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 semi
nars.
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 funct
ions.
There is an aspect of this area 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.
This particular research proposal is a mild one compared to other Zeilberger
proposals I reviewed in the past. The problems are reasonable and all
lie within the realm of his mastery.

I will only  comment  on two of the specific areas proposed:

General theory of Permutation Statistics:

There are problems in  Algebraic Geometry, Commutative Algebra and
Representation Theory
that lead to (and may be solved by) advances in the enumeration of
permutation according to certain statistics. So that the area itself is
significant in contributing tools and insight in other areas of
Mathematics. The deepest and most useful contribution of this nature
is in the now classic work of Lascoux-Schutzenberger on the "cocharge"
statistic and its implications in the Algebraic Geometry of the Variety
of Flags that are fixed by a given unipotent matrix.
However the proposed problems here are slanted in a direction which

a bit overworked ⊘ the last decade. They may⊘be of interest
⌀e combinatorists ánd, in principle, could turn out to be useful
⌀ther areas of Mathematics, but I would attach to this event low probability

⌀he reason for this is that the examples cited are somewhat naive and tired:
statistics such as "maj", "inv"   and other overworked variations of them.
Also, the results that have been obtained in this direction are mostly
ad hoc. The proposer aknowledges this in framing his question as
"Develop a general Theory of Permutation Statistics..."
However the proposer only suggests that he plans to develop such a theory from
"within" the subject.. A more exciting approach and more likely to produce
significant new additions to the subject would be to develop such a theory
from an  Algebraic Geometrical, Commutative Algebraic or
Representation Theoretical  point of view. To cite an example in point
I should mention the $q,t$-Catalan studied by Garsia-Haiman, which involves
a new pair of statistics on Dyck paths. Any progress in understanding this
pair (Combinatorial or otherwise) could have far reaching implications
in the Algebraic Geometry of the Variety of "Commuting Unipotents".
However so far the "pure" combinatorists have remained shy of this problem
and the only progress to date has been the (Algebraic Geometrical) proof
by Mark Haiman the $q,t$-Catalan does give the bivariate Hilbert series of
Diagonal Harmonic Alternants.

Refined enumeration of Alternating Sign Matrices:

The magnitude of Zeilberger's exploit in its proof of the Mills-Robbins-Rumsey
conjecture (assuming that it has been finally corrected), does indeed
put him among the top contributors to the Theory of partition (and related) ide
ntities.
That places him at par with such people as                        ,
Yet we must appreciate that there are further orders of magnitude to be
reached even in the very narrow area of constant term identities.
I have in mind here the absolutely spectacular work of                in his
complete solution of Maçdonald constant term conjectures. The latter feat
is by no means a "brute-force" "ad-hoc" verification of the validity of
some identity (as was most of the previous works on the subject), but
it resulted from some truly fundamental advances in the Theory of Affine
Weyl Groups. Advances which will benefit and enrich researchers in every
area of Mathematics that has any connection with Roots Systems and their
associated Hecke Algebras. I do not expect Zeilberger's further contributions
to this area to have anywhere near such an impact.


In conclusion, we have a proposal here by a very distinguished researcher
who is at the very top of the purely Combinatorial and Special-Function-
Theoretical Scale. It is likely to produce significant results in the
specific areas proposed but perhaps not likely to lead to contributions
of benefit to the wider community of researchers.

The stature of the proposer and the nature and importance of the problem

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proposed should certainly  place this proposal within the required 10%
and place it in the "Excellent" category. Yet I am a bit confused by the
further requirement that it should also be "truly outstanding".

2. Proposal Number: DMS-9500646

3. P.I.'s Last Name: Zeilberger

4. Overall Rating: Excellent

5. Suggestions for Other Reviewers:

## Assessment of Proposal

This is an excellent proposal that involves various-aspects of algebraic combinatorics, special functions, and computer algebra. Zeilberger is an exceptional problem-solver who has a definite talent for combining techniques of bijections, $q$-difference equations, and symmetry to solve difficult combinatorial problems. One example is his earlier proof of the $q$-Dyson conjecture (with D. Bressoud), and more recently his proof of the alternating sign matrix conjecture (carefully checked by D. Bressoud). Zeilberger has more than enough talent and energy to solve a number of the fundamental problems in his proposal. Zeilberger offers clear, detailed outlines of how to attack several of these problems. In addition, he is an expert at using computer algebra not only to discover what is going on, but also to construct proofs. His achievements here (some with H. Wilf) are quite impressive. Zeilberger's first problem of developing a general theory of permutation statistics is elegant, and he should make quick progress here. His second problem of improving the algorithm for mechanical summation is technical in nature, and should be doable. The third problem of extending the ZW proof theory to more general classes of functions is in an early stage, but is quite interesting. Here, I would like to see the extension to *bi-basic* and *multi-basic* equations. The real depth of this proposal involves the fourth problem of proving the refined alternating sign matrix conjectures, and their consequences. Given Zeilberger's recent work, solid progress here is very likly. The fifth problem of developing holonomic methods in combinatorial statistical mechanics is of general scientific interest and should lead to some deep combinatorics. The last problem of developing a theory of computational enumerative combinatorics should eventually succeed, but this is very long-range. Much of Zeilberger's previous work directly supports all the problems here. The immediate impact of problem I and the depth of problem IV are already enough to fully justify three years of funding.

Zeilberger's excellent proposal of important, original research should be funded for three years, as requested.

**Comments on quality of Prior Work:** Much of Zeilberger's prior work, especially [2,5,8,14,15,16,37], which is outlined on pages 1-5, is excellent. The papers [2,5,8,14,15,16,37] have already had a substantial impact on combinatorics and its applications.

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**Comment on Education and Human Resource Development:** Zeilberger has had three Ph. D. students (finished in 1993-94), and has three current students. He has also taught both graduate and undergraduate courses in using Maple and Mathematica to do research in mathematics. His impact in this area is excellent. Computer algebra is clearly important to all aspects of his research and teaching program.

**Comparisons:** Zeilberger is an outstanding researcher in algebraic combinatorics, special functions, and computer algebra. His work compares favorably with                and
           This is one of the most interesting proposals I have reviewed in recent years. Of the proposals I have reviewed this year (1994-95),                is stronger that
and                (who are all excellent), and all of these are so much better than        .
(who was only good), that such a comparison is hardly worth making. These comparisons do take into account the differences in professional experience of the researchers involved.

I was sent 5 proposals in algebra to review this year, 4 were renewals,
_                                      . Zeilberger)
and one did not have recent NSF support _              .

All 6 researchers are well-known and worthy workers, and will definitely
make progress.

I will combine my evaluations.

The most original of the six is Zeilberger,
who has solved several important open problems. He has a future program for
mathematics by computer that is controversial and sometimes overstated,
but his research in this area is influential and path breaking.

Outstanding leaders in the US in their areas are            and            .
        is broader (and more senior) than            , but both
have teams of graduate students working on their programs.

        ,          , and,          have worked in closely related areas. There
are more good workers in these areas, mostly because of the excellent
students of Stanley at MIT. So there is more competition here.
Of the three,        has the best record of imaginative and innovative work.

On money matters, each proposals asks for 2 months support
for each investigator. Clearly the                proposals is distinct
because of the excellent past work with undergraduates. Also note
that            does not allow grants from NSA, thus they could not apply
there.            has the best record so far on PhD students who have gone
on to productive careers.

OVERALL RATINGS

Zeilberger Excellent