

taken place in the Soviet Union, the area of the world about which Mr. Hellie is presumably knowledgeable. Obviously, they have all escaped his attention.

No less shocking is Hellie's assertion that Wittfogel's irrigation hypothesis "has been definitely disproved." On the contrary, by the mid-1950s Wittfogel's theory of "hydraulic society" was being supported by empirical research in many parts of the world—by Anouar Abdul-Malek in Egypt, Joseph Buttinger in Vietnam, Pedro Carrasco in Tibet, George P. Murdock in Africa, Edwin O. Reischauer and John K. Fairbank in China, E. R. Leach in Ceylon, Marshall D. Sahlins in Melanesia, Robert F. Gray in Tanganyika, Milan Kalous in Benin, Richard S. McNeich in the Tehuacan region of Mexico, William T. Sanders and Barbara J. Price and many others in Mesoamerica. In fact, Sanders and Price note the development of a veritable "hydraulic school" based on Wittfogel's theories. Marvin Harris's concept of "Cultural Materialism" testifies to the inspiration and influence of the same. Moreover, the late Julian Steward, who contributed to this volume and who was largely responsible for introducing Wittfogel's ideas into American anthropology, even as Pedro Armillas did in Central and South American anthropology, continued to uphold the validity of the "irrigation thesis" in Tehuacan, Mexico, Peru, and Hohokam. This is detailed and fully documented in my book, *The Science of Society: Toward an Understanding of the Life and Work of Karl August Wittfogel* (The Hague and New York: Mouton, 1978).

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PROFESSOR HELLIE REPLIES:

Why is it that authors responding to reviews begin with a claim that this response is a unique event? Is it a literary tactic to heighten the outrage of the assumedly mindless reader? G. L. Ulmen's first imprint is dated 1978; how resolute is his "policy"?

Wittfogel's reputation as a "cold warrior" is well founded, based on experiences ranging from his appearances before the House Un-American Activities Committee and the McCarran Internal Security Subcommittee to Ulmen's own statement that his teacher wanted to show in his works "the role [the 'Asiatic restoration'] had played and was playing in Soviet ideology and policy" (G. L. Ulmen, *The Science of Society: Toward an Understanding of the Life and Work of Karl August Wittfogel* [The Hague and New York: Mouton, 1978]). Whether "cold warrior" is a term of approbation or condemnation has not been, to my knowledge, defined by lexicographers: the victors in the 1980 national elections probably applaud one; Morning-side Heights leftists do not.

Ulmen is incensed by the fifth line of the review, as was I when I saw it. Regrettably, he failed to perceive that the offending line is gibberish because of typographical errors. The confused nature of Wittfogel's biography may be gathered by comparing Ulmen's letter with his statement in *Science of Society*: "Dr. Wittfogel told the committee that he had joined the Communist Party in Germany in 1920, but rejected its program in 1933. He has since been actively anti-Communist" (p. 568).

Most readers fail to find Pulleyblank's review "crude"—in fact it was the simplistic crudity of Wittfogel's work that forced the Cambridge Sinologist to pen his words. Pulleyblank began his review by noting that most other reviews had been favorable, but he was not intimidated by the "democratic approach" to evaluating scholarship. One can only be surprised that Ulmen has such a criterion.

Ulmen quotes Spuler, an authority on the Mongols, not on Russia, as though his were a definitive judgment. I, however, agree with Edward L. Keenan's judgment: "There is not a shred of evidence of borrowing from the 'Golden Horde' in the realm of abstraction in general, including political abstraction. Algebra and Aristotle, like the mythology of imperial power, tyranny, and despotism, came to Moscow not from Urgench of Samarqand or Sarai, but from Europe" (Edward L. Keenan, "Studying the 'Tatar Influence': Notes for Methodology," paper delivered at Claremont, California, 1968). Metonymically, one may ask Ulmen, which one of Jochi's many sons was Agapetus?

Readers are advised to apply to Ulmen for his list of citations to Wittfogel in recent Soviet works on Russian history. Perhaps Wittfogel is a lively topic of discussion in the Soviet Union today, but émigrés I have met say "I had never heard of Wittfogel until after I left the USSR." Perhaps Ulmen is confusing the USSR with East Germany, where Wittfogel's Communist writings are reportedly popular among the young.

My review was confined to the USSR, but Ulmen, who purportedly "in Marx, Weber and Wittfogel establishes the foundation and structure of a universal science of society" (advertisement for *Science of Society*), insists on the cosmic significance of his hero's dated work. In the Festschrift itself the late Julian H. Steward, discussing the "doubt" about the "irrigation hypothesis," wrote: "Inadequacies of the irrigation hypothesis leave us with the challenge of finding alternative hypotheses to explain the growth of early states" (p. 4). Terming the hydraulic thesis "absurd," China specialist Ho Ping-Ti notes that Chinese civilization began with dry-land farming, that the Chinese did not know irrigation until the sixth century B.C. Robert McC. Adams, writing on Mesopotamia and pre-Hispanic Mexico and often cited in the Festschrift, observed that "detailed scrutiny of the historical development of a number of regions crucial to [Wittfogel's] grandiose theory tends not to confirm either the specific role of irrigation management in the growth of governmental controls or the existence of structural barriers to further development that Wittfogel adduces." It was not my intention to interrupt Ulmen's canonization of Wittfogel, but I suspect that his pantheon is somewhat unbalanced.

TO THE EDITOR:

Paul Bushkovitch's review of my translation of George Vernadsky's *Russian Historiography: A History* (*Slavic Review*, 39, no. 3 [September 1980]: 487–89) bears the mark of a youthful zealot bent on overextending himself at all cost. In this case, youth loses to reason and rational analysis.

Vernadsky obviously does not need much defense. As the premier Russian historian of our time, his work will stand on its own merits. The book is criticized as being not one of his best, and myriad reasons and explanations are tried to uphold this judgment. This totally misses the point since the introduction states quite clearly that (a) Vernadsky did not complete the manuscript; (b) much represents notes and sketches that he was working on in conjunction with the broad topic of Russian historiography; and (c) the publisher thought it worthwhile to publish the work since a scholar of Vernadsky's stature has much to offer (even in uncompleted texts set to be his final work). A reviewer who spends a full page arguing against a book for what it does not purport to be is flailing about in a vacuum. A more mature scholar should have discussed the book on its own merits.

Mr. Bushkovitch's comment that this was one of the worst translations he had seen fits again into the zealot category. This judgment contrasts markedly with Richard Hellie's description of my translation of Fedotov's *St. Filipp: Metropolitan of Moscow* (*Slavic Review*, 39, no. 2 [June 1980]: 301–302) as "generally superb, highly literate." It is hard to believe the same translator is in question, and Fedotov is by far the more difficult to translate. Bushkovitch thinks the translation too literal. Evidently, he wants Vernadsky's quite sparse and matter-of-fact style to be embellished. Since Bushkovitch appears never to have translated, he would agree, I assume, to undertake such a task. He would then learn that in a work of 575 pages some errors invariably crop up. He would also learn that one does not make style the keynote in judging a work of this type. And finally, he might learn Russian better since the examples he chooses to cite are not convincing.

Mature scholars know the pitfalls and rewards of translating as well as its goals, and it would be hoped they be given reviews of this type.

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