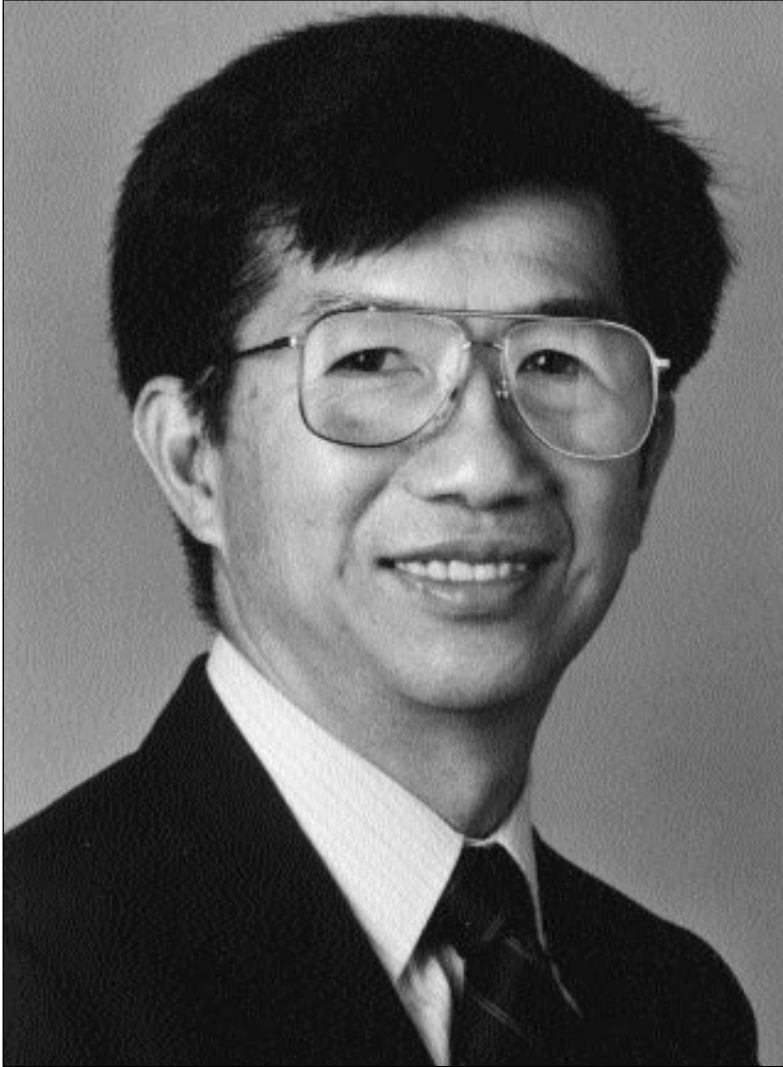
basic research. So instead of doing basic research on anything, the PIs [principal investigators] are encouraged to work on basic research pertinent to areas of national needs.

NOTICES: *Has this trend been affecting DMS?*

WAN: Certainly in the broad sense, yes. It's not necessarily that we agree or disagree with it, but recent increments—for example, for this year or last year—are specifically targeted. So we don't really have any choice, never mind whether I personally feel that targeted funding is a good or not a good idea. Our budget increments for 1994 and 1995 are all targeted to certain areas.

However, we are fortunate that we have a very diverse community, and up to this point, with mathematicians doing what they naturally do, we have been able to meet our targets or commitments to invest in the strategic areas. To be more specific, we have not done anything other than what we do normally—the proposals come in, we send them out for review, they are evaluated on the merits of the reviews, and we fund or decline to fund them according to whether or not they are outstanding proposals. And after we fund them, we see if these awards fit into one of the

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national needs or strategic areas. And if they do, we code them as such. By this process we are able to invest the targeted fraction of our budget in the targeted areas.

