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Frontiers of futures research: What's next?

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Abstract

This paper describes some important frontiers of futures research with the aim of identifying new opportunities for improving the value and utility of the field. These frontiers include the exploration and/or the reexamination of

(a) Potential for integrating new technology with futures research methods,
(b) Ways to reduce the domain of the unknowable,
(c) Ways to account for uncertainty in decision making,
(d) Strategies for planning and management of nonlinear systems operating in the chaotic regime,
(e) Ways to improve understanding of psychological factors that lead to irrational decisions
(f) Appropriate levels of aggregation in investigation of forecasting problems.
(g) The potential offered by new sources of social data.

Keywords: Futures research methodology; New technologies; Decision making; Uncertainty; Non-linear systems; Futures methodology issues

1. Introduction

There are many methods and approaches to the study of the future. While futures research methods are internally coherent and used systematically, there is no assurance that the evolution of such methods

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will lead to a more organized "science-like" field with a theoretical basis. Not only are there many diverse techniques for theorizing, observing, and interpreting the future directions and consequences of societal, economic, and technological change, but also the methodological approaches used in their analysis vary greatly. There are few attempts to aggregate futures data and build current work on proven prior work. The result, for better or worse, is that the field lacks the consistency and coherence that mark more scientific fields. Yet there are some methodological frontiers which, if addressed, may improve the quality of the enterprise [1].

2. Integrating new technology with futures research methods

New technologies carry great potential for improving and refining the conceptualization and application of futures research methods. For example, the Internet has made participatory approaches among geographically dispersed people practical. Just forty years ago, computers were not much of a factor in futures research. The Delphi method was accomplished with pencil and paper in 1963, and sent through mail. However, if the current trends continue, forty years from now nearly all futures methods will be conducted in software, through networks, with diverse and changing sets of people, continually cross-referencing data, and monitoring decisions. Hence, the image of a few bright people, using a few interesting methods to forecast the future, may be replaced by the image of many people interacting with many combinations of methods to shape the future by blurring the distinctions between research and decision making. Furthermore, new technologies such as wireless Internet, knowledge visualization software, and improved computer translation will allow more international foresight activities to build collective intelligence through participatory feedback systems far more complex than the current futures research methods.

The examples mentioned above represent only a small part of the immense potential of new technology in futures research. Imagine the potential of brain research in understanding decision making, the possible use of behavioral data from which values may be inferred, the use of large numbers of computer generated scenarios to optimize policy choices [2], and the creation of credible indices of progress across countries, companies and groups with common assumptions to measure progress.

3. Reducing the domain of the unknowable

It is hard to imagine the consequences of a new breakthrough before it occurs. Our answers to questions such as "what do you think might happen?" and "what do you want to happen" are limited sharply by what we believe is feasible, by what is taken to be "good or normal science", and by what has already been demonstrated or postulated. Some future developments of importance are currently unknown but discoverable. Others however are intrinsically unknowable. No matter the size of the model or the computer that runs it, some developments are beyond current discovery because the breakthrough that makes them feasible has not been demonstrated or even postulated yet. Some of these undiscoverable events may turn out to be the most important aspects of the future.

By definition, the geography of the unknowable is unknown. We could certainly speculate about such discontinuities (science fiction specializes in this domain) but, taking Kuhn's perspective, an idea before its time is apt to result in derision and dismissal [3].

So, given these circumstances, how can the domain of the unknowable be reduced? We would like to argue that plausibility is the key. However, just when then does an idea about the future move from wild speculation to plausible and worthy of consideration? And, how does a concept move from disrepute to respectability? How does vision of the future and perceptions of reality change? Or, more importantly, how do breakthroughs really happen and can they be anticipated, if not individually, at least categorically?

4. Decision making in uncertainty

Uncertainty, arising from new and unprecedented events, noise, chance, systemic changes and experimental and observational errors, can never be completely eliminated from the decision making process. Thus, we and others argue that instead of using forecasting methods to produce single-value deterministic images of the future, uncertainty and underlying assumptions should be made explicit. Yet, the tools for dealing with uncertainty, for evaluating the adequate return for risk-taking, are far from perfect and, outside of market beta theory, rarely used. Although some of the methods such as decision analysis, scenarios, portfolio theory, or decision trees have proven somewhat helpful in decision making in uncertainty, the field is still very primitive. The quantitative techniques available to us are not yet capable of quantifying risk in ways other than probability. Managers often do not know what risks are associated with particular strategies. A lot of work is needed here.

5. Planning in nonlinear, chaotic systems

While most physical and social systems are nonlinear, mathematical models and simulations of those systems usually use linear assumptions [4]. The linear approximations are made because linear equations are simpler to handle mathematically and over vast regions of operation the linear models provide a good match with reality. Linear systems can be stable (that is, when perturbed, the system settles to some stable value), can oscillate (that is, when perturbed, the system settles into a periodic cycle), or can be unstable (that is when perturbed, the system movements become very large and continually increase or decrease). When the systems are nonlinear, however, a fourth state of behavior can be triggered: chaos. In this state, the system appears to be operating in random fashion, generating what appears to be noise. In this state, the system behavior is still deterministic but essentially unpredictable.

The central premise of planning is that forecasting is possible. The policy sciences teach us to identify optimum policies by testing a set of prospective policies on models that simulate the real world and choosing the policy that brings the model outcome closest to the desired outcome. But if the model – and the real system – are in a chaotic state, the results of a policy may be exquisitely dependent on a number of factors other than the policy itself. In fact, quite different results might be obtained on successive runs of a model (or in two "plays" of reality) with the same policy, if the initial conditions used in the simulation (or in the "second run" of reality) are only very slightly different.

