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A LETTER TO ELLIS JOHNSON

John B Lathrop

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(Received April 4, 1960)

BY WAY of a few words tucked away in the last sentence of your paper^[1] as published in the JOURNAL, I gather that you didn't think much of the 1959 address^[2] of the retiring President of ORSA

The purpose of the speech was to provoke thought and discussion on what I consider to be a serious problem—the proper place and role of OR and its acceptance in industry. It was meant to be inflammatory, or even infuriating. Apparently it succeeded in your case, since your “some inferior form of engineering, semi-charlatanry or horseback estimating” are fairly strong words. But I certainly hoped that you and others would be constructive and say why you could not believe my thesis and offer some alternatives.

It is all very well to proclaim that OR is a science, that it should remain research, that it should concentrate on complex problems at higher organizational levels, that “industry must be willing to provide for prolonged education of individuals,” etc. And I agree that this development would be very desirable and ultimately of great benefit to industry and government. But I am afraid you overlook the criteria by which industry selects its research programs and judges their value. The opportunity to do scientific research on top management problems with industry paying the bill must be earned, not demanded. The question is how to earn it.

At the Pasadena meeting of the SOCIETY I chaired a session of very nice papers on production scheduling. They were on relatively low-level problems, of the type you would call “operational engineering.” To each author I addressed the question as to whether his results had been accepted and used by management. With one exception the answer was “No.” This is typical. Why should it be?

You speak of the flight of excellent operational researchers from the military groups. I know of quite a few who went to high-level staff positions in industry only to move on again after a short while—usually to research organizations. Why should this be?

You suggest that the JOURNAL should exclude papers of an operational engineering nature, yet point to Lanchester Prize winners as typifying excellent and superior operations research. But the nonmilitary examples—by EDIE, BRIGHAM, ALLAIS—featured the problem and the usefulness of the solution as much as the research. Were the toll gates at the George Washington Bridge or the number of clerks in a tool crib large-scale problems? Edie's title when he did his prize-winning work was Operations Standards Engineer. Brigham majored in Engineering Sciences. Allais' title was Chief Engineer, among other things. Perhaps operations engineering is not so bad after all, and more skill and acceptance for it is a necessary steppingstone to more comprehensive work.

I can certainly agree with you, though, that "This question of studying large and complex problems, which is costly, presents real difficulties to the practitioners of operational research "

REFERENCES

- 1 ELLIS A JOHNSON, "The Long-Range Future of Operational Research," *Opns Res* **8**, 1-23 (1960)
- 2 JOHN B LATHROP, "Operations Research Looks to Science," *Opns Res* **7**, 423-429 (1959)

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C. West Churchman

IN CONNECTION with my article, *Sampling and Persuasion*, which appeared in the March-April issue of OPERATIONS RESEARCH (vol 8, pp 254-259, 1960), I would like to add the acknowledgement that a good deal of the research work described there was accomplished with the aid of personnel from the Finance Department of the Chesapeake and Ohio Railroad, who fully appreciated the potentiality of sampling methods