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Curt Stern

In the 1920's and 1930's there were two centers of biology in Berlin. One in the north around the Museum für Naturkunde, the Zoological Institute, the College of Agriculture, and the Medical Institute. The other focus centered around the Kaiser Wilhelm Institut für Biologie and the botanical institute at Dahlem in the south of Berlin. The naturalists were concentrated in the north, the experimentalists, physiologists, etc. in the south. After the reports of Mayr and Rensch, who studied or worked at the northern center, it will be valuable to have an 'eye-witness' report of someone who worked in the south. Although Curt Stern is not an evolutionist in the narrow sense of the word, he always took a considerable interest in evolutionary questions.

Curt Stern was born August 30, 1902 at Hamburg. He received his Ph.D. in Berlin (1923) with a thesis on the morphology and physiology of certain protozoans (under Max Hartmann). From 1923 to 1933 was a scientific investigator at the Kaiser Wilhelm Institute in Berlin-Dahlem, loosely attached to Richard Goldschmidt's department, but doing independent research. Originally Stern's thinking was much influenced by Max Hartmann, who was one of the most outspoken oppenents of Kammerer.

Of decisive influence on Stern's career was a two year fellowship in T.H. Morgan's laboratory at Columbia University (November 1924-November 1926). There was a second shorter stay with Morgan in Pasadena (December 1932-March 1933).

"During my two years in the fly room at Columbia University with Morgan, Bridges and Sturtevant discussed various aspects and particularly in the case of Sturtevant, evolutionary aspects of the classical genetics which were conducted at Columbia University in the middle 1920's. Sturtevant definitely was interested in evolutionary questions and so was Morgan. Sturtevant in fact was actively interested in speciation problems with ants, not because he was interested in ants themselves, but because he wanted to have another species, very different from an ant, to be used in considerations of the species concept and various similar things. Bridges, on the other hand, was not interested in evolution. The discussions in general were not of evolutionary problems but of chromosomal properties."

Much of Stern's genetic research has evolutionary significance, not only his classical studies on crossing over, but also his work on interspecific sterility (1936), and his research on isoalleles (a term coined by Stern) (1943).

Curt Stern was not able to attend the conference*, but answered a set of written questions.

Concerning factors that delayed the coming of the synthesis he commented:

"After 1910 the interest in evolution decreased. The reason was not a disenchantment so much with the problems of evolution, but with having reached a dead-lock concerning the contributions of different factors to the possibilities of evolution. Bateson in 1922 gave an address in which he explained evolution in terms of loss of genes, so that in every evolutionary step there would be more and more genes lost. This of course did not seem likely, but the interpretation of Johannsen's work with pure lines seemed to represent a difficulty which was not easily overcome. It is also true that the best minds became preoccupied with

* On the Evolutionary Synthesis, American Academy (1974)

different areas of research such as chromosomal genetics, Entwicklungs-mechanik and other branches of biology flourishing in the 1920's." How drastically the interest in evolution had declined among the experimentalists is well illustrated by Max Hartmann's magistral work on General Biology, a volume of 756 pages in which only 5 pages are devoted to evolution.

"The synthesis was delayed greatly by the different opinions of paleontologists and other biologists regarding the question of inheritance of acquired characteristics. There was an attempt in 1929 to get various opposing groups together, as the German genetic society and the German paleontological society met in a joint meeting of several days duration to discuss particularly the question of inheritance of acquired characteristics. Weidenreich spoke in favor of an inheritance of acquired characteristics, Federley against. Federley was an excellent cytogeneticist and Weidenreich was an excellent paleontologist, but these people could not get together to make this meeting a really conciliatory synthetic one. Added to this divergence I would like to point out that the geneticists were accused by the paleontologists of dealing only with superficial traits which had no relation to the essential organic nature of organisms and of evolution. Thus the theory of natural selection was attacked both from the narrow point of view of paleontologists and from the broader point of view of biologists in general who thought that evolution was too complex a process to be easily accounted for by "mechanistic" interpretations"(see Zeitschr. ind. Abst. Vererb., 54 (1930): 20-50).

"While Weidenreich had recounted the [point] that the real genetic contribution showed that there was an inheritance of acquired characteristics, Weidenreich shared the opinion of a good many other older biologists of the period according to which genetics played only a very inferior role in evolution. He was convinced that genetics explained only the most superficial aspects of evolution and that actual evolutionary phenomena had to be based on very much more fundamental considerations. It was, in other words, not just a lack of communication between evolutionists and other types of biologists."

"The lack of communication was probably not too serious. The geneticists, the paleontologists, the systematists were basically familiar with the genetic facts which had a bearing on evolution but in spite of this familiarity they rejected the application to actual cases."

Stern feels that the eventual progress in understanding "was made primarily through a clarification of concepts, such as mutation, somatic, germinal, etc." rather than by new discoveries.

The opponents of Darwinism found it particularly difficult to accept an explanation of adaptation.

"The complexity of adaptations impressed biologists rightly and they found it difficult to give natural selection a role in which this role implied the origin of the most complicated and unexpected types of adaptations. I myself in my teaching discussed at length the parable of the monkey at the typewriter who on a completely chance, random basis composed the whole library of books. This in itself would appear as a very improbable way of getting a library, but the additional thing which had to be considered was not only the random typing of the monkey but also the presence next to him of a censor who with each letter typed looked at it and decided whether this made sense, whether the letter for instance completed a word which was not a unit yet, and eliminated all those types at every step which did not fit in to the general picture of adaptations."

