NATIONAL SCIENCE FOUNDATION

OMB No.3145-0060
NSF Form 1 (5/90)

PROPOSAL EVALUATION FORM

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
DMS-9123836	Temple University	12/09/91

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Doron Zeilberger	ALGEBRA AND NUMBER THEORY

TITLE Mathematical Sciences: Computer-Generated and Computer-Assisted Research in Combinatorics and Special Functions

Please evaluate this proposal using the criteria presented on the back of this review form. Continue on additional sheet(s) as necessary.

(SEE ATTACHED)

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

OVERALL RATING: ☒ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Zeilberger has been contributing some of his best work. [illegible]
Evaluating Criteria, this proposal meets the #1 criterion. They are of significant importance to meet in part also the #2 criterion. (I am assuming that #3 and #4 do not apply here).

Putting all this together I must place this proposal in the excellent category. Although I wish that we had a finer way to grade the proposals and we could distinguish the absolutely excellent proposals from those that do contain some deficiencies. If we had such distinction I would not place this proposal in the very top category.

I am well acquainted with the work of D. Zeilberger. In fact some of his papers are on my reading list and I have covered his best results in courses and seminars. We have here a first class researcher, who has made excellent contributions in exciting areas at the boundary of combinatorics and the theory of special functions. We might refer to this as "manipulatorics" or MICS in brief. There is an aspect of MICS that is worth mentioning here. Basically, because the concepts we are dealing with have such explicit representations in terms of multivariate but finite combinatorial structures, the investigations can often take a remarkably experimental turn. Theorems can actually be "discovered" through computer data and computer directed proofs may be obtained by judicious explorations. Zeilberger is a master at this kind of activity. His algorithms (some jointly with H. Wilf) for the computer construction and/or verification of Hypergeometric series identities will remain a classic in the theory of special functions. There are other past contributions of Zeilberger that may be labelled as "seminal". He can be a brilliant and innovative "game-starter" and tool maker. Qualities that are shared only by very few top researchers in the field. Such are for instance (in his age group) Although I would rank works superior to Zeilberger's in depth and long range applicability. In comparison with the latter researchers I would characterize Zeilberger's activities as "trendy". He is more intent on impressing his contemporaries than digging deeper into mathematical labyrinths purely for the sake of mathematics as it is for and As a result the latter have not yet reached the recognition they fully deserve. For instance, these new highly heralded Macdonald polynomials (mentioned in the present proposal) were first discovered by developed their theory extensively, and formulated some very exciting conjectures generalizing and extending work to the point that hardly anybody knows were it all started. Whatever Zeilberger's motivations may be there is no question that he has done first class mathematics and is perfectly capable of doing more of it. On these grounds alone I would strongly recommend funding.

Now, to deal with the present proposal. I find here a mixture of "competently well planned research problems" (COMPS) and "shooting from the hip targets" (SHOOTS). At some point I thought I was reading a list of the "who is who" in the most famous open problems in the area. I.e. the "Riemann hypotheses" of the MICS field. He is clearly well informed as to what are the most difficult and frustrating questions of today. I can divide the proposed problems into the two above mentioned categories as follows:

COMPS: #1, #2, #3, #4, #5, #8, #11, #14

SHOOTS: #6, #7, #9, #10, #12, #13

I don't much see the point of proposing a hard problem without including some evidence that the proposer is properly equipped (other than with his/her enthusiasm and natural mathematical ability) to solve them. I know of no one in the MICS area that does not (presently) wish to prove the Macdonald q,t-Kostka conjecture, (this is the proposed problem #7). The same can be said for problems #10 (the symmetric chain decomposition for L(m,n)) and for problem #9. (the alternating sign matrix conjecture). And I might add that lately some very good researchers have done a bit more on these problems than just wishing.

Nevertheless, even discarding the SHOOTS. The list of COMPS is impressive enough to warrant recommending support. In fact, I dare say that significant progress in one half of the COMPS should be the limit of reasonable expectations here.