Now, this does not mean that the situation will remain this way. If the current trend continues—namely, the increments continue to be targeted—in a year or two, we might get to the point, when we count up all the proposals that fit into a strategic area in a natural way, that the total investment is less than our target in that area for the year. At that point we would have to ask the community to be more specific in saying whether their research is related to some strategic areas. If that total still doesn't meet the targets, then we will have to do something else, such as soliciting proposals in certain areas. But I would like to emphasize that to help the Division continue to meet its goals, the proposers, whenever they can, should relate their research

to one or more strategic area, so that we can count them toward our investment in these areas. [It should be noted that the Republicans who will control the new Congress have indicated publicly since the election that they are not in favor of targeting specific areas for research funding.]

NOTICES: *Can you give me an example of a proposal that says nothing about national needs but which a program officer has been able to put into the "strategic areas" category?*

WAN: Take the case of packing of spheres—geometers work on this problem without saying anything in their proposals other than that it is mathematical research. But in fact packing of spheres has a lot to do with manufacturing and the efficiency of packing and transporting items. In a more applied area, somebody might be studying the buckling of shells. While the applications may be to airplane fuselage design in the mind of the proposer, it could also very well be part of manufacturing. When you manufacture and ship soft drinks in cans, the cans had better not buckle, or nobody will want to buy them. On the other hand, you don't want the cans to be too sturdy (and hence too heavy) either, because it would cost more to transport them. So you have to come up with an optimal design. But the person who studies the buckling of cylindrical shells might not have in mind the shipping of soft drink cans or any other applications in Advanced Manufacturing Technology [one of the strategic areas].

NOTICES: *Are some areas in DMS being phased out because they don't fit in with national needs?*

WAN: No program is completely fixed and static, regardless of whether or not we have emphasis on strategic areas of national interest. We always have changes in our programs, because some areas mature and stop being areas of active research or are so well developed that any further progress would be incremental. In that case the program officer would decide that this is not an area that's worth further investment, the payoff is not so great. In this way he or she gradually shifts the portfolio. This is the case in the Classical Analysis program, in the Applied Mathematics program, and in Algebra and Number Theory—in fact, in all the programs. The programs are always changing, but so far they have not been driven by the strategic areas, mainly because there's already enough diversity in our programs to meet our goals.

So, the answer to your question is yes, we always do phase in new mathematics and phase out the more established, mature areas. But so far we have not done so on the basis of national needs or the strategic areas.

NOTICES: *So what is the basis?*

WAN: Well, take the case of Classical Analysis, which over the years has chosen to deemphasize certain areas and bring in, say, wavelets. When Jack Ryff [program director for Classical Analysis] first decided to seed research in this area, it was not at all fashionable. But he saw the tremendous potential of wavelet research, because we need more efficient ways of transmitting information than the more classical methods. As a result of his seeding, some projects blossomed and caught the attention of DARPA [Defense Advanced Research Projects Agency] in the early 1980s. That agency decided to fund this in a big way, and a \$1 1/2 million research grant was actually channeled through NSF. ARPA, as it's now called, doesn't make grants; they usually get another agency to make them for them.

NOTICES: *Did this money show up in the NSF budget?*

WAN: No. In principle, we get handling fees. But because of NSF procedures being a little more strict than others, the money eventually went through another agency. But, that's an example—Jack's budget didn't grow by leaps and bounds, so in order to seed this wavelet research, he had to deemphasize some other areas.

NOTICES: *What areas?*

WAN: My guesses are nonlinear functional analysis and classical complex variables—I don't really know if that's the case; I'm just giving you some possible areas.

NOTICES: *It sounds like going from more classical areas to wavelet theory is going more in an applied direction.*

WAN: No, wavelet theory itself is not applied. Classical Analysis funds the analysis part of it. The application of wavelets is in the Applied Mathematics program. Also, the classical area of complex variables is very applied—Fourier transforms are also used in signal transmissions. So, the name of the research area doesn't mean anything; it is what you actually do in the research project. And in Ryff's program, all the research projects are concerned with the analysis rather than the applications.

