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Introduction

This paper assesses briefly what seems to be the state of affairs with respect to practical industrial usage of System Dynamics. This leads to a discussion of topics which, it is felt, should be examined as objectively as possible in the hope that steps can be agreed which will lead to greater practical application.

Background

The original literature on System Dynamics used the name 'Industrial Dynamics'; the first published applications were to industrial problems, and the subject was developed in a major business school. SD seems to have great potential in management analysis as it seems, at any rate to someone who has used both SD and the other techniques of management science, to have the following attributes:

1. It is comparatively easy to learn.
2. It requires no particular mathematical skill and can, therefore, be used by people who understand the problems.
3. It is very flexible in use, so that the problem does not have to be forced into a given mould.
4. It addresses very well the question of action to be taken as time passes and recognises that decisions made now may merely store up trouble for the future unless those decisions are harmonised to the structure of the system and the exogenous shocks it encounters.

Given those attributes, and the fact that practically 25 years have elapsed since the first publications of SD, which is about the same as the lifetime of Linear Programming, one would expect to see SD widely used. The reality seems to be that

1. SD is taught in perhaps 5% of Business Schools (significantly, Harvard seems to ignore it, for all practical purposes).
2. There is only one small consulting firm regularly using SD.
3. It is almost unknown for SD papers to appear in the leading management science journals.
4. The software for SD is not widespread.
5. Where industrial applications of SD do take place that seems to be often on a one-off basis, and rarely leads to continuing activity.

For all of these statements, exactly the opposite would be true for, say, linear programming.

Reasons?

The reasons for this are many and various, but they may include the following.

1. Perhaps SD is simply not as good as we think it is, and there are better and easier ways of achieving the same result.
2. Because SD is not taught, it is not used, and therefore it is not taught.
3. Many people who have never used SD, and some who have, think that it has been wildly over-sold.
4. To people who have not used it, the DYNAMO syntax is seen as clumsy and restrictive, and it is certainly true that DYNAMO itself lacks many facilities which would be useful.
5. Managers are not interested in the longer-term solutions which are produced by SD and merely want to know what to do next. Perhaps they are right in that view, and feel that the world changes so rapidly that longer-term solutions are invalid.
6. It is certainly the view of this author that practitioners of SD have done themselves a grave disservice by arguing that SD is a profession in its own right which is somehow separate from, and superior to, the rest of management science.

Hopeful Signs

It is, perhaps, no accident that the years from, say, 1953 to 1973 were a period of comparative stability in managerial affairs and that the main developments in applied management science were in LP (for optimising essentially stable systems), and in econometrics (for predicting essentially stationary stochastic processes). The increasing economic and social turbulence since the mid 1970's may increase managerial awareness of the need for analyses of controllability (though see item 5 in the list of reasons). On the other hand, there is increasing emphasis in the management science/econometrics literature, on papers which basically apply the mathematics of control theory, ignoring the work already done in SD.

The Papers in the Plenary Session

The papers have been selected to demonstrate developments in industrial application, which appear to be particularly important.

1. Rigorous descriptions of systems as they actually are rather than vague 'conceptualisation', whatever that means, of a system capable of producing a reference mode of behaviour.
2. Considerable advances in equation-writing practice to enable us to represent real systems as they are.

3. Demonstration that the validation process for SD models is done to a higher standard than is commonly believed.
4. Close attention to implementation, and the use of the model to test managers', rather than analysts', theories about the system.
5. Very heavy emphasis on the driven response of the system, i.e. the firm in relation to the rest of the world, rather than the firm seen as a closed process.

Development for the Future

It seems to the Chair that what is required for the future is some or all of the following:

1. A far more modest approach to the presentation of the results of SD analyses.
2. Fewer authoritative, or authoritarian, pronouncements on what SD is, and how it should be done.
3. The publication of more papers in the mainstream management science literature, so that SD can come to be regarded merely as part of the intellectual furniture.
4. Much more care in the testing and debugging of models, and more effort put in to modelling the real processes of systems, and far less into very broadly aggregated models.
5. Considerable development in the use of appropriate control theory algorithms, and their incorporation into the software. Suitable use of optimisation facilities.
6. The development of a unified, accessible, cheap, software package, rather than the present situation of two principal, and several subsidiary, packages, at prices ranging from $150 to $10,000.
7. Far more co-operation between different SD groups, and the eradication of the Not Invented Here disease, which seems to be epidemic among us.