LETTERS

To the Editor:

Mr. Schram in his review of my book, *The Comintern and the Chinese Communists, 1928–1931* (December 1973 issue), declares that I have "chosen to regard virtually all those who have written previously on this period in the history of the Chinese Communist movement as either incompetent or dishonest" (p. 822). I call attention only to three aspects of the review.

(1) Schram fails to mention *even one* of the four theses of the book, which are: that Moscow formulated the strategy of revolutionary warfare which has heretofore been attributed to Mao; that during the entire period under study the Comintern consistently called upon the CCP leadership to execute this strategy; that the Li Li-san line was a deviation from this strategy; and that Li's line was essentially his endeavor to secure the party leadership and prevent power from passing into the hands of those who were carrying out Moscow's strategy in the countryside, notably Mao Tse-tung (pp. 225–26).

(2) Schram errs in stating a matter of evidence and proceeds to use his own error against the author. He states that I chose to date the Comintern letter as July 23, 1930, "though it was in fact drafted in April and May 1930." The suggestion is that the interpretation based on the letter is thereby invalidated. In fact, I declare in the book that the letter was *received* by the CCP leadership in Shanghai on July 23, which is the date of the Chinese source (p. 168), not when it was *drafted*.

(3) Schram accuses me of what can only be termed an intentional misrepresentation of the evidence without in any serious manner attempting to support his charge. He claims that I "leave out everything" in the July 23 letter which would indicate that "Moscow expected the decisive confrontation in China to occur 'in the very near future,'" and "everything" in Li Li-san's June 11 letter "displaying the least realism." Not only does Schram fail to support this outrageous assertion, it is in fact false. Even a casual reading of my discussion of both directives reveals extensive quotation and comparison of the documents (pp. 154–57, 168–75, 220–21).

Do the above points indicate incompetence, dishonesty, or something else?

Richard C. Thornton
The George Washington University

Professor Schram replies:

Mr. Thornton's first two points can be summarily dismissed. The main thrust of his argument was clearly indicated, and all of what he calls his "four theses" adumbrated, in the second and third paragraphs of my review. As for the dating of the Comintern's resolution (not letter) of June 1930, the author does indeed say on page 168 that this document "arrived in China" on July 23, but on page 221 he reproduces the incorrect statement, contained in the Chinese text, that it was "passed" on that date. In any case, I did not "use" this fact against him; I merely pointed it out in passing as an example of sloppiness which might give rise to confusion.

Point 3 of Thornton's reply is of quite a different order of gravity. He challenges me to document my statement that he presents the two key resolutions, setting forth the Comintern's strategy and that of Li Li-san, in such a way as to produce a black-and-white contrast amounting to a caricature. Here are a few instances, drawn from his discussion of the Comintern resolution: (1) On page 168 he quotes paragraph 4 of the document as saying that the revolutionary situation, even if it cannot embrace the entire territory of China, "may cover several important provinces." In fact, the text says "*will* cover several important provinces . . . *in the very near future*." (Paragraph 19 also refers to "decisive battles in the near future.") (2) On page 171 Thornton quotes at great length from the Comintern's warning against "leftism," but omits the final sentence of paragraph 20 (italicized in the Russian text), which states unequivocally that in the two-front struggle against left and right deviations, rightism represents the "main danger." (3) On pages 220–21, he quotes paragraph 12 (in fact paragraph 13) of the Comintern resolution as calling for the development of "political strikes." Actually, the call is for the preparation of "a *general* political strike *in all industrial centers or in a number of them*." This point obviously relates not only to the time-scale of the revolution but to the role of the cities and of the industrial proletariat, which Thornton chooses to regard as merely "diversionary" in the Comintern's view.

Li Li-san's leftism really was very extreme, and Thornton's account of his views is therefore less distorted than that of the Comintern resolution. Here I will merely mention that in summarizing (p. 155) paragraph 18 of the resolution of June 11, Thornton says that in Li's view "a revolutionary victory would lead to an immediate transition to the socialist revolution," but fails to mention that in this context the resolution talks about a "revolutionary government" carrying on the struggle from a limited territorial base, until final victory can be achieved in all of China.

I repeat here what I said in my original review: Mr. Thornton has made an important contribution, and much of what he says represents a needed corrective to previous errors in the opposite direction. His account is nevertheless so one-sided as to be often misleading. I did not accuse him, as he says, of "intentional misrepresentation"; I left open the possibility that he might be simply a prisoner in his own "labyrinth." His reply does, however, engender skepticism as to whether he has any wish to break out of the net of his preconceptions.

To the Editor:

I am writing to take exception to some of Professor Granick's remarks in his March 1974 review of my book. After commenting on the excellence of my study on the basis of internal evidence in the book, and recognizing that it is "careful and well argued throughout," Granick goes on to state that "the results of a Soviet study covering 1964 cast considerable doubt on Abouchar's conclusion. . . ." He refers here to a 1968 study by Loginov and Astansky ("esp. p. 15") and to Ellman's study, *Soviet Planning Today* (pp. 171–78). Ellman's study, which is excellent in many respects, is not an especially valuable treatment of the Soviet cement industry. It is not based on independent research but, rather, on the very same Loginov-Astansky article cited by Granick.

What are we left with, then? We are left with the one page cited in the Loginov-Astansky paper. In fact, the Loginov-Astansky study contains barely two paragraphs which assert that savings of 30 percent could have been achieved in 1964 by using the optimal linear program solution as compared with the actual solution. The authors give no indication how they reached their results, most of their 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.

Alan Abouchar
University of Toronto

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