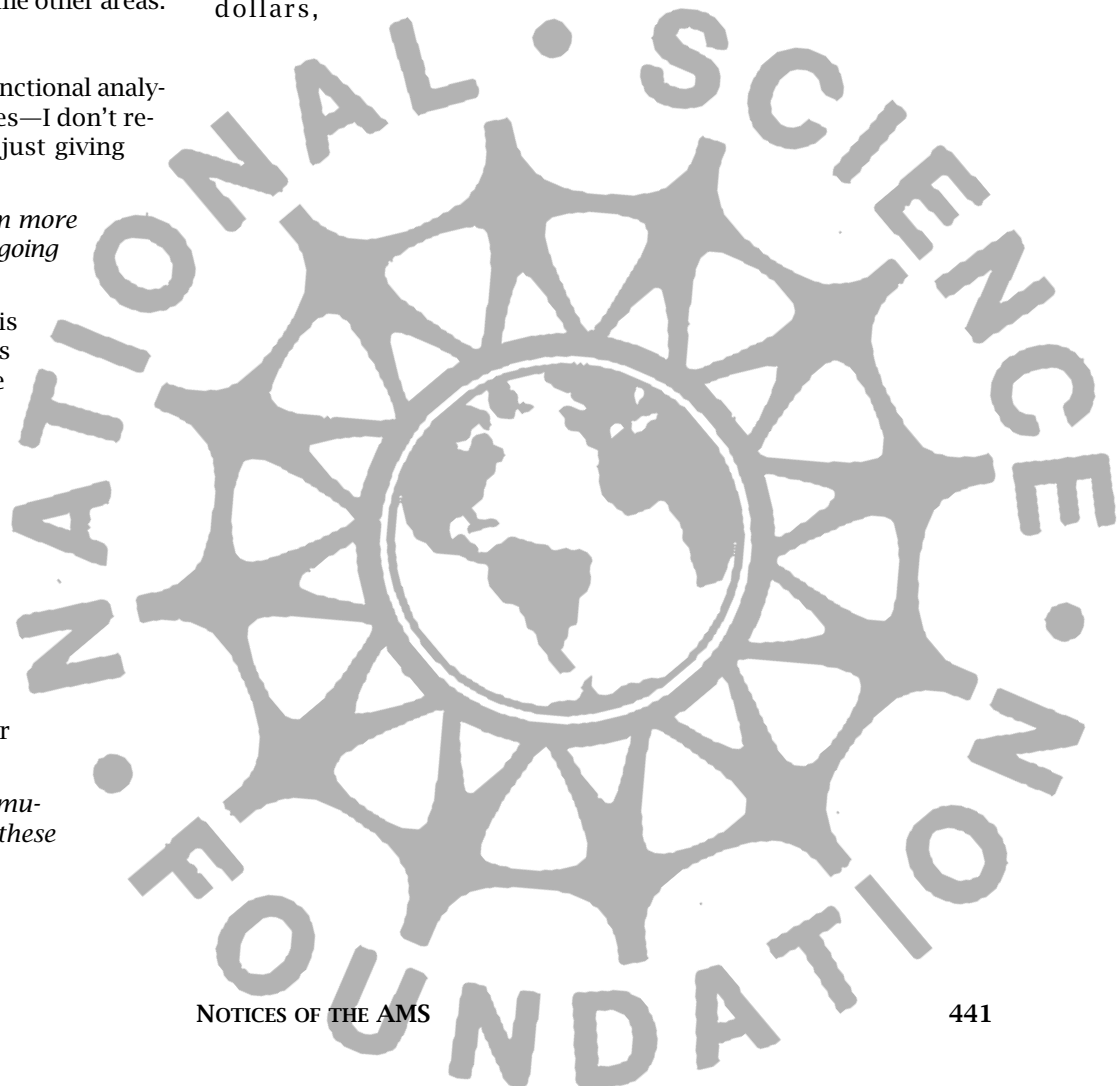
NOTICES: *Many people in the community have been concerned about these kinds of shifts of emphasis.*

WAN: When NSF emphasizes national needs, we are still talking about basic research in areas of national need. It's basic research in different areas, whether it is pure mathematics or applied mathematics, or real applications, so there's a range. There is very applied-sounding research in very theoretical areas and vice versa. In the Algebra and Number Theory program, there are research topics that sound very abstract but have clear applications in national security and cryptography. If proposers can help us by indicating how their research might tie in with these strategic areas, that would help us be more flexible with the use of the available funds.

