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PROFESSOR KOOPMAN ON FALLACIES - A COMMENT

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P-870 BH

21 May 1956

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This comment followed Professor Koopman's address on "Fallacies in Operations Research" at the fourth annual meeting of the Operations Research Society of America, Washington, D. C., May 10, 1956.

It is being submitted for publication in <u>Operations Research</u>.

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I cannot stress enough how fundamentally I am in agreement with Professor Koopman and how much I hope that his address will lead to some reorientation of our thinking about OR and OR's future development.

Specifically, I agree that the most common fallacies of OR occur in connection with what Professor Koopman calls "choice of figures of merit" and "choice of measures of effort," and which I will refer to jointly as the choice of criteria, or "the criterion problem."

The extremely difficult problem of choosing appropriate criteria (i.e., tests of preferredness which balance figures of merit and measures of effort) is not only the central problem of OR; it is the problem which distinguishes OR from (dare I say?) other sciences. Operations researchers, by definition, seek to recommend policy, not merely to understand and predict. In Professor Koopman's words, which I cannot improve upon, there is more to OR than mathematics and the experimental sciences; there is a working version of the concept of value, with all its human and practical overtones.

Professor Koopman has pointed to a number of fallacies that arise in choosing criteria. Instead of adding examples, which it is tempting to do, let me ask: Why do these fallacies emerge so frequently in choosing criteria? And let me suggest as a proximate answer: Because we have no body of theory to guide us in choosing good criteria.

Why do we have no such body of theory? A sufficient explanation is that we haven't yet had time to develop one. OR is new. The development of good theory is typically painfully hard and painfully slow. What is disturbing to me about OR, however, is not the fact that we haven't a good body of theory for choosing criteria, but that we (I mean most practicing operations researchers) don't seem interested in developing one. Practicing operations researchers, whether physical scientists, engineers or mathematicians, with a few exceptions like Professor Koopman, shun the whole problem. They don't even think about it, let alone write about it.

Why do they shun it, when the choice of criteria is central to most OR problems, and indeed the distinguishing characteristic of OR? I suspect that the underlying reason is yet another fallacy – a variant of the fallacy which philosophers call "scientism" – a conviction in this case that philosophizing about criteria, which involves value judgments, is "not science" and therefore not quite respectable. We can't talk about it because it isn't science, even though as long as it remains "not science" OR can't be science either. Consequently our journals tend to be filled with two kinds of articles:

1. Articles on mathematical, statistical and computing techniques – on linear programming, game theory, queuing theory, Monte Carlo, etc. – eminently respectable, in many cases interesting, for some problems very useful, but all techniques of other disciplines and all strictly peripheral to OR.
2. Case studies of actual OR, in most of which the figures of merit and measures of effort chosen are reported with the same casualness with which they were obviously selected in the first place – a casualness which betrays the contempt of the operations researcher for making this choice at all.

How do operations researchers in fact set about choosing figures of merit and measures of effort – choices to which the results of their analysis are far more sensitive, in my experience, than the choice of mathematical model, to which inordinate study and debate are normally devoted?* In some cases, the only and complete answer seems to be – casually. The operations researcher takes the first obvious criterion which pops into his mind and dashes on to the less important but more congenial aspects of his job. In other cases, he falls back on one of Professor Koopman's procedural fallacies, either:

Authorititis – letting the customer choose the criterion, even though this involves letting the customer "ask the question" or "define the problem" – a thing no self-respecting scientist will let his customer do, for good reasons, when he is on his own familiar ground as scientist. Or

Mechanitis – putting his machines to work as a substitute for hard thinking. Because he lacks any rationale for choosing a good criterion, the operations researcher writes down all the figures of merit and measures of effort which occur to him, links them in various permutations, and lets the machines optimize for each in turn. Then either he bases his recommendations on some form of majority vote (this might be called the fallacy of misplaced democracy – all criteria are inherently equal), or

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*It is hard to find instances in which conclusions are not extremely sensitive to the choice of criteria. For a recent familiar study in which results are sensitive see F. V. Hurst, Jr., "Evaluating the Adequacy of Airport Parking Lots," Operations Research, Nov. 1955, and "Comments" by Stephen Waldron and Jacinto Steinhardt, Operations Research, Feb. 1956.

he combines the fallacy of authorititis with that of mechanitis, passing all results on to the customer, who, even more sublimely ignorant of the implications of what he is doing and now more confused than ever by a welter of seemingly equally plausible opposed solutions, makes the choice.

There is no excuse for this escapism on the part of the operations researcher from the core problem of his discipline. I repeat that we have no theory to guide us in choosing good criteria, in the sense of a well-rounded, systematic, widely accepted corpus of theory. But we have a lot of relevant theory, much of it good and widely accepted, which, for example, permits Professor Koopman to spot fallacies in choosing criteria. We don't know how to choose optimal criteria, but we know enough in many cases to distinguish the good from the very bad.

