

UNIVERSITY OF CALIFORNIA, SAN FRANCISCO

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

SCHOOL OF MEDICINE

SAN FRANCISCO, CALIFORNIA 94143-0502

Department of Microbiology
and Immunology

January 23, 1990

Maxine Singer
Carnegie Institution of Washington
1530 P Street, Northwest
Washington, D.C. 20005-1910

Dear Maxine:

Thanks for your interesting correspondence about retroelements. Although I agree with your general assessment of the table in Hull and Will's meeting report, I wonder whether their formulation is taken seriously enough as a proposal for classification to warrant a direct published response to it (other than, perhaps, a very short letter correcting their coassignment of LINEs and SINEs to the same category). I think anyone wanting to find (or cite) authoritative viewpoints will rely upon the ASM's new tome, Mobile DNA, earlier reviews, or the evolutionary papers from Eickbush or Doolittle.

In my experience with classification and nomenclature, proposals for change only have merit and compel usage when there is widespread confusion or disagreement in a field (e.g. over the names for the AIDS virus or for viral oncogenes) and when most of the major parties are willing to abide by the agreement. I don't sense a great need for much new definition (although I occasionally wish the term "retrotransposon" were better defined and that "retroposon" and "retron" would disappear) or any serious disagreements (assuming the Hull and Will gaff with SINEs was simply an error). No doubt we could generate some fights over the terms "retron", "pararetrovirus", and "retroid element", but I'm not persuaded it's worth the effort to resolve at this stage.

If you intend to go ahead with some formal statement in TIG, I would suggest the following:

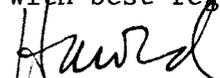
- that it be cast as a general statement by several leaders in the field of retro elements, with the intention of codifying usage and clarifying criteria that define types of elements, not with the intention of setting Hull and Will right;
- that you give equal weight to two criteria: the presence of an LTR and the ability to encode RT...this would give three major groups: active elements with LTRs, active elements without them, and the passive elements (noting that some [processed pseudogenes] are transposed but not transposable);

Singer
January 23, 1990
Page Two

- that you emphasize the point made at the bottom of your proposal about derivation from active elements, to be sure that mutants unable to encode RT will still be classified among the active elements;
- that you include plant retrotransposons (Tal and Bsl);
- that you find some special niche for DIRS-1 (whose LTRs are inverted) and TOC1 (see EMBO J.7:1917,1988) whose LTRs are complicated;
- that DNA viruses NOT be called retroviruses (or pararetroviruses);
- that msDNA be placed outside the classification scheme until matters are better defined (e.g. there is no evidence that msDNA itself is transposed and the extent of the element that has entered occasional E.coli genomes has not been determined);
- that other means of classification be acknowledged in an accompanying statement (e.g. primers for the DNA strands make caulimoviruses seem closer than hepadnaviruses to retroviruses, sequence comparisons show Ty and copia to be closer to each other than to other fly elements, etc.);
- that a slightly more complete description of each type of element be given (the terse phrases for SINEs and processed pseudogenes seem particularly prone to misinterpretation).

I hope these comments are useful. If you do decide to proceed, I trust you will give me an opportunity to endorse (or argue with) the formal statement.

With best regards,



Harold E. Varmus, M.D.
American Cancer Society
Professor of Molecular Virology

HEV/aa