

Letters

Footnote to History

Since the meeting of the International Astronomical Union in Moscow last August I have been asked repeatedly why I had decided not to attend, or to accept an earlier, very generous invitation by President Nesmeyanov of the Russian Academy of Sciences to visit the Soviet Union at a time of my own choosing. I should like to explain my attitude to the many American astronomers who attended the meeting, and to the numerous Russian astronomers who had urged me to come.

First, I want to dispel any rumors that I mistrusted their assurance of "safe conduct," or any suspicions that there may have been "hidden activities" in my Russian past that could have made a trip to the Soviet Union dangerous for me. I have never doubted the sincerity of the Russian astronomers, and I had no fear of personal violence. Any fear that may have existed was of my own memories: of a family disrupted; of the cruel, bloody, and hopeless struggle of Russian against Russian during the civil war of 1918-1920. There were no "hidden activities," as my dossier in the files of the Soviet secret police must show. (I had knowledge long ago that such a dossier exists and that it is quite complete.)

In 1916 I enlisted in the Imperial Russian Army and served on the Turkish front until January 1918. After the revolution, in the spring of 1919, I enlisted as an officer in the White (anti-Communist) Army, and took part in all military engagements until the evacuation of the remnants of the White Army from Sevastopol in 1920. The only occasion I ever had, while in Russia, to give expression to my political views was in the summer of 1917, when I cast my vote for the Socialist-revolutionary party of Alexander Kerensky in the elections to the "constitutional assembly." Since 1927, when I became a naturalized citizen of the United States, my political attitude toward the Soviet Union has been that of an average American.

After the end of World War II, I shared the concern of many others about the fact that a stalemate had developed in the International Astronomical Union that prevented us from scheduling a meeting in the U.S.S.R. or in the United States. The Soviet Academy had invited the Union to meet in Pulkovo in 1952, and they later renewed their invitation for a meeting in 1955 in Moscow. Both invitations were declined by the executive committee (I concurred with these decisions), and the two meetings took place in Rome and Dublin, respectively. But it was apparent by 1952 that a great majority of the delegates wished to break the deadlock between the United States

and the Soviet Union and to schedule meetings in both countries. Therefore, in my speech of acceptance of the presidency of the Union in Rome (1952), I included the following sentences, addressed to V. A. Ambartsumian, then Soviet vice president of the International Union: "Take with you to Russia the assurance that this Union desires to meet in your country in the not too distant future. Our deliberations in the executive committee, and the vote taken earlier this morning, have shown that this cannot be done now."

Two years later, at an executive-committee meeting in Liège, I asked the Russian vice president whether the Soviet Academy would wish to renew its invitation, and gave assurances that the executive committee would recommend acceptance to the general assembly in Dublin. I felt that a meeting in Moscow would pave the way for a later meeting in the United States. The Russian invitation was formally extended in Dublin; the motion was seconded by J. J. Nassau, chairman of the United States delegation. A tentative invitation to meet in America in 1961 was extended by Nassau and Menzel and was seconded by Karkarkin of Moscow. The invitation of the United States of America was formally extended in Moscow last August and was accepted by acclamation. Thus, I not only endorsed the decision to hold the 1958 meeting in Moscow but encouraged it by all the means at my disposal.

But having done this as an American astronomer elected to the presidency of the Union (1952-1955), I still had to decide whether I, myself, would attend. (I was still technically a member of the executive committee.) I concluded that if my attendance would be in the best interest of the United States I would make the trip to Moscow, and I so informed the proper organizations. I received no encouragement; consequently I based my decision entirely upon my own preference.

When I enlisted in the White Army in 1919 and fought against the Red regime, I was one of hundreds of thousands of young men on both sides who were motivated wholly by patriotic impulses, and I regard my enlistment as the most self-sacrificing act of my life. I have no doubt that the time will come, though it may not be in my life-time, when the Russian people will recognize that patriotism was not the exclusive privilege of those who fought on the winning side. (An American would not now doubt the high ideals of General Lee and his soldiers.)