A Funding Crisis?

NOTICES: *Is there a crisis in mathematics funding?*

WAN: Yes, but not for the reasons that people usually give. The reason for the recent tension is that the size of our community has grown much faster than the average annual rate of growth of the budget. Over the past seven years, since the time of my previous tour of duty at the NSF, the average annual rate of growth of our community is about 6+%—this refers to growth in the number of proposals we receive. But in terms of real dollars,



the average annual budget increase has been less than 2%, if you take away inflation.

The fact that the community has grown at a rate faster than the DMS budget is the reason for the stress in the community, not the strategic emphases. So far we have not felt the impact of the emphasis on the strategic areas. In particular, we have not felt the impact as much as other divisions. I am not saying that this situation will continue—I predict that in a few years' time, if things continue as they have been, we will feel the impact. But so far, sitting in my position, I can say that emphasis on strategic areas has not impacted us severely. Even programs which are extremely theoretical and have participated very little in the strategic areas—like the Modern Analysis program—have a 40% success rate for unsolicited proposals submitted. Some other programs have a 30% success rate. That's a lot better than in the biological sciences programs, which have only about a 10% success rate.

NOTICES: *Do the NSF higher-ups look at the DMS success rate and conclude that the DMS doesn't need more funds?*

WAN: They are not so concerned about the success rate because they realize that part of the reason our success rate is so high is that, relative to the needs of the PIs, our grants are small. And if we had done the normal thing and funded PIs "adequately", we would have a smaller success rate, and it would be more in line with other divisions'. It might still be high, but it would be more in line. So, they are more concerned that DMS grants are small and not sufficient for the PIs to do the proposed research; we are trying to support too many people.

The Foundation's policy is to fund an award adequately. And if we are perceived as not funding PIs adequately, then we could be in trouble with the Foundation. So far, we have adopted a funding practice, which I described in our interview a year and a half ago [*Notices*, July/August 1993], which has been more or less accepted by the directorate. But if we try to fund more people with smaller grants, if that trend does not get reversed, we will be called to the carpet.

Ensuring Strong Federal Support for Mathematics

NOTICES: *What can the mathematical sciences community do to ensure strong federal support of math research?*

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WAN: The mathematical sciences community, like other communities that try to promote more funding, can be successful only if we are not perceived as being fractious and unable to come to an agreement on what is the best thing for the community. We all have different interests, but it serves no useful purpose for one group to advocate its own interests in competition with other groups. We need to agree on a common goal. Perhaps not everybody would get everything he or she wants, but together we could advance with a common goal. That would make it much easier for people who are trying to help us to get us the support. The success of the Astronomical Sciences Division over the years can be traced to that community's willingness to set its own priorities.

There are obviously people who feel that there should be more emphasis on individual investigator grants and less on big projects, like centers or institutes, or on educational projects. The truth of the matter is, we need our investments in all these other areas. Centers and institutes are in fact a very small fraction of our budget. But having that balance in our portfolio is to our advantage. Different types of research require different funding mechanisms. It just doesn't make sense to be single-minded about the kinds of projects DMS funds, because the needs of the community are quite broad. There are different people doing different things, from education to group projects to individual investigator research. I'm not suggesting that people should uncritically support everything. You can be critical to make sure the projects supported are of high quality through the review process. But there should be a common agenda and a balanced portfolio for the mathematical sciences.

I should mention that the mathematical sciences community is well ahead of many other disciplines in terms of activities in areas of national need. In education, we were the first to issue standards—the science standards took years to produce and have only recently been issued in draft form. We have done various curriculum reforms, and while we are not completely successful at the precollege level, we are getting involved. We have a good track record in terms of meeting our national needs in education.

The DMS is the only division—certainly in the MPS, but probably in the whole Foundation—that is participating in every single strategic area. We are the first division in the MPS, and

probably in the Foundation, to have a cooperative research program with industry. This program was so well received that MPS uses our model for other divisions, and we now have a \$5 million, directorate-wide program. The DMS can tap into that, and we are in the best position to do so because we already have our university-industry cooperative program in place, while the other MPS divisions do not.