One final comment concerning the COMPS is that they are squarely in the area where Zeilberger has been contributing some of his best work. Thus relative to the provided Proposal Evaluating Criteria, this proposal meets the #1 criterion. They are of significant importance to meet in part also the #2 criterion. (I am assuming that #3 and #4 do not apply here).

Putting all this together I must place this proposal in the excellent category. Although I wish that we had a finer way to grade the proposals and we could distinguish the absolutely excellent proposals from those that do contain some deficiencies. If we had such distinction I would not place this proposal in the very top category.

DMS-9123836, Zeilberger, Temple
Overall rating: between excellent and very good.

The PI is one of the most original researchers in combinatorics.
He is very knowledgeable and hard-working.
He has had a number of very nice insights, of which
the work with Wilf on algorithmic and automated proofs
of identities is a recent and very significant example.
The proposal is a bit of a mess, but there is an underlying
logic to selection of topics, and I am confident that serious
progress will be made.
This proposal should definitely be funded.

Previous NSF support: The work done under previous grants was excellent.

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PAL INVESTIGATOR	NSF PROGRAM
Doron Zeilberger	ALGEBRA AND NUMBER THEORY

E Mathematical Sciences: Computer-Generated and Computer-Assisted Research in Combinatorics and Special Functions

Please evaluate this proposal using the criteria presented on the back of this review form. Continue on additional sheet(s) as necessary.

Principal Investigator, who has a lot of fine and subtle results to his credit, proposes a wide ranging program of computer assisted research in hypergeometric mathematics, related combinatorial problems, in examination of new properties of classical, hypergeometric, Askey, Gessel and Macdonald polynomials. This is a very interesting proposal.

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

OVERALL RATING: ☐ EXCELLENT ☒ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

RATING:

Evaluating Criteria, this proposal meets the #1 [illegible] meet in part also the #2 criterion. (I am assuming that #3 and #4 do not apply here).

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PRINCIPAL INVESTIGATOR	NSF PROGRAM	
Doron Zeilberger	ALGEBRA AND NUMBER THEORY	

TITLE Mathematical Sciences: Computer-Generated and Computer-Assisted Research in Combinatorics and Special Functions

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Zeilberger's work in the past 5 years has been extremely valuable. The WZ-algorithm and its variants are amazing discoveries. The NSF is to be congratulated for supporting this excellent research.

I strongly feel that this proposal should be supported. Zeilberger's past work proves he is well able to assault the problems he outlines here. Some of the problems (e.g. #9) seem extremely difficult to me. However the total package will undoubtedly yield much interesting mathematics under Zeilberger's scrutiny. Research problem 1 alone would cause me to urge his support.

I seldom rate a proposal excellent; however I believe that Zeilberger's proposal (backed up by his recent accomplishments) merits it.

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

OVERALL RATING: ☒ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Evaluating Criteria, this proposal meets the #1 criterion. They [illegible] meet in part also the #2 criterion. (I am assuming that #3 and #4 do not apply here).

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RINCIPAL INVESTIGATOR	NSF PROGRAM
Doron Zeilberger	ALGEBRA AND NUMBER THEORY

TITLE Mathematical Sciences: Computer-Generated and Computer-Assisted Research in Combinatorics and Special Functions

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The Principal Investigator has been doing excellent work in applying computers to problems in special functions and combinatorics. He is a researcher of originality and plenty of energy. The type of work he has been doing in recent years, especially regarding the Macdonald root system conjectures and computer generated proofs is fundamental and has inspired other important work.

The proposal is a assortment of 14 problems. He is the perfect person for these problems. Undoubtedly he will make significant progress on them and in the bargain really enhance our understanding of the applications of computers to combinatorial problems. The problems are of interest, not only in his field, but also analysis, physics and group representation theory. I would strongly recommend this proposal for funding and rate it as an excellent minus.

Regarding his results from previous NSF support, they are excellent, abundant and touching a wide variety of subjects. He has made significant contributions to many interesting areas.

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

OVERALL RATING: ☐ EXCELLENT ☒ ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Evaluating Criteria, this proposal meets the #1 criterion: They are of significant importance -- meet in part also the #2 criterion. (I am assuming that #3 and #4 do not apply here).

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