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DEPARTMENT OF GENETICS

Dear Viktor,

Many thanks for your long letter and the response to my Dubrovnik talk. As I pointed out in the very beginning of that lecture (page 1), it was not my intention to present a "sound and objective history of embryology". Obviously, I took advantage of the chance to give strong expression to my personal views. So, my response to your criticisms will be expressed in the same manner. (I hope your do not mind the typewriting which makes it much easier for me to write this letter.)

1) To your point about my comments concerning the possible biochemical or molecular nature of the organizer: Obviously I am as aware as you are of the work of Toivonen, Saxén, Tiedemann, etc. Nevertheless, I am not alone in not putting much weight on their contributions toward the identification of the "biochemical basis of the organizer" (cf. page 8 of my lecture). why? You might be interested in a paper by Gurdon, of which I enclose a copy in case you don't have it. Gurdon's paper makes it quite clear that many different processes can act as embryonic inducers. At the same time, the actual molecular nature of the organizer has turned out to be, to quote Gurdon, "an extraordinarily recalcitrant problem". It seems to me that some current work by Kirschner and Melton is beginning to point the way towards the actual identification of the molecular mechanisms involved in early embryonic inductions. I enclose copies of those papers as well.

2) You refer to a "factual error" in my paper. However, I mention on page 4 the fact that Else Wehmeier's name did not appear on the relevant publication. I did not refer to what you report in your book, namely that Spemann <u>mentioned</u> her name. I still maintain that her name should have been among the authors of that paper.

3) As to the sad story of my Ph.D. thesis: even though you expected it to provide a stage series for your planned species hybridization experiments, Rotmann was not really as much interested in such a stage series as he was in the particular pattern of differentiation of the extremities, a pattern which differed in the two species. In that sense my descriptions were essential for the interpretation of his transplantation experiments.

There is no question that in the final analysis you and I had different relations with Spemann and this, of course, strongly effects our ~~any~~ possible analytical approaches at this time. It is quite interesting that Holtfreter would probably tend to agree with me. I conclude this from the interview which Moscona published in Cell Differentiation.

However, enough of that. I was glad to learn from your letter that you are flourishing and that Spring is bursting. The trees are in bloom here but the weather is so grey that colors seem somewhat out of place.

With warmest wishes,

Yours,

Salome