So, the Foundation, the directorate, and many people in the government have a very good feeling about what the mathematical sciences community is doing. They probably don't know what we do for research, and they don't need to know specifically. I wish they did know better, but even if they don't, they have a positive impression of what we're trying to do. But that's not to minimize the possibility that we will continue to be squeezed, both by the growth of our community relative to the budget and by further targeting budget increments.

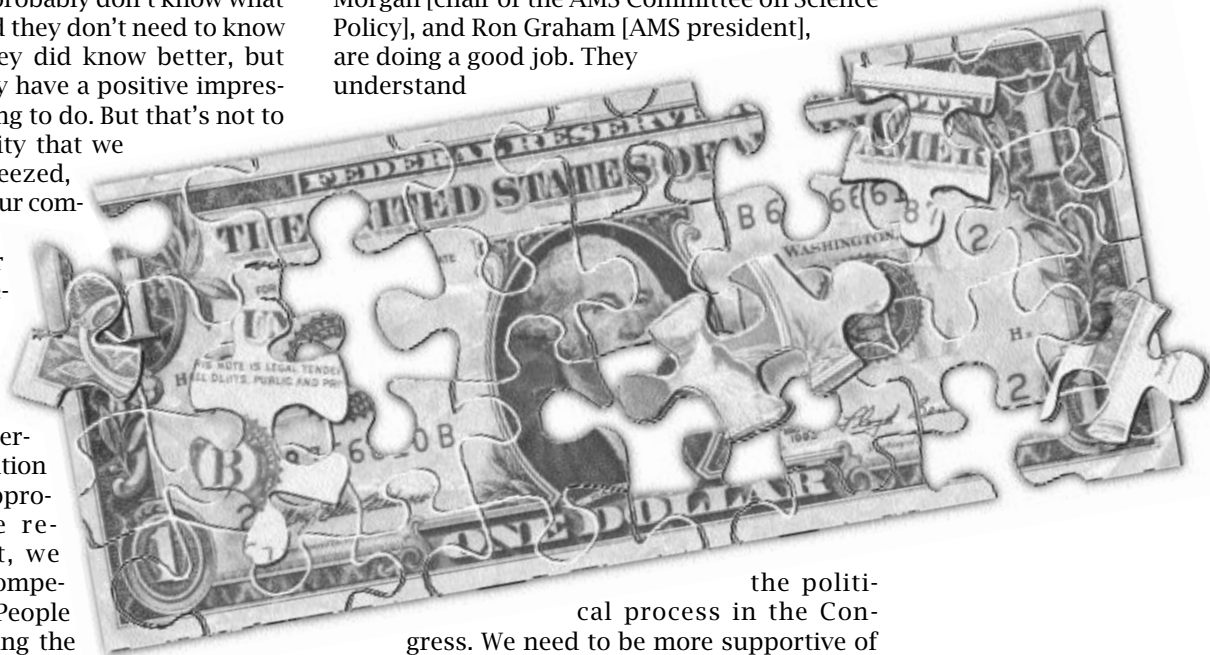
Today the resources available are not as unrestricted as, say, five years ago. But nevertheless, we have to position ourselves to get an appropriate share of the resources. If we don't, we won't get them. The competition is pretty fierce. People who have been watching the physics and astronomy communities can tell they've been hurting much worse than the mathematical sciences. Why? Because they didn't position themselves as well as we did when Judy Sunley was division director. The last year of Judy's term, she put us in a good position to benefit from the current trend of funding. I don't know whether people were happy with it at the time, but certainly we benefited from it, and a lot more people realize that now.

In a related area, I wanted to note the important change in the relationship between the people who fund universities and the university community. There has been a loss of trust or of credibility, depending on whose side you are looking at. One of the most important things the mathematical sciences community can do is to reestablish that trust. When funding was good for research, some faculty did not pay enough attention to nonresearch activities. Hence there was a feeling that universities were not doing for the public what they were established to do. So if we can rededicate ourselves to both teaching and research and reestablish that trust, we will

go a long way in getting back the public support for research in the mathematical sciences. The public is not asking anything more than what they expect the university normally to do, namely, educate their children.

