Dear Dr. [Name],

If I wait much longer, I'll be sending more of a pamphlet than a letter. That letter starts my making a table of molecular weights etc. and then talking about them.

<table>
<thead>
<tr>
<th>Species</th>
<th>No Antibody</th>
<th>$s 	imes 10^{13}$</th>
<th>$d 	imes 10^{-7}$</th>
<th>M</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pig $B_2A$</td>
<td>72 %</td>
<td>18.7</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$W_A$</td>
<td>10 %</td>
<td>17.5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$W_B$</td>
<td>15 %</td>
<td>18.5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$W_B$</td>
<td>84 %</td>
<td>17.4</td>
<td>1.64</td>
<td>1,040,000</td>
</tr>
<tr>
<td>Cow $D_3$</td>
<td>100 %</td>
<td>18.2</td>
<td>17.9</td>
<td>1.64</td>
</tr>
<tr>
<td>Horse 902 $A$</td>
<td>44.5 %</td>
<td>17.7</td>
<td></td>
<td></td>
</tr>
<tr>
<td>902 $B$ in 15% salt</td>
<td>19.7</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>902 $B$</td>
<td>58 %</td>
<td>17.2</td>
<td>16.7</td>
<td>1.63</td>
</tr>
<tr>
<td>Rabbit $4564A$</td>
<td>86.2 %</td>
<td>7.0</td>
<td>4.23</td>
<td>159,000</td>
</tr>
</tbody>
</table>

I also made a smaller amount of 17 component.

In my last letter I told you about the pig $B_2 A$. I then made a fellow call from Pig 10, called 901 in the table. This only had 10 % $A$ but showed 43 % of the 17 component in the centrifuge. On further purifying the antibody by the agglutination method $W_B$ only a single component resulted. This would indicate that the antibody 17 component is not concerned in the specific lift.

Horse 902 $A$ 30% salt extract showed only a single peak as did 902 B in 15% salt extract. The original serum contains 20 % antibody and 20 % 17 component.
in the ultra centrifuge. On purification, however, the resulting P02B was only 58.2% antibody. Apparently something happens in the process which alters the antibody in such a way that it does not affect the sedimentation constant or homogeneity. It certainly would be interesting to know why the P02B antibody we began with was so different.

The rest of the table speaks for itself. I am quite pleased having been able to get enough monkey antibody for an analysis, two centrifuge runs and a diffusion run. It seems to belong to the softer group within the 40% antiserum. Since the 40% component is less than 10%, the 2 component can also form antibody in the monkey. The original precipitate from all the 20 cc of monkey serum looked so small that I didn't expect to get anything.

I haven't any more different antigen to work with, but am in the process of making a large amount of horse antibody and will begin a study of the pH stability range of the antibody correlating it with quantitative precipitins, which should be of considerable interest.

I had dinner with Dr. Kallbo last week. He is trying to get me some sera from convalescent pneumonia patients — they don't use serum therapy in Sweden, so I may be able to get some human antibody. He will also try to arrange for me to do complement fixation on some of these antibodies, but if possible they had better be done in N.Y. Also, Mrs. Kallbo sent their regards. They have a very lovely baby which you didn't see long enough to see.

The vaccine stock over with me is marvelous. Dr. Daubos certainly knows how to make the bugs stay inside. I have prepared two antibody solutions — first horse and the goat method and have run a good many determinations.

We also have some very exciting antitoxic results. It seems that horse antibody forms a new component in horse serum which migrates between the P and S components. On removal of the antibody this component disappears. I am enclosing a picture of the whole serum and absorbed serum. These sera will be available in a few days and we are planning to run the antibody prepared from the serum serum (2028) and then perhaps send a note to Science. I hope you will be willing to send it folks, since it will save a good deal of time in writing proof back and forth etc.
The dye serum albumin arrived safely and we have made a scale and done some more cataphoresis experiments. The scale values showed a strong agreement with $S = 1.6$ as compared with a value of 5.1 for the light absorption method. It appears at first glance that the scale values are not quite the same. Dr. Pedersen feels that we are definitely dealing with a substance of the same order of magnitude as the dye (the spectral absorbance of the dye is probably much different from that of a protein) and that the dye protein is fairly well characterized. I am also sending a curve of the cataphoreses and the values of $S$ for comparison.