About two years ago I received from the Soviet Union a book by A. I. Slashtenov entitled *Astronomy in the University of Kharkov, 1805 to 1955*. I had received my undergraduate education at Kharkov, where my father served as professor and director of the observatory for

25 years. I found the book interesting and devoid of any *great* amount of political propaganda. But the Library of Congress has another, earlier version of the book in which several pages are devoted to my own so-called "traitorous" activities, and in which I am described as "having been for a long time in the service of American imperialists as the director of the Yerkes Observatory, near Chicago." The account continues with a reference to Theodore Dreiser's novel *The Titan* which had "torn the mask off the American capitalists and had clearly and convincingly demonstrated the predatory character . . . of the capitalist Cowperwood [a fictional name for Yerkes]." I might be amused that my work at the Yerkes Observatory from 1921 to 1950 should be thus linked with Dreiser's literary opus, but I do not consider it amusing that I was called a traitor, nor that in my particular copy of the book the relevant pages had been replaced by newly printed pages of quite innocuous content which do not mention me at all.

I have been assured by several of the Russian astronomers that few copies of the original version were placed in circulation, and I am grateful for, and flattered by, their efforts to have the offending statements "expurgated." But I cannot avoid the impression that an atmosphere of hate persists, at least at my own alma mater, and that not only I but also quite possibly my Soviet colleagues could have been painfully embarrassed by my attendance at the Moscow meeting.

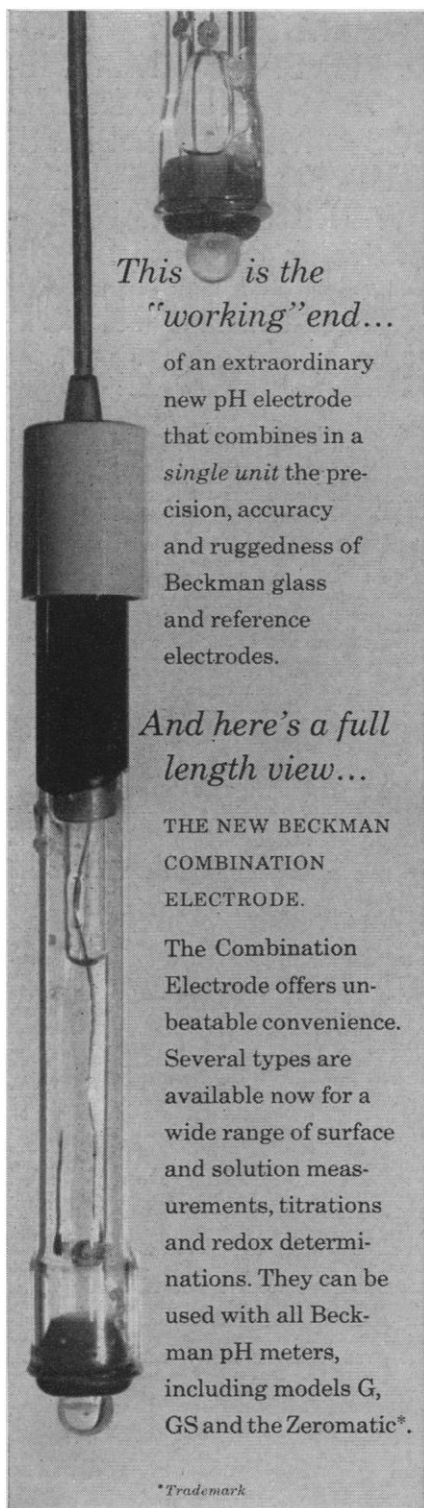
OTTO STRUVE

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Government of Portugal

In his survey article entitled "Basic Research in Europe" [*Science* 128, 227 (1958)], David M. Gates refers to the present government of Portugal as a "benevolent dictatorship under a constitutional monarchy." However, may I point out that Portugal has been a republic since 1910, when the last Portuguese king was forced to flee the country. The parliamentary and multiparty type of democracy set up in that year was supplanted in 1926 by a military regime, which paved the way for Salazar's rise to power in the early 1930's. The current fascist-type dictatorship of Salazar, while based on a constitution adopted in 1933, has never been tied to any monarchy.