NOTICES: *What else can the community do to make its voice heard in Washington?*

WAN: I think people like Rich Herman [chair of the Joint Policy Board for Mathematics], Avner Friedman [chair of the Board on Mathematical Sciences of the National Research Council], John Morgan [chair of the AMS Committee on Science Policy], and Ron Graham [AMS president], are doing a good job. They understand



the political process in the Congress. We need to be more supportive of them so they can make the case for us. It never hurts to speak to your congressman or senator whenever you're in Washington or when they come home to your district, to let them know what good things the mathematical sciences community is doing and how they should support mathematical sciences research and educational activities.

Will Summer Salaries Disappear?

NOTICES: *There is talk that salary support will soon disappear from NSF grants. Is the NSF seriously considering such a move? If such a move is in the works, how can the mathematical community contribute to the discussion?*

WAN: There's no specific move to eliminate summer salary, in the DMS or in the MPS directorate or in the Foundation. Whenever money is tight, that's always an option that's available, but I don't think there is any serious move in that direction. I certainly would not want to see the mathematical sciences community go out alone on this, namely, to limit its own salary support independent of other sciences. The reason is

very simple: If the mathematical sciences are perceived to be less deserving or to have fewer resources available than other sciences, then talented young people would elect to go into an area which is perceived to be more worthy of support.

NOTICES: *Do you really think it would have an effect on young people? People who are thinking seriously of going into mathematics are not doing it for the money.*

WAN: But it's not even a matter of personal income; it's the perception of how the subject is valued. If what a physicist does is deserving of summer salary and what a mathematician does is not, that puts a value on the kind of work that the mathematician does. I'm not saying everybody would vote with their feet. But there must be minimum support for the research environment. As it is, we already have a terrible research environment in terms of the kind of facilities that we need for research. Even now, when PIs in DMS have support for computing equipment, they are not being provided with system support and maintenance. We usually go with hat in hand to the university to get that support. More often than not, mathematics faculty members become system programmers fixing the machines, and that's not a good way of using their time. If we're not getting treated on a basis equal to that of other basic sciences, it is perceived that what we do is not as important as a scientific or intellectual endeavor as other areas.

There is a basic philosophy of why an agency like NSF is funding research in mathematical sciences. Are they funding it because they are purchasing the research? Or are they subsidizing the research? If the NSF is subsidizing the research, then if the resources are tight, its funding would be treated like a National Endowment for the Arts or a National Endowment for the Humanities. If it is purchasing research that it wants done, then it's got to fund it adequately, or people will do something else.

NOTICES: *Any advice for the next DMS director?*

WAN: I don't really have any advice to give. With the new Republican Congress, things are rather fluid at this time, so it's best to let the person come in and observe and size up the situation. But any Division director should be sensitive to the fact that it's the community that has to do the research and it's the program officer who has to process proposals and do the other day-to-day work of the Division and the Foundation. Different individuals have different strengths and weaknesses; there might be things that I would like to do but cannot, while somebody else can and vice versa. So even with the best intentions,

whatever advice I might give may not be useful for the next Division director.

NOTICES: *What are you most proud of that you did at the DMS?*

WAN: At least the community has not been in an uproar! But seriously, the DMS staff and I have worked together to put in a few new programs, such as the group-infrastructure program. That may not take off fast at first, because we're going to begin with a small project; but judging from the response, I think its going to be an important program. Also, the program for collaboration with industry has been very well received. With such things, it takes a little time for them to settle and to see the real impact they may have. In addition, we reorganized the Division internally to make it more functional. Given that the workload has increased tremendously since I was

here in 1987 and given the changing needs of the DMS, we couldn't survive without some changes. Fortunately the staff has been very helpful in the reorganization process. We have a very good staff.

It's been an interesting and exciting two years. I came to realize more and more the importance of the task at hand and the impact the new Division director can have on the mathematical sciences research community and mathematics itself. He or she can count on me for any needed support I can give for that important task. Thank you.

— Allyn Jackson

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