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Planning Problems in the USSR: The Contribution of Mathematical Economics to Their Solution, 1960-1971. By MICHAEL ELLMAN.

New York: Cambridge University Press, 1973. Pp. xx+222. \$14.95.

Reading this book evokes a mixed reaction. Some of the chapters and sections are good, even excellent, but some strike me as way off the mark.

First of all, it is always a treacherous exercise to try and gain insight into a real social system via the scribblings of its technocrats; the temptation being great to make something profound out of the mundane. Does the rise of "urban economics" as an academic discipline in the United States and the demise of "economic development" reflect an inward turning of the powers that be in America? There would be some truth in such a characterization, but somehow it is not really the right way of posing the issue. The final two chapters of Ellman's book are replete with examples of overtheorizing about Soviet society on the basis of a few trends in mathematical economics.

The parts of Ellman's book which I think are best are the straight descriptions of the rise of mathematical economics, its use in the current Soviet economy, and the problem areas of current planning (introduction and chaps. 1-4). This is the finest survey I have seen of these topics. It would make very useful reading for any economic theorist wanting to acquaint himself with the Soviet planning milieu and the models applied to it. Chapter 4, with its detailed case studies of a large number of operational programming models, is especially valuable.

Unfortunately, in my opinion the book starts to slip downhill fast starting with chapter 5. Here Ellman begins to go out on a limb, evaluating such things as the meaning of scarcity prices and the role of optimal planning in the Soviet economy.

A keynote of chapter 5 is Ellman's criticism of the use of efficiency prices based on the fact that although Kantorovich "thinks that the shadow prices associated with the optimal plan enable the rationality of small changes in the production programme, resulting from changes in the conditions, to be assessed, without the need for recalculating the entire plan, . . . this idea is not valid in general, does not appear to have been applied in the U.S.S.R., and as far as I know has not been applied elsewhere." Now this is much, much too strongly stated an incorrect position to go unchallenged. One of the great achievements of modern economics, maybe even the greatest, is the demonstration that under certain conditions (namely, convexity) efficient plans will have associated with them shadow prices which can be used for decentralized decision making concerning newly proposed variants. This result is really the basis of cost-benefit analysis in any form. Strictly speaking, the relevant proposition is that if the proposed project is unprofitable it cannot improve the situation, whereas if it is profitable *and if* the production possibilities surface is locally flat (differentiable or a hyperplane) *and if* the proposed project is sufficiently small to remain on that local flat, the situation will be improved if the project is adopted. I think it fair enough to call this a very significant result, Ellman's "counterexamples" for large projects notwithstanding.

This fifth chapter is very disappointing because Kantorovich is being raked over the coals for all the little things he doesn't have exactly right in Ellman's opinion. For example, when there are n goods in a multiperiod model, there are naturally n own rates of return and "the" rate of return is at best a useful approximation. But what a useful approximation it is! Ellman's strong statement that Kantorovich's conceptions cannot be applied because all the assumptions are not valid would doom most of the work of Samuelson, Solow, and Malinvaud in this area. At least poor Kantorovich is in good company!

In building any model, there is no way to include everything. The key idea is to disregard what is unimportant to the central message the model builder is trying to get across. It is child's play to go back to any model and enumerate all the things it has left out. The real question is: Has it left out anything which is essential to the message it is trying to convey? Most of the last half of Ellman's book is just a list of everything under the sun which has been omitted (allocation models don't take account of growth, etc., etc.). I don't think any of this dims, say, the brilliant achievement of Kantorovich who at all times sees very clearly that some notion of shadow prices, the "terms on which alternatives are offered," is indispensable for making rational economic choices.

High-powered, highfalutin mathematical theory is rarely of direct use, in the West or East. It is the modest understanding by on-the-spot managers and engineers of the general way to analyze choices, and an understanding of opportunity costs, which ends up saving an economy dollars or rubles on a large scale.

In the final analysis, the success or failure of Soviet mathematical economists will come from how well it filters down to influencing ordinary economic decision making toward rationality. In this context the role of Kantorovich is especially great because he has been a leading propagandizer of the new ideas—in popular books, articles, and lectures for bureaucrats.

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Benefit-Cost and Policy Analysis, 1973. By ROBERT H. HAVEMAN et al.
Chicago: Aldine-Atherton, Inc., 1973. Pp. xix+524. \$20.00.

This 1973 Aldine Annual, like that of previous years, reproduces a representative sample of the more important (as judged by the seven joint editors) studies of public policy decision making, analyses, and evaluation techniques that were completed during 1972. Sixteen of the 22 articles are reprinted from journals, books, or published reports, with the remainder representing conference or working papers. The papers are fairly evenly divided between studies concerned with specific benefit-cost calculations and policy studies focusing on broader or more general topics. The studies in the 1973 Annual (and previous editions as well) often pertain to the most important national problems during the previous year.