I think that a short paper for the J. E. T. N. would come along further to emphasize the need for continuing work using pure proteins as the starting material.

Dr. Pedersen and I are planning to make a micro cataphoresis run using the light absorption method to get a more quantitative idea of the cataphoretic homogeneity than the earlier efforts, and we will then write something out and describe it further.

I am Dr. Knipps's $K$ fraction, and the case is extremely complicated. It looks much more complicated than the blood serum of anything else. We haven't had a chance to do much with it, but I'll write him more about it as soon as I can. Please apologize to him for not having written and tell him that I will write soon.

I had a very enjoyable time in Stockholm a few weeks ago. Prof. Simon announced to give a lecture and I spent the week-end there. I had a long talk with people who is doing some really exciting work. He has found that hemoglobin is produced by the blood cells of the liver. He asked me to apologize to him for his not having written and said that he will soon send you another paper and a long fore the latter. Also with Brown, this is an enthusiastic and able work. We had a long discussion of current problems in the field and he sent me a copy of his thesis which is well worth having. Unfortunately his is unable to continue any of his work since he has no facilities and a full time teaching
I also dropped in on Hildebrandt who sends his regards. He was going to a faculty meeting but took some time to show me the plans for his new institute which is being built. Dr. Kellogg is leaving in a few weeks to work there. Hildebrandt invited me to come back and spend an afternoon looking over his laboratory etc. I also met Hugo Theorell and had a very interesting talk with him.

The people here are all wonderful and I am certainly trying to make the most of the opportunity to learn a great many new things. Dr. Fiske and Beversot expect someone from the foundation to come upstairs and see them. They feel sure they will have no difficulty in convincing him that the foundation would be making a mistake if they didn’t buy me a micro-calorimetry apparatus to take home.

I’ve been somewhat busy but I can easily hope for the best.

Prof. Sweezy just returned Saturday. He had been extremely busy, but stopped in the hall to tell me what a nice time he had at the laboratory.

Dr. Pedersen showed me your letter – both you and Prof. Sweezy must have had an extremely interesting and hectic time.

I could only find two unused U.S. stamps on my last trip to Stockholm. They were commemorating the Sullivan Expedition issued in 1927 in 24 denominations, and were listed at twice face value so I didn’t get them, but if you want them, you can see if they’re still available next time. The store you found out on the web actually didn’t have much of a stock.

Dr. Honefeld is coming to work here for six months or a year and is expected before Christmas. It will be quite pleasant to have another immunologist in the laboratory.

I was very glad to receive your first letter and learn that you had gotten settled after getting back. Regards to everyone in the laboratory.

Sincerely,

Elsie

P.S. If you think we ought to send a note about the molecular weights, we can do that too.
Thought I'd left something out. I also saw the 792 DD (0.756 vaccine extract — preservative). Most of the protein had precipitated on transportation, but there were definitely two components — so I definitely traceable to the original serum. Since they are not stable like any of the others, I suspect that some preservative must have been put in or something — that was the salt in the workd plant — ether which they told us had no preservative when you stored and added them. I wish there was some way we could check from it. The 15% salt extract had two 'components' according to Dr. Palmer and we about 11–13 which would also indicate some degradation.

I also had a long chat with Dr. Trowell this afternoon and he asked to be remembered to you and Mrs. Heidelberger.

I am enclosing a check to Dr. Palmer to pay for the refrig.

Miss Seibert asked if you have any antibiotics which react with TB carbohydrate as she is anxious to test some of a polypeptide which she separated from the Tuberculin by cataphoresis. The polypeptide is non-toxic. Miss Seibert said that she wrote you about it some time before I arrived, but hadn't heard from you. Miss Seibert also sends her best wishes as do the rest of the laboratory.