Indeed, there is more theory in this domain than Professor Koopman uses in his discussion. Let me, asking his apology and understanding, develop my themes by exploiting his examples.

First, consider the anti-submarine warfare example. It is clarifying and useful to distinguish the aggressive from the defensive use of anti-submarine warfare formations and the very different figures of merit appropriate to each. But it is hard to think of any practical ASW problem in which one is not interested to some degree in both the offensive and defensive utility of the formations. How do we choose figures of merit to optimize the formations? Certainly not by first optimizing for offense, then for defense, and then somehow combining the partial optima. This would be analogous to, and as bad as, taking the expected value of a function of two variables as the same function of the expected values of each variable. We proceed <u>either</u> by finding a criterion which includes both offensive and defensive utilities, reducing them to some common measure, or, if we can't find a way of doing that, by imaginatively seeking a near dominant solution by methods suggested by game theory. The latter alternative may well lead to the invention by the operations researcher of formations not in the original set to be compared.

I am sure that Professor Koopman wouldn't disagree. I am merely using his example to show that we can carry the central analysis of criteria beyond the point at which he left it. The operations researcher may be able to do better than trace a boundary of "efficient points" and let the customer choose his preferred point on the boundary.

Let us now backtrack and take a harder look at the suggested criterion of defensive utility. Is "the probability of preventing enemy submarines penetrating the screen" a good criterion? Maybe it is - for some highly constrained low-level problems. But to me it bears the familiar marks of casual selection. Probably one can minimize penetrations by keeping all one's shipping and formations in harbor. If that is ruled out as a side condition, the answer would appear to be extremely dense screens. Perhaps that is the right answer, but dense screens mean few screens, and with few screens can enough shipping be moved across the Atlantic? Maybe that solution could be constrained in some way too, but I think I have gone far enough to illustrate two points:

1. The criteria in a low-level problem must be consistent with high-level criteria* - in this case, enhancing the flow of

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*I am not saying that we must convert low-level OR problems into higher level ones and work only on those. On the contrary, the operations researcher must cut his problems down to workable size. The whole point of my article on sub-optimization (Operations Research, May 1953) is that he can do so and reach valid and useful conclusions by choosing with care criteria which are consistent with objectives at higher levels. He need not solve global problems in all their complexity in order to recognize their general features, a happy circumstance which makes useful OR possible.

war shipping, the basic reason for having ASW formations.

2. Beware of criteria which have to be repeatedly hedged by constraints to prevent them from giving absurd results.

Finally, according to Professor Koopman (and I agree) some of the most glaring fallacies in choosing criteria are caused by the tendency to use money as a measure of effort. I myself would guess that fallacies result even more frequently from failure to use money as a measure of effort where it is a more appropriate measure than any practical alternative. It is my impression that most scientists and engineers in OR are ill at ease with money measures, and prefer almost anything more physical or tangible - like man-hours, tons of steel, or aggregate time of waiting - no matter how inappropriate to the problem at hand.

But counting the number of errors on both sides of a golden mean is not the point. The point is that we have a lot of pretty good theory to guide us in when to use and when not to use money as a measure of effort. Money is not a perfect, but in many circumstances an adequate, common measure of economic resources. In some OR problems, we need such a measure, and money cost is appropriate and an order of magnitude superior to the next best alternative. In many other OR problems (including, I think, all the examples mentioned by Professor Koopman) money costs are simply irrelevant.

Let me develop this idea a little further. I think most of us suspect that the US economy is more efficient than the US military. This is likely to be so, among other reasons, because money is almost universally used in the economy to measure both effort and merit, whereas in the military no corresponding measure is available as a common denominator. Where money values are used to measure effort and merit, a weak but highly significant kind of optimizing process results in an economy, one with which Adam Smith was familiar, and operations researchers ought to be but generally aren't. By using money measures in military OR, we can simulate this optimizing process to a limited degree.

Don't misinterpret me. I am not arguing for the universal use of money measures in OR. They can be overused and misapplied. But there is a general case for using money measures of effort in certain circumstances; there is relevant theory which is helpful and which needs to be applied, developed and extended.*

When one reads much of what is written on criteria, utility, value theory, etc., one can understand and sympathize with OR editors and program chairmen who are unsympathetic to the subject. But let's face it. Our treatment of criteria is the weakest link in the OR chain, and we can't simply shorten the chain, cutting out this link, for it is central to every OR problem. OR itself can be no more "scientific," if that is our objective, than this link. This is where we need to develop theory. And the only road to good theory, we know from the history of every other science, is by way of bad theory. So we'd better get on with developing the theory we need, even though, for a time, it is likely to be bad as well as distasteful.

And in the meantime it would improve the OR product enormously if we would give as much attention to criteria as we do to models or computing techniques. At least we can stop choosing them casually.

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*See M. W. Hoag, "The Relevance of Costs in Operations Research," p. _____, this issue.