Dr Joshua Lederberg
Department of Genetics
School of Medicine
Stanford University
Palo Alto, California

CDC
1600 Clifton Rd
Atlanta 22, Ga

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My dear Joshua - Recently I received from Jimo several reprints and among them was one of yours on alternative phase 1 loci (or duplicate phase 1 loci) which interested me very much. It was most unfortunate that the strains involved had such low phase variation rates since this no doubt hampered the work.

Intermittently for about 2 years I worked on some cultures of S. mikawasima which had 4 reversible H phases (6,7:y:z47:z50:e,n,z15). These behaved as diploids (?) and segregated triphasic and diphasic forms in which e,n,z15 was constantly present, i.e. one obtained stabilized forms such as 6,7:y,z47:e,n / 6,7:y:z50:e,n / 6,7:y:en / 6,7:z47:en / or 6,7:z50:e,n. After reading your paper I felt very strongly that we were dealing with a triplication of the phase 1 locus.

I've had the idea that these strains were recombinants and have wanted to do some antigenic work on recombination but this now is out of the question unless I can get a good boy to do it. After resisting for some years I finally broke down and took over the job as chief of the Bacteriology Section so I'm afraid my days of working in the laboratory are finished. In talking with Les Baron recently I gained the idea that antigenic recombination was a tough nut to crack but with proper systems of selection and known F+ strains I do not see why it couldn't be accomplished

DTC

I wish you would put some of your people on this problem since we probably will never get to it

Also complex phases like $z_{27}, e, h : z_{27}, 1, 2 / z_{49} r : z_{49}, 1, 5 / z_{45} e, h : z_{45} : l, w$ pose a distinct problem. We've done some unsuccessful transduction experiments with these

Kindest regards

Phil