Concerning his contact with population genetics, Stern wrote me:

"I learned first about the Russian population genetics of the Chetverikov school during the Berlin Congress in 1927. At that Congress Chetverikov gave a paper about finding mutants in heterozygous recessive condition in the Caucasus, thus proving that mutations were not just products of laboratory environments but occurred in nature just as well.

The full relation of these findings took some time to be realized. Timofeeff-Ressovsky who at that time worked in Germany, followed up Chetverikov's work by some studies of his own in population genetics, but by and large his evolutionary period began only after he had been for many years a radiation biologist interested in the effects of x-rays on chromosomes but not so much with the aspects of wild populations of *Drosophila* as he studied later."

On the subject of mutations and their evolutionary significance, Stern wrote me, as follows:

"I do not remember when I first came across the concept of mutation, but probably during my first semester (1920) listening to lectures of Max Hartmann. I read about de Vries first in Richard Goldschmidt's textbook of genetics, and in the textbook by Baur. Mutations were from the very beginning of the *Drosophila* work something which did not pose any problems for me. They were not necessarily large deviations from a standard type, but could be very fine in nature. Muller worked on mutations produced by temperature effects since 1920 and mutation was not anything which was not compatible with evolutionary expectations.

I am not sure that I ever fully gave up the idea that a mutation was nearly always a deleterious disturbance of the genotype. But I did recognize that there are alleles known which are very similar in effect and can only be shown by special methods to be really different, and called them isoalleles (1943).

The concept of the small, frequently invisible mutation was brought home to me particularly clearly in a study by Baur on the snapdragon where he investigated wild populations in Spain for the occurrence of variations of a very fine quantitative nature. It was a pity that Baur discontinued his experimental work when he organized the new Müncheberg Institute for Plant Breeding.

The frequency of mutations was measured for the first time to some degree by Muller who showed not only that they were rare events, but more importantly that they were events which repeated themselves in appearing, so in spite of their rareness they increased up to certain limits in populations. Muller's experiments with x-rays producing mutations were regarded as models of what really was going on in the germplasm."

Concerning his early views on variation Stern wrote me:

"I never thought that there were two kinds of variation, cytoplasmic and chromosomal; not, as was sometimes proposed, that chromosomal mutations were of a more fundamental evolutionary nature than so-called cytoplasmic mutations which represented continuous variation.

I first learned about the work of Schmidt, Sumner, and Goldschmidt when Goldschmidt's monographs on geographic variation appeared. I remember that I was much relieved by reading Sturtevant's paper of 1918 on selection of modifiers. Here orthogenesis was explained in terms of

Darwinian selection and thereby removed from the somewhat mystical aspects to their reasonably better understood ones. Payne's work at the same time with Drosophila also contributed to the demonstration that different kinds of variations all obeyed the chromosomal genetic aspects and were not outside of them."

Concerning contemporary views on the role of the environment in evolution, he wrote:

"I do not know when I first realized that the environment acts solely as the agent of natural selection. I think I grew up in an atmosphere which supported this idea; it did not cause any revolutionary upset in my own mind.

Jollos in 1922 published an inaugural lecture at the University of Berlin entitled "Natural selection and the formation of species". This was not in itself a very influential publication but it showed the way some people thought along modern lines" (Selektionslehre und Artbildung).

Natural Selection

"I think I was a total selectionist from my student days on but aware of the disagreement of others who could not conceive selection to be able to create some of the morphological adaptations, for instance the vertebrate eye. In 1929-1930 I had a literary feud with Weidenreich (Natur und Museum 1929-1930), who regarded Muller's x-ray mutations as evidence for the inheritance of acquired characters. I "certified" that Weidenreich had not been able to understand genetics and he referred to the Drosophila mutants as "kinkerlitzchen". Fisher's book (1930) had no influence in Dahlem.

Little interaction existed between Berlin-Buch and Dahlem for I think primarily external reasons. The distance from Dahlem to Buch was too large to permit getting together more than once in a year or so. Also, as I pointed out earlier, Timofeeff-Ressovsky's work was then primarily radiation genetics in nature and not on evolutionary aspects."

The Nature of Evolution

"In the 1930's I saw evolution as a strictly random process, not containing a trend toward progress and improvement or in some other manner. And yet it required some aspects which formed, so to say, a bonus over what was accomplished by strict random processes. In other words random processes led to a variety of genetic constitutions and some of these, only a very small minority, were of such a nature as to bring with it an improvement of the situation. To some degree this was part of Sturtevant's article on orthogenesis published in Science, 1924".

The species problem

"My own work on fertility and sterility in Drosophila with different Y chromosomal segments (1936) was stimulated by Bateson's famous sentence (Toronto 1922): "The production of an indubitably sterile hybrid from completely fertile parents which had arisen under critical observation from a single common origin is the event for which we wait". Dobzhansky's Genetics and the Origin of Species (1937) was an important source leading to my becoming more deeply interested in the species problem."

A comparison of Professor Stern's impressions with those of Rensch and Mayr brings out vividly the fact that two worlds existed side by side in Germany in the 1920's. That there was an urgent need for bridge building is obvious.