NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1 (4/84)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
DMS-8600243	Drexel Univ.	11/30/85

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Doron Zeilberger	ALGEBRA AND NUMBER THEORY

TITLE: Mathematical Sciences: Toward a General Theory of Combinatorial Bijections

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

Doron Zeilberger is a creative genius though not always as disciplined in his work as he should be. I've been very impressed with his ability to generate fresh new ideas when studying old problems.

I am not impressed with the content of his proposal. Research problems 1 3 and 4 (pages 6 and 9) don't have enough meat to justify a three year grant and problem 2 (page 8) is too open-ended. I compare this proposal unfavorably to the one of _____ and favorably to the one of _____.

Despite the content, I give this proposal a very good rating. Zeilberger has done exceptional work in the last few years. He is one of the people most likely to settle the Macdonald conjectures and I think this proposal merits funding on his abilities alone.

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

Doron Zeilberger's proof of the q-Dyson Theorem (with Bressoud) is a truly outstanding piece of work.

● Edited at NSF.

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

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PAL INVESTIGATOR	NSF PROGRAM
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Zeilberger is an excellent mathematician who has made exciting advances in the field of combinatorics. His recent proof (with Bressoud) of the well-known q-Dyson conjecture has demonstrated not only his power as a mathematician but the power of the combinatorial method in proving identities.

I found Zeilberger's proposal to be extremely interesting. The problems he proposes are of fundamental importance. Finding a more natural bijection than the Garsia-Milne bijection for the Rogers-Ramanujan identity would be quite worthwhile. Also a general principle (along the lines of the Garsia-Milne involution principle) for bijectifying inductive proofs would be interesting, especially if it leads to natural bijections. This approach to finding a bijection for showing that the distribution of his intriguing new z-index is the same as the major index and inversion index seems reasonable. A Foata style bijective proof of this identity would be very appealing. The problem on symmetric chain decompositions of L(m,n) is a major open problem in combinatorics and it is certainly worthwhile for Zeilberger to devote time to studying this problem.

I am convinced that Zeilberger's talent and expertise in combinatorial bijections will lead to success in his investigation of the proposed topics.

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

The prior work described in the "Results from prior NSF support" section is certainly very impressive. He has continued to provide combinatorial proofs of new and known identities.

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

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One looks for a number of things in a proposal. First, does the proposer have some new ideas, or at least some important problems that should lead to new ideas. Second, has the proposer done deep work in the past, and is it likely that deep work will continue in the future.

The second of these is clearly answered with a yes. The Zeilberger-Bressoud proof of the q-Dyson conjecture is a major piece of work. The introduction of the z-statistic and its generating function showed deep insight. The two problems mentioned in the final paragraph are both very important, and while it is unlikely either of these will be solved by any particular individual in any one year period (well over 50 years of work by individuals have been spent on each of these problems, so far without success, so on probability grounds my statement is clear), they are both important enough so further work should be done on them. Eventually they will both be solved. Zeilberger's record is better than others in one of these areas, since he has solved a problem that is probably not too much easier than the MacDonald conjecture for BC_n. The only other person who has published a proof of a result that is about as deep (or at least it seems to be about the same depth) is A. Selberg. That is very good company to keep.

Most of this proposal deals with one theme - matching is very important. I was not as convinced of this as Zeilberger is, but notice the past tense. Some of Foata's work is very important, and has led to new insights both in combinatorics and in some other fields. The Garsia-Milne proof of the Rogers-Ramanujan identities does not really add any insight to my understanding of these identities, but it does add significantly to the combinatorial tools that are available. Zeilberger has further added to the combinatorial lore in his paper with Bressoud on the q-Dyson conjecture, and

(continued on separate sheet)

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

The Zeilberger-Bressoud result is a marvelous result that I would loved to have proved.

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

here he has also added significantly to our analytic understanding. I am now willing to buy the central nature of correspondences, although not to the extent that Sylvester did in his quotation on page 4 of this proposal. The problems Zeilberger mentions in the body of this proposal seem important, some are probably within reach of current techniques, and others will lead to new techniques as they are solved. The proposer has learned this area very well, and while I feel he is overly optimistic in some of his questions (such as Research Problem 1 on page 6), unless one aims high, one never accomplishes much. Zeilberger's idea of recurrence as the key to some of these questions might pay off.

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Zeilberger has written an entertaining and appealing proposal, although occasional vagueness makes it difficult for a non-specialist to follow it completely. The most serious difficulty is that he does not make clear what he means by "bijectifying" a proof, other than finding a bijection that proves the same thing. Only for the Garsia-Milne proof does he say what he means by "translating" a proof into bijective language. The involution principle deserves an example in some detail, especially since the definition as given is incomprehensible. Also, I was never able to find a definition of the "major index" of a permutation.

Despite such gaps in the details, I nevertheless believe that Zeilberger's command of the material discussed in this proposal is as good as just about anyone's. His contrasting of the various problems and aim to set up a general machinery to translate induction into recursive construction shows a valuable global viewpoint. He has been making progress on the program he proposes, as discussed in his report on prior research. The results have been good.

