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LETTERS

Soviet and Post-Soviet Science

In their editorial "Science in Russia" (27 May, p. 1235), Colin Norman and Daniel E. Koshland Jr. state, "Some areas of Soviet science were crippled by ideological barriers such as Lysenkoism in genetics and anti-plate tectonics in geology. . . ." Richard A. Kerr, in his survey of Earth science "Contacts with the West bring cultural revolution" (27 May, p. 1277) continues this theme.

Plate tectonics, the ruling paradigm in the West since 1970, was dismissed by Vladimir Beloussov, head of the Department of Geology of the Soviet Academy of Sciences, right up until his death in 1990. Instead of plates moving horizontally, Beloussov theorized that continental crust was sinking and becoming ocean crust. And because he said it was so, plate tectonics got short shrift in the Soviet Union.

It is regrettable that in otherwise interesting and informative articles one finds what we consider to be mischaracterizations of the nature of Soviet geology and the teachings of one of its distinguished practitioners. We are aware of similar accounts of the history of plate tectonics, including Claude J. Allègre's *The Behavior of the Earth* (Harvard Univ. Press, Cambridge, MA, 1988) and R. M. Wood's *Dark Side of the Earth* (Allyn & Unwin, London, 1985). Both books have been criticized for their whiggish accounts of the plate tectonic "revolution" in Western science, portraying historical events as a story of glory and scientific progress while chastising various anointed villains, particularly Beloussov.

It was the privilege of one of us (V.S.F.) to participate extensively in Beloussov's classes at Moscow State University in the period 1955 through 1960, as well as in the classes of V. Ye. Khain and others in the Soviet Academy of Science. (Beloussov was never elected to full membership in the Soviet Academy, apparently because of actions by his enemies in the Academy.) It is true that Beloussov was stongly critical of the doctrines of continental drift and plate tectonics. However, this was not for reasons of pseudoscientific ideology, as in the case of the politically motivated Lysenko. Rather, Beloussov was critical of the failures of these theories to explain important geological evidence of vertical movements in continental interiors. Many of these vertical movements seemed to him better explained by the direct action of processes operating vertically from Earth's mantle. In the classes by Khain, on the other hand, the horizontal movements were emphasized and continental drift was advocated.

Although until recently isolated in personal contacts from their Western colleagues, geologists of the former Soviet Union have had a long tradition of free and open scientific inquiry. This is a tradition that has resulted in great success in the exploration for minerals and fuels. The Earth sciences in Russia display many differences from their counterparts in the West, including the employment of a large percentage of prominent women. Western scientists have a great deal to learn from their new Russian contacts, and the first lesson will be to get past the myths that close minds to free inquiry.

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Science's broad survey of the situation for science and scientists in the former Soviet Union is incisive and offers much insight into what is really going on in this huge part of the world. I would add that, while money is important, it is not enough.

Together with colleagues from the Swedish University of Agricultural Sciences, I have visited Russia and worked with scientists there a number of times not only in Moscow but also in Novosibirsk, St. Petersburg, and Barnaul. According to my experience, let me stress the following.

1) What Russian scientists and students need more than anything else today are encouragement and self-confidence. They must regain their creativity and activity and start to believe in themselves and their country. They must feel that scientists all over the world realize that the reason they are so behind in many domains is not owing to themselves but owing to the system they have been forced into for 70 years.

2). We must remember that most of the administrators in Russia are the same as before. They have only changed their ties from red to blue. Therefore, it is important to offer grants and research money on a personal level. Research money from the West should not feed the very heavy bureaucracy that still exists. Furthermore, grants should be given with a long-term horizon, even if initially limited to 3 to 5 years.

3) The best thing scientists from the West can do today is to visit their Russian colleagues, especially those far from Mos-

cow, work with them and their students, give lectures, plan with them, and show them that we respect and trust them as fellow scientists. Let them feel that they really belong to the world community of science with first-class membership. Plan and start scientific projects together concerning Russian development potential or problems; invite young students, whom you more or less have picked yourself, to your own lab and country; educate them in your own country within the framework of the project you have started together; and send them back with grants and facilities to continue and finish the project. This will give them a real incentive to go home again, which otherwise can be difficult.

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Revelle on Global Warming

Having been Roger Revelle's closest junior colleague during his final years, I feel it my duty to clarify that his actual views about global warming were not represented by his mistaken statements published in a 1991 *Cosmos* article (*1*) (ScienceScope, 3 June, p. 1391).

In 1992, I served on an editorial board charged with considering republication of the *Cosmos* article as a book chapter (*2*). I protested that it failed our editorial criteria because it was less than objective and inadequately referenced. Moreover, I saw little social benefit in publishing an article in which Revelle had so obviously committed a major blunder in the key scientific statement at the core of the article.

The *Cosmos* article predicts that the most likely warming in the next century would be "*well below the normal year-to-year variation* . . ." (emphasis added). In an earlier *Scientific American* article (*3*), Revelle recognized that the normal year-to-year variation in global average temperature has been only 0.2 degrees Celsius. I knew Revelle to believe in 1991 that the likely average global warming in the next century would be in the range of 2 to 3 degrees Celsius, with even greater warming at the higher latitudes. In fact, he had opened a 1990 address to the AAAS by saying there was a good chance that the world's average climate would become significantly warmer during the next century (*4*). This major discrepancy convinced me, and still does, that the *Cosmos* article did not represent Revelle's view and that a serious mistake went uncorrected.

My commentary should defer to the evidence. Documents and testimony produced through the lawsuit brought against me by S. Fred Singer will be preserved in the archives of the Scripps Institution of Oceanography. Included are original drafts, related articles (*5*), the galley proof version bearing Revelle's handwritten comments, a sworn affidavit of Revelle's personal secretary, and sworn testimony of Singer.

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5. S. F. Singer, *Environ. Sci. Technol.* **24**, 1138 (1990); W. H. Munk and E. Frieman, *Oceanography* **3**, 125 (1992); C. R. Hufbauer *et al.*, *Washington Post* (13 September 1992), p. C7; B. J. Hurley, Ed., *Global Environ. Change Rep.* **6**, 1 (14 January 1994).

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DOE Peer Review

The 20 May ScienceScope item "DOE peer review ruled illegal" (p. 1071) may have left the reader with the incorrect impression that the Department of Energy (DOE) Office of Basic Energy Sciences program is "not usually peer reviewed." That statement is not true. The review under discussion was an *additional* review. Although its substance was greatly flawed, the DOE General Counsel was only asked to examine the compliance of the methodology of this panel-type review with the Federal Advisory Committee Act. Wastefulness resulted because management wanted to review each and every individual project on top of the initial peer reviews, rather than to set an objective for the additional review and to end it after its accomplishment. The objective seemed to be multipronged and to vary from day to day. A sampling, if done properly, might have served some purpose. Although started in the Bush Administration, the additional review was continued well into the Clinton Administration, and was terminated after I left DOE—far short of having covered all the projects. That fact is a measure of its usefulness.

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Noblesse Oblige

John Ziman, in a Vignette (22 Apr., p. 603) quoted from *Prometheus Bound: Science in a Dynamic 'Steady State'* (Cambridge University Press, 1994), states, "Only a scien-