

Jemski

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Joseph Jemski
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Dear Joe,

Always good to hear from you. I had seen the article by Laurie Garrett. It was sent to me by the National Academy of Sciences because, as it turns out, I am a member of the committee to which Garrett refers. Of the six projects we have funded so far, one makes use of anthrax toxin in a novel way to make vaccines against various antigens other than anthrax itself. The Russian work on the project is being done in a former BW-related lab about an hour's drive south of Moscow. I have been there twice on site visits for our committee. One of the Russians will be coming to Harvard Medical School to work with Professor John Collier for a few weeks. Collier is the American partner in the project. The Academy has now submitted a proposal to DOD to expand the Russian-American collaboration by a factor of about ten.

Laurie Garrett makes quite a few mistakes in the article, for example, our committee is not a joint DOD-NAS entity. It is purely an NAS committee. DOD provides the money but the committee is independent. Also, the largest grant is not \$20,000 but more like \$200,000.

But the worst mistake is the implication the antibiotic-resistant anthrax strain developed by the Russians is a pathogen. Actually, it is a non-encapsulated strain and is not a pathogen. The fellow who isolated it, Pomerantsev, is the one coming to Collier's lab. I have met him a couple of times in England and in Russia. For your interest, I enclose the abstract describing the strain that he presented at Winchester in England and the article published in BIOTECHNOLOGY the following year. As you will see, it makes some sense to use an antibiotic-resistant strain if one is to use a live vaccine. It allows you to give antibiotic therapy at the same time as immunization.

Paul Jackson at Los Alamos has isolated anthrax DNA sequences from tissues of Sverdlovsk victims. The work will be reported soon in the PNAS.

More than a year ago, I received a manuscript about anthrax dose response from Tom Collins at JAYCOR. I meant to send it to you and Bud Larson at the time, but other things intervened and I never got around to it. Your letter gives me an excuse to enclose it now. As you will see, it argues that the probit slope is larger than the value 0.7 that Glassman published based on your data.

There are, however, several problems with the manuscript and I have heard that there is a revised version. I enclose two copies in case you want to give one to Bud. I also enclose a couple of copies of a description of the calculations I made for our SCIENCE article.

I hope the opportunity comes along for me to visit Detrick again to visit with you and Larson. Also, I have never met Bill Patrick and Norm Covert, and it has been many years since I have seen Riley Housewright.

With all good wishes,

Sincerely,