

article being concerned with analysis of future location and transport flows. It is of doubtful value to invoke such results, which are presented with no supporting evidence, to "cast considerable doubt on Abouchar's conclusion about the high static efficiency of cement distribution in 1936." Nor should the Loginov-Astansky conclusion gain any greater credence by virtue of the fact that they are "there" while Abouchar is on this side of the ocean. Those in the Soviet Union familiar with much of Loginov's post-1960 work on the economics of the cement industry recognize many flaws in the analysis (e.g., his analysis of plant long-run average cost in the industry—details available on request).

The Loginov-Astansky assertion in question here, of course, cannot be evaluated, since too little information is given. I suspect, however, that most of the savings were due to two factors: (1) re-routing water shipments to rail, which would reduce the ton-mileage of the shipments affected by 40 to 60 percent, roughly; and (2) conversion of all cement into grade-400-equivalent tonnage, establishing grade-400-equivalent consumption requirements at the sinks, and allowing the program to substitute reduced tonnages on the long-distance routes, subject to the constraint of meeting the 400-equivalent target. That this was the procedure is suggested by the fact that there is very little change in the overall regional self-sufficiency as stated on page 15. This is also suggested by an earlier Loginov-Minz study (in *Primenenie matematiki pri razmeshchenii proizvoditel'nykh sil*, in which he followed a 400-equivalent approach, p. 106). This outcome, of course, is not directly comparable with my minimization in terms of physical tons. This is not to say that I did not consider in my study the question of product substitutability. I discussed this problem at some length on pages 85–88, actually going much further than Loginov, since I considered the economics of a single-grade approach, such as is more usual in the West, and calculated the production cost *and* transport cost savings therefrom (5 percent and 23 percent respectively).

I believe that any scholar owes it to his professional colleagues to look closely at the evidence that he invokes to criticize a work which he is asked to review. When this elementary responsibility is neglected by a scholar of Professor Granick's reputation, it is particularly lamentable.

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PROFESSOR GRANICK REPLIES:

Two issues are raised by Professor Abouchar's letter: (1) the substantive question of the degree to which Soviet postwar work on the cement industry casts doubt on a major conclusion in Abouchar's book, (2) the extent of the responsibility owed by a reviewer to his readers.

As to the first point, I bow to Abouchar's view that no substantive Soviet writing has been published in this field. His criticism of the quality of the work done may be quite sufficient to remove the doubts I raised regarding his conclusion as to the industry's high level of static efficiency of distribution in 1936. It does not, of course, answer my comment that he should have referred in his book to the work that has been published.

On the second point, I do not believe that it is reasonable to expect a book reviewer thoroughly to research the subject matter of the study on which he is

commenting. I have never done any work on the Soviet cement industry. By chance, I came across two references to such work. (It is not clear whether Ellman bases his comments entirely on the single article by Loginov and Astansky; this may well be so.) Although I agree that the Loginov-Astansky assertion cannot be evaluated on the basis of this article, I assumed that there was likely to be a further literature which elaborated the subject. I thus said that it raised doubts as to Abouchar's static-efficiency conclusion. I did not attempt to evaluate how justified such doubts were, nor could I have without extensive searching of the primary sources.

Although I agree that it would have been preferable if I had been in a position to evaluate the Soviet model-building of the 1960s in the cement industry, I think that such a requirement in the choice of a reviewer would result in the virtual elimination of the book review section of the *Slavic Review* and of all other journals. A reviewer's obligations in this respect must be less than those of the author of an article or book.

TO THE EDITOR:

Normally I would not take issue with a review of a book of mine, and, indeed, I hesitate to do so even in this instance. However, the review by Mr. Lucjan Blit of *Revolution and Tradition in People's Poland: Education and Socialization* (March 1974, pp. 160–61) is so full of factual errors, misstatements, and inaccuracies that I feel compelled to write. That Mr. Blit's perception of the book should differ from that of any other reviewer thus far is, of course, his prerogative; careless treatment of fact, on the other hand, is not.