While most work in the field of chaos has been in the physical sciences, most social systems are also nonlinear and can be driven to chaotic behavior. If a system that we attempt to control is

nonlinear (that is, input and output are not related in a one-to-one fashion) and, through excessive feedback or "gain," is exhibiting chaotic behavior (that is, its behavior resembles random motion or noise), then prediction of the future of the system (interest rates, day-to-day swings in the market averages) is essentially impossible in most circumstances and, therefore, predicting the outcome of contemplated policies is equally impossible. In addition, historical precedent fails for systems that are operating in the chaotic mode. Since chaotic systems are very sensitive to initial conditions, history is no guide since conditions in the past were almost certainly different than the present.

Do these arguments lead to the conclusion that modeling and policy research are dead? We think not, but a whole new set of approaches to planning and systems management need to be invented. When chaos is possible, it is no longer adequate to say "choose a policy that brings the expected future close to the desired future." In chaos, the expected future is a chimera, and disorder can mask valid normative visions.

What might be some of these new strategies for management of chaotic systems? Here are some thoughts:

First, analysts should recognize that random appearing data and bizarre behavior may not be what they seem.

Second, nonlinear models can be built to simulate real life systems that operate in a stable mode most of the time. Such models can be used to find conditions that drive the systems they simulate into oscillatory or chaotic states. Then, using the model, policies can be found that move the system back toward stability. One of the authors (Gordon) found that slowing down the feedback tends to stabilize social systems exhibiting chaotic behavior. So the old advice "sleep on it" may have some validity after all.

Third, the nature of modeling changes. In the old days validity was tested by building models with data through some date in the past and then using the model to "forecast" the interval to the present. If there was a match, the model could be believed and used in forecasting. Now we see that if the system was in a chaotic state, it could be almost exactly correct in its match to reality and yet replication of history would be an impossibly stringent criterion. Nevertheless, such models are useful because they can point the way toward stability, establish reasonable ranges of expected operation, show periodic tendencies, and, if an attractor can be identified, even "nudged" at the right instant to achieve damping in the chaotic regime.

Fourth, using such models, the analyst can identify the future limits of operation of a system and set plans to accommodate those limits, saying, in effect, "I don't know precisely where the system is going, but I do know its limits. I'll set plans that are effective at the limits."

Fifth, planners might use the attributes of a chaotic system (rapid response to very small impetus) to his or her benefit. Educating people in an organization as to the potentials for the organization can increase the likelihood that better decisions are made when the normal decision process is impossible. In chaos, things happen quickly.

The problem of planning and management of systems operating in the chaotic regime is a frontier of great importance to our field. It challenges old concepts and, with any paradigm shift, opens new opportunities of unprecedented magnitude.

6. Judgment heuristics

People often make irrational decisions. They do so for psychological reasons that are not completely clear. Judgment heuristics is a field that documents some of these irrationalities. One or two examples from Ref. [5] will suffice to make the point:

> Memorable events seem more likely than less memorable events. For example: which is more likely, suicide or murder? Most people say murder, apparently because it commands a higher visibility in the press and is, therefore, more memorable. But, in fact, the opposite is the case.
>
> We ignore probabilities in our decisions. In Tversky's example, Sam is a meek, retiring, helpful, tidy, soft-spoken person. Which occupation is he more likely to have, salesman or librarian? Most people say librarian, but there are about 100 times more salesmen than librarians. So given only the sparse amount of information in this example, salesman would have been a better bet.

Since futures research has as its primary *raison d'être* informing policymaking, a better understanding of the mechanics of decision making would be useful. This assumption moves us into the realm of psychology.

7. The assumption of reductionism

There is an implicit assumption in some methods of futures research that reducing a problem to its elements improves the forecasts produced by the method. We may have the feeling that by breaking down the problem into its elements we gain accuracy. The notion is appealing but unproven. Do we know the decision rules of the buyers and sellers with any more precision than the market as a whole? We validate such disaggregated models by comparing their output with the real world and adjusting the rules of behavior of the agents until there is a match. This same implicit assumption is made in many other applications.

There is a frontier here: since many forecasting problems can be investigated at various levels of aggregation, what levels are appropriate?

8. New sources of social data

As large scale data bases become available in the future it will be possible to perform cluster analyses and multi dimensional scaling to identify groups that exhibit similar behavior or have similar attributes. These data will also be a stimulant to the search for correlates: what kind of behavior, for example, leads to propensity to particular diseases. With increasing statistical sophistication, the analysis tools will be able to isolate causal relationships and social model building will get a needed boost. This marriage between epidemiology, statistics, and futures research will be important and powerful. It will give new salience to agent modeling since the implicit rules of behavior of ever smaller groups will be known with increasing accuracy. This marriage may bring agent modeling to a higher level of usefulness, pending of course, the limits imposed by the assumption of reductionism.

9. Conclusions

This paper has identified several frontiers and challenges that may give new vitality to futures research. Certainly as they – and other directions – are explored the field will gain new thinking and new approaches, expand its utility, promote innovation, and hopefully improve decisions which incorporate its findings. Thus, these frontiers will serve as important orientation in the elaboration of the second edition of Futures Research Methodology 2.1 (CD-ROM) to be published by American Council for the United Nations University early in 2005.

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