June 24, 1957

Dr. Elizabeth Russell
Jackson Laboratory
Bar Harbor, Me.

Dear Elizabeth:

I enjoyed your letter. It dawned on me when I received the routine batch of reprints from Jaxlab that the Silvers' papers gave the clue to my question, but I was delighted to get your details. In fact, you may be hearing some from Newton Morton (the other "medical geneticist" here) about possible studies on dystrophies.

Certainly we will keep in mind your remarks about collaborative projects. Nothing would delight me more, and I have never been able to understand why Wisconsin hasn't gone in more for this type of mammalian genetics. I wish I could get in on it personally, but this won't be possible at least for a while. We're going to Australia this summer on a Fulbright trip (a bare 3 1/2 mos. as it now turns out: in fact, weren't you on one of the boards that reviewed our applications?) and have some plans for next as well.

We did visit Bar Harbor in the summer of 1952 & were sorry you were away (at the Genetics meetings at Cornell). We had a wonderful time then, and won't miss a chance to do it again when we can.

Another question I could probably get from the papers: if you use histo-incompatible ovaries and get fertilizations before they are destroyed? Just a point of curiosity—and a system where you might get a cleaner answer to questions of maternal prenatal effects of induced tolerance, than where mother and offspring share at least half the factors. In fact, has this point been tested even crudely. E.G. If you backcross an XY hybrid (with respect, say, to $H_2$) to XX, are the XX progeny still intolerant to Y grafts? (This is the reverse of the usual question of immunization of the mother). I suppose they would have to be or the effect would have confounded the genetic analysis of histocompatibility long ago; You get the same experiment in F2. As you can see, I'm just thinking out loud about these matters. Some year, I hope we will have some work going on here along these lines.

Yours sincerely,

Joshua Lederberg