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

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Doron Zeilberger	ALGEBRA AND NUMBER THEORY	

TITLE: Mathematical Sciences: Toward a General Theory of Combinatorial Bijections

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Quality of Prior Work: D. Zeilberger's recent solution of George Andrews' q-Dyson conjecture [35] is highly original and very impressive. Zeilberger's synthesis of his difference equation approach in [17] and direct combinatorial approach in [20] is brilliant (see his publication list on pg. 15-17). Zeilberger's solution of the q-Dyson conjecture is very important since his work now provides many deep examples (previously conjectured) of transformations and summation theorems for multiple basic and ordinary hypergeometric series.

In addition to his proof of the q-Dyson conjecture, Zeilberger has made other striking and original combinatorial discoveries. For example, he discovered the main ideas and basic algorithms in his bijective proof of the hook length formula (see [19]). Although not as deep as [17, 19, 35] Zeilberger has very recently consistently produced excellent combinatorial and analytical work in [37, 40, 43, 46] and "A combinatorial interpretation of the integral of the product of Legendre polynomial s", by J. Gillis, J. Jedwab, and D. Zeilberger. In light of the methods in [35], the partly expository paper [41] contains a very intriguing approach to the Jacobian conjecture that just might eventually work.

Evaluation of Current Proposal: In his work in [17, 19, 20, 37, 40, 41, 43, 46] Zeilberger has clearly demonstrated a substantial amount of cleverness, persistence, and energy. Furthermore, in much of his work, he has shown a talent for quickly learning and then effectively using previous ideas of others. All of these strengths will be important in his proposed research.

I expect Zeilberger to solve problem 3. By making use of [46] he also has an excellent chance of finding an inductive construction of a symmetric chain decomposition of L(m,n) and solving problem 4. Problem 4 is highly worthwhile doing. Many very good people have worked on 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

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Because of his work in [35] in solving the q-Dyson conjecture and his combinatorial expertise illustrated by his other recent work, Zeilberger has a very good chance to make significant contributions to a solution of the Macdonald conjectures. New, technically difficult combinatorial techniques involving root systems are needed. Zeilberger's originality and excellent problem-solving ability should be a real asset here.

Problem 2 needs a more precise formulation and it is highly unlikely that anyone will find a reasonable solution to problem 1. Nonetheless, Zeilberger's ideas in this part of his proposal are highly original and interesting in their own right. They may well lead to a substantial amount of beautiful new combinatorics and should be pursued.

Based upon the strength of Zeilberger's recent work and the originality of ideas this excellent proposal should be funded for two years.

Comparisons: Zeilberger deserves full credit for his solution of the q-Dyson conjecture. No one else was trying the same type of approach. K. Kadell's analytic methods were elementary but quite different, and everyone else's approach was much more abstract and/or technical. Except for the last few pages of the last section, [35] is almost identical with Zeilberger's original version. Using basic lemmas Zeilberger had already proven earlier in his paper, Bressoud simplified some of the anlysis in these last few pages and replaced the rest by a very pretty combinatorial argument (cancelling involutions). Bressoud's work filled some minor holes in this part of the paper, Bressoud's contributions made this part of Zeilberger's paper more elegant, precise, and easy to follow. However, compared to Zeilberger's major achievements on this problem, Bressoud's ~~contributions (up to that time) were minor and routine. Bressoud has subsequently gone~~ on to significantly generalize Zeilberger's constructions.

Now, [redacted] is deeper and broader than Zeilberger but because of his work in [17, 19, 35], and more recent combinatorial results, Zeilberger compares very favorably with [redacted] and [redacted] Moreover, [redacted] an excellent combinatorialist, but Zeilberger has shown more originality and depth.

Because of his demonstrated talent and original ideas I strongly recommend that this exclent proposal be funded for two years.

I give the following ranking of the individuals I have reviewed this year:

1. [redacted] (excellent)
2. [redacted] (very good)

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This is a first rate proposal. Zeilberger's work in the past has made me a believer in his bijective approach. I find the first five pages of his proposal to be an excellent introduction to why such work is important. Research Problem 1 (page 6) is quite hard; however I believe that Zeilberger is more likely than anyone to crack it. Research problem 2 is broad and important; here it is almost certain that Zeilberger will make significant advances.

Concerning "Results from Prior NSF Support," the work varies from very interesting to absolutely outstanding. The paper with Bressoud is one of the most amazing combinatorial achievements of recent years. I havan't seen [10]; however I know it to be a hard and significant problem and one that Zeilberger is skillful enough to do. Indeed if I could guarantee that someone could do all this out of his NSF proposal, I would be absolutely confident that he should be funded.

I rate this proposal Excellent.

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: [X] EXCELLENT [ ] VERY GOOD [ ] GOOD [ ] FAIR [ ] POOR