NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

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

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
DMS-8800663	Drexel University	11/29/87

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Doron Zeilberger	ALGEBRA AND NUMBER THEORY

TITLE: **Mathematical Sciences: Constant Term Identities and Combinatorial Enumeration**

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

Based on comments from mutual acquaintances and previous papers of Dr. Zeilberger, I have always thought of him as being exceptionally clever but unconventional and somewhat sloppy and disorganized. I was surprised therefore to find his proposal to be well written, well organized and well thought out.

Zeilberger has demonstrated himself to be a competent, highly original researcher under previous support periods. His proofs of the q-Dyson conjecture and the G_2 case of the Macdonald conjectures show that he is well qualified to undertake his latest list of projects.

I find constant term identities to be very important, highly interesting objects. Whether Zeilberger will succeed in finding a general proof of the Macdonald conjectures I cannot say but past performance indicates that he should be supported in his attempts. I rate this proposal as excellent though toward the bottom of that group.

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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INVESTIGATOR Doron Zeilberger	NSF PROGRAM ALGEBRA AND NUMBER THEORY	

Mathematical Sciences: Constant Term Identities and Combinatorial Enumeration

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

The proposer is a highly prolific and original contributor to combinatorial enumeration. His results from prior NSF support include one major result (the proof of the q-Dyson conjecture with D. Bressoud), one ingenious but thus far incomplete attempt to solve one of the outstanding problems of algebra (the Jacobian conjecture), and numerous results of variable significance, ranging from several ingenious gems, a couple of preliminary attempts to begin wide-ranging and potentially highly important programs, and a few rather routine arguments. All in all, a highly productive output which places the proposer among the leaders in enumerative combinatorics.

The proposer's research plans show a great number of ideas at various stages of development. I find all four proposed areas of research highly interesting, and all within the proposer's potential to make significant progress. The least appealing part is the attempt to resolve the F_4 and $F_4^\vee$ cases of the (qM-M2) conjectures with a supercomputer. Assuming their validity, such a proof would provide absolutely no insight into why the conjectures are true. On the other hand, such problems as Conjectures M-R-R1 and M-R-R2 are of great significance and may yield to the proposer's ingenious use of algebraic and combinatorial reasoning.

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 ating is Very Good.

oesn't know where to begin a review of this
. There are so many ideas mentioned with passing
ce that it is impossible to zero in on any particular
t of it. Zeilberger has many balls in the air at one
e. He has a lot of energy. The proposal itself is a
ant. Many of his ideas will lead to important contributions.
Some of his past ideas have led to important concepts.
In particular his work with Bressoud has led in several
different directions. He is likely to continue with this
energetic approach. He is very knowledgeable about the field
which is changing very rapidly. It is quite likely that he
will be able to do at least some of the things he describes
especially when there are computational aspects to them.

I feel Zeilberger is one of the important researchers in this area and should be supported.

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...LE Mathematical Sciences: Constant Term Identities and Combinatorial Enumeration

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This proposal covers topics outside of but tangential to my area of expertise. The proposal covered a variety of topics some of which seem quite interesting and have connections with several areas of mathematics. The proposer has already obtained significant results relating to the Macdonald-Morris conjectures and it seems likely that new results will be achieved. It would be very nice if such results could be achieved without the aid of computers, though perhaps this is not a realistic goal.

I have one concern regarding the proposal which possibly is just due to my own ignorance of the area. Many of the problems discussed are related to some deep questions in mathematics, but it was not at all clear to me that there is all that much depth in techniques described. This opinion is based on a reading of the proposal as well as cursory readings of the preprints enclosed with the proposal.

Still, it is clear that this is an active researcher who will likely make many further contributions. I favor support.

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

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INVESTIGATOR

Doron Zeilberger

Mathematical Sciences
Combinatorial Enumera

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OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD

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TITLE: Mathematical Sciences: Constant Term Identities and Combinatorial Enumeration

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

I have rated the proposal somewhere between excellent and very good. I believe that this work should be supported by the NSF, but I have not seen enough proposals to compare with Zeilberger's to decide more specifically which of the two ratings is more appropriate. In comparison with other people who have worked on similar problems and who currently are receving NSF support, I would rate Zeilberger

Intrinsic Merit: In ~~xxxxxxxxx~~ addition to the keen interest among combinatorialists, the problems that Zeilberger has proposed to work on are of interest in several areas of mathematics outside of combinatorics, such as hypergeometric series, Lie theory and cyclic homology. It is conceivable that the Macdonald/Morris/Mehta conjectures could become the tip of a spectacular iceberg, in the way that Macdonald's affine root system identities became the tip of an iceberg that led to the understanding of Kac-Moody Lie algebras.

One might object to his proposal to use computers to attack the exceptional root systems on at least two grounds: (1) If a VAX is unable to

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resolve F_4, then a Cray is unlikely to resolve E_7 or E_8. (2) Any proof that uses computers is "ugly," and the related criticism: any proof of a

continued.......

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR theorem on root systems that uses the classification of root systems is "ugly." To counter these criticisms, I would suggest that the conjectures are of such interest that we cannot afford to be choosy about how they are proved; the first proof should be allowed any amount of ugliness.

Comments on Previous Research: The quality of Zeilberger's work seems to vary wildly, ranging from routine (such as the combinatorialist's disease of finding bijections in situations where the need is questionable) to ingenious. He has demonstrated remarkable ability to apply combinatorial techniques to difficult problems involving special functions and hypergeometric series. Perhaps his most notable NSF-supported successes are his amazing solution (with D. Bressoud) of Andrews' q-Dyson conjecture, and his solution of the three G_2 cases of the Macdonald-Morris conjectures.

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Doron Zeilberger has tremendous creative talents which give him a realistic chance at solving some of the most difficult problems in algebraic combinatorics. He already solved one important problem in his career, that being his proof with D. Bressoud of the q-Dyson conjecture. The problems in this proposal are important, interesting and very difficult. For most people, I would say that these problems are too difficult. But in Doron's case, I think he has a chance at them. Even if he doesn't solve any of them, he'll put forth some creative effort and I'm sure some new ideas will emerge from his research.

What I question in this proposal is Doron's planned approach to the Macdonald conjectures. As I understand it, he proposes to finish the problem for certain root systems by reducing the problem to a large amount of computer calculation. That completely misses the point of the conjectures in my mind. The Macdonald conjectures as they are stated are of no particular interest. They are important because they are believed to be a manifestation of some deep combinatorial, algebraic, topological or analytic fact. The problem here is to discover and prove the deep underlying fact, not to prove that the constant term identities themselves are true. I would urge Doron to think more about what's behind the conjectures rather than to expend alot of energy trying to prove them by computer.

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