In the same article (p. 231, column 1), we are told that "there are nine universities and technical high schools in the Netherlands." The term *high school*, in this context, is obviously a mistranslation of the Dutch *hogeschool*, or German *Hochschule*, meaning an institution of higher learning, a university (roughly corresponding to our senior-undergradu-



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ate plus graduate-or-professional type of school). In the case of technical schools, the best translation would be "institute (of technology)." On the other hand, where we speak of a "high school," Europeans usually speak of a "middle" or "secondary" type of school—never "high."

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I wish to thank Leo Pap and others who have written correcting my misimpression concerning the government of Portugal.

With regard to his second point, I think it is worth while to point out that the connotation of the term *Hochschule* in German is not the literal translation to "high school." However, I assumed that most scientists realized that the use of this term in European science did imply a school of higher learning on the level of a university or college.

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Acetylcholine Metabolism and Behavior of Rats

Before Chow and John submitted their paper (1) for publication, they were kind enough to correspond with us at length about their findings. At that time we indicated to them our reasons for believing that their study did not afford an adequate test of our major hypothesis. In their published article no reference is made to the questions we raised, and since Chow and John interpret their data as contradictory to our major hypothesis, we would like to point out publicly why we believe that their experiment does not provide a test of our hypothesis.

In our original *Science* article (2) we suggested that a higher rate of cortical acetylcholine metabolism is related to a greater number of spatial responses in the Krech hypothesis apparatus. In our second *Science* article (3) it was made explicit that this referred only to the animal's initial problem-solving behavior. Pentobarbital sodium (which retards acetylcholine synthesis) was shown in that article to affect the animal's choices strongly if it was administered at the outset of maze experience; if it was given after four days of maze experience, the drug had little or no effect.

Chow and John gave their animals six days of maze experience. By that time the animals had adopted different response patterns. In subsequent testing Chow and John found that anticholinesterase drugs had little or no effect on the animals' choice behavior. They conclude, "The fact that such injections did not alter the hypotheses displayed by the animals in running a maze seems to indicate

that hypothesis behavior is not dependent on cortical levels of acetylcholine." Actually, our results and theirs seem to be similar where they can be compared: When animals have had prior maze experience, drugs that affect acetylcholine metabolism do not appear to affect behavior. We urged Chow and John to test the effects of injections on behavior at the outset of maze experience, the condition under which we did obtain drug effects. Unfortunately this has not been done, so no comparison can be made under this critical condition.

Quite aside from this major point, there are a number of additional features about the report of Chow and John that make it difficult to evaluate their results.

1) Their Table 1 indicates that on a random reward schedule animals of the S1 strain made predominantly spatial choices and animals of the S3 strain made predominantly visual choices. Such a large strain difference in behavior—in the same direction but far larger than we have ever obtained—would appear to provide striking corroboration of our hypothesis, since we have shown the two strains to differ significantly in cortical cholinesterase activity. The data of the table cannot, however, be taken at face value. Correspondence revealed that over half the S1 and S3 rats were *trained* to give spatial or visual responses. There is no indication in the table as to which animals were trained to give specific response patterns and which adopted such response patterns spontaneously. Therefore it is impossible to evaluate the apparent strain differences, and we would prefer not to interpret these findings as supporting our hypothesis.

2) It has been our experience that when animals are transferred from a schedule that rewards one type of choice to a random-reward schedule, they tend to give up the previously rewarded choice rather rapidly. This did not occur in the Chow and John experiment. Their "no injection" results were obtained on the fifth and sixth days of a random-reward schedule, but many previously rewarded response patterns still persisted. This testifies to the strength of the prior training and indicates further why it would have been difficult to find a drug effect. It is not stated whether the animals were retrained between successive drug experiments (as many as five 6-day sequences were given to an animal). If response patterns persisted through such a long period, it is indeed interesting, and raises additional questions about the meaning of their data.

3) The legend of their table indicated that the number of times a rat was tested "varied from 4 to 30." No explanation is given as to why some rats were discarded after only four tests whereas their data up to that point were retained. Because of this feature of the experimental design, the subjects are repre-