The reviewer writes: "as the author admits, the statistics made available to him are very imprecise, or are completely silent on many important aspects of Polish education (p. xviii). Thus we are never given the hard facts about the earnings of teachers or a comparison of these with the salaries of other social groups (in the very last chapter, on page 318, the author just mentions the 'extremely low salaries of teachers, especially at the lower levels')." "

As to the first part of the above quotation, my introductory statement regarding the availability of statistical data and their quality was with reference to difficulties of doing research on Poland based upon aggregate data. It has absolutely nothing to do with the research reported in my book which is based upon surveys undertaken either by Polish sociologists or by myself in collaboration with Polish colleagues—as is made perfectly clear in the introduction and in the main text. Moreover, information on salaries is readily available and is accurate. As to the second part of Mr. Blit's statement—even a less than careful reading would prove that material conditions of teachers, including incomes, are discussed throughout the book beginning with page 99. In fact, an entire section is devoted to that problem, and a table (4–5 on p. 102) conveys the "relationship-gap" between prestige and income of Polish public school teachers, university professors, and a variety of other professions and occupations. Although incomes in specific *zloty* amounts are mentioned occasionally, this table, to be sure, avoids citing specific figures in *zlotys*, since they would mean very little to the Western reader unfamiliar with Polish economic conditions and the vagaries of Polish currency. On the other hand, specific *zloty* amounts are given for the performance of administrative chores (p. 96), as in the case of principals and secondary school directors. It so happens that the "very last

chapter” constitutes the conclusion—conclusions, one should add, based upon findings reported in the main text. Ironically, also, most reviewers have found the discussion of the material conditions of Polish teachers to be among the most satisfactory aspects of the volume.

Mr. Blit charges that the “Polish author most quoted” on the question of state-church relations “is Mr. M. Kozakiewicz, a leader of the insignificant Atheist Association” while, at the same time, “the impressive sociological works . . . by highly trained Catholic scholars” are ignored. Aside from the fact that Blit inaccurately reports the name of the Association of which Kozakiewicz is “a leader,” I have used Kozakiewicz’s work because (a) he is one of the most prolific Polish writers (as well as one of the most knowledgeable) on the teaching profession and on the sociology of education, and (b) he was one of my collaborators in the field, as is clearly stated in the book. As for “Catholic scholars,” allegedly never mentioned, reference is made to the research of Father Wileński and Konstancy Judenko, to mention just a few.

Mr. Blit supports my alleged assertion that because of the pro-church attitudes of younger teachers it is not easy to divide Poles already born into the “Marxist-atheistic state” (his expression!) into “‘Progressives’ . . . and stubborn ‘Traditionalists.’” This dichotomy (and these terms used within quotes, at that) is never once employed by me.

On an interpretative plane, Mr. Blit takes exception to my viewing the situation in Poland as a conflict “between revolution and tradition.” At least this criticism has some intellectual legitimacy, although it is open to debate. He asserts, “The Poles made no revolution after the war, but found themselves in a geopolitical situation which they were not strong enough to change.” How one goes about changing a “geopolitical situation” is beyond me, although the fact that socialism in Poland did not grow out naturally from its native soil, as it were, is indicated repeatedly in the book (as, for example, when discussing the various traditional personality types in conflict with the “ideal” socialist type, and so forth). However, the crux here is in our respective understanding of what a revolution is. Revolutions are not “made” in the way Blit’s statement would imply. Like Harold Lasswell I view revolution as a process involving changes in values with corresponding changes in symbols (including vocabulary), sources of leadership recruitment and leadership skills. In these terms, a sociopolitical, economic, and industrial-technological revolution did take place in Poland. The question is how radical, how far-reaching, and how deeply rooted is this revolution. My study indicates that the revolutionary changes are more apparent than real and that many traditional values and styles are still very much entrenched.

As stated, however, my quarrel is not with Blit’s point of view but rather with the inaccuracies with which his review is dotted, although I guess I should at least be grateful for his rather casual and left-handed compliment that I made an “effort not to become a propagandist of the proscribed line.” It is this kind of journalistic jargon in a respectable academic journal that hurts—in addition to the fact that I happen to have a great deal of respect for the author of the review. Had he read the book with care, as befits a responsible reviewer, and had he written his piece accordingly, the misstatements, I am sure, would not have occurred and this letter would not have been necessary.

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