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ABSTRACT

This paper explains some of the problems with, and their importance to the application of, the Cross-Impact Matrix (CIM). The CIM is a research method designed to serve as a heuristic device to enhance a person's ability to think about the future and as an analytical device to be used by planners to help in actually forecasting future occurrences. The author makes no judgment about CIM's usefulness as a heuristic device; but he does fault it as an analytical methodology. He partitions the analytical problems into two categories: (1) theoretical -- including questions about underlying assumptions of the model, the meaning of inputs, the ability of experts to perceive accurately, and the ability of mathema[illegible] to handle the inputs; and (2) practical -- including inval[illegible] [illegible]ematical formulae and questionable use of simulation techn[illegible] Related documents are EA 004 239 and EA 004 241. (Author/RA)

EPRC Research Report RR-5

A CRITICAL LOOK AT THE CROSS IMPACT MATRIX METHOD

by

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ABSTRACT

I. In recent years the development of rational methods for studying the future has accelerated dramatically. The cross-impact matrix method (CIM) is one of many such methods, and in many ways the problems of CIM are typical problems for futures methodology. In this paper I take a close look at CIM, and what I see as the basic assumptions behind the method, to determine what role it should play in helping men to think about the future. I try to show why I think it should be used with the utmost caution as a decision-making tool. At the same time I argue that this does not preclude its potential value as an heuristic device.

II. I outline what I see as the major problems with the CIM computer program as developed by the Institute for the Future and used at the Educational Policy Research Center:

A. Problems having to do with the formula:

$$P_B' = P_B + \Delta P_B \quad P_B + (1 - P_B) S_{AB} \frac{t - t_A}{t}.$$

1. The time factor need not, and in some cases should not, be linear or decreasing.

2. If the initial probability P_B of event B is less than 0.5, the change in probability ΔP_B is the opposite of what it should be.

3. If the year of 50% probability (yfp = t_A) for event A is not between year 0 and year 2t (where t = time horizon), then the final probability P_B' may be greater than 1 or less than 0.

4. The quadratic nature of the formula places undue constraints on possible change in probability ΔP_B.

5. The yfp of event B ought to be a factor in calculation of the change in probability ΔP_B.

B. Problems having to do with the order in which events are assumed to occur.

1. Potential order of occurrence (ooo) of events should be taken into account, and it should be recognized that not all ooo's are equally probable.

2. I analyze how the present program handles ooo, and try to infer from this the basic assumptions upon which the method is based.

3. The monte-carlo nature of the program necessitates a large number of "games." I suggest some ways of systematically cutting this down by as much as a factor of 100.

4. I suggest some alternative ways of generating "better" ooo's.

C. Problems of specifying the input and how that input fits into the formula:

1. Some necessary assumptions require us to reconsider our definition of "impact" S_{AB}.

2. Some alternative definitions are suggested.

3. We also have to address the question of the meaning of "probability" as we treat it.

III. Having examined some of the epistemological problems with CIM, which must influence the way we employ the method, I briefly discuss the use of CIM as an heuristic device. I suggest a simple alternative procedure which is easier to understand and is less likely to produce results which might be misconstrued.

IV. The final chapter of the paper is an attempt to look at the contents and nature of the foregoing in the light of the whole area of futures methodology. I suggest that many problems with CIM arise generally among rational methods for studying the future. As examples of the kind of problems we should address, I discuss (1) simulation, (2) probability, and (3) judgments based on knowledge which is not made explicit. I suggest that the future of futures research might best lie in research on problem areas such as these, rather than in the development of new techniques which gloss over them, or ignore them completely; we might find that much of so-called "scientific" futures methodology is based upon scientifically indefensible foundations.

ACKNOWLEDGEMENTS

I wish to express my sincere thanks to the many persons who supported the completion of this paper. In particular: Many of the ideas in the paper are a result of suggestions made by Gerhard Kutsch and Stanley Moses, as well as other members of the Center's staff. Professor Robert Wolfson (Syracuse University), Lawrence Hudson (St. Lawrence University), Dr. Howard E. Johnson (Emerson Electric Company), and Dr. Murray Turroff (U.S. Office of Emergency Preparedness) provided valuable critical comments on the paper in draft form. Finally, I owe special thanks to my wife, Martha Liveright Folk (Syracuse University), for tireless substantive and editorial assistance in drafting the final version of the paper.

I

INTRODUCTION

"In the days of Kronos and when Zeus was newly king,"* he attempted to destroy mankind by giving us absolute knowledge of the future, for he knew that it would break man's spirit to know the circumstances of his death. But that black sheep of the Titans, Prometheus, had taken a liking to us mortals, and ever since we have had problems with seeing into the future:

Prometheus

> . . .I rescued men from shattering destruction that would have carried them into Hades' house. . .

Chorus

> Did you perhaps go further than you have told us?

Prometheus

> I caused mortals to cease foreseeing doom.

Chorus

> What cure did you provide them with against that sickness?

Prometheus

> I placed in them blind hopes.

Chorus

> That was a great gift you gave to men.

Prometheus

> Besides that I gave them fire.

Chorus

> And do creatures of a day now possess bright-faced fire?

Prometheus

> Yes, and from it they shall learn many crafts.*

These and other gifts of Prometheus transformed man to a reasoning being. Alas, whether we like it or not we had no choice those many years ago but to trade off our prophetic powers for survival and craftiness. Lacking

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*Aeschylus, Prometheus Bound (Translated by P. Vellacott. Penguin Books, Baltimore, 1961).

the power of prophecy, and being possessors of rationality, we have over the years invented many devices for predicting the future and in the last two decades, as faith in our ability to use the promethean gift of rationality has increased, the development of scientific methods for studying the future has accelerated dramatically.

A. Rational Forecasting Methods

A recent survey* cites no less than twenty-four such methods, including everything from econometric modelling techniques to game playing, currently being employed for generating every imaginable kind of information about the future. I suspect that a systematic study of these many different methods would identify a number of recurring concepts, among them: probability (personal and objective), plausibility, exogenous and endogenous variables (in a social/technological environment), the extension of past and present trends, interactions among future events, time of and order of occurrence of future events, and tacit (untellable) knowledge.

The question that has to be asked about these concepts, and it has to be asked every time somebody invents a new method which involves them, is how capable are we of dealing with them rigorously? Do we, for example, have a keen enough understanding of and agreement on the concept of personal probability to make it a working parameter in a precise research method? Or do we regard personal probability as extremely variable and the method therefore as extremely imprecise?

B. Motivation for the Paper

The cross-impact matrix method is one which uses many of these concepts; it is typically rational and typically systematic, and typically it relies heavily on a precise command of the concepts. The cross-impact matrix method (CIM) was developed by T. J. Gordon and O. Helmer about four years ago as a tool for describing and analysing interactions among a set of possible future events.

*John McHale, "A Survey of Futures Research in the U.S.," The Futurist, IV, 5 (December 1970), p. 201.

Most of the original work on the development of CIM was done by the Institute for the Future, and I recognize the valuable research still being conducted there in developing and refining the method.*

I am concerned, however, about what I see as a developing over-optimism about the usefulness of the method as an analytic tool for decision-making. In our haste to find quick solutions to the problems of forecasting, we may be misinterpreting the cross-impact output as hard and reliable data, when in fact there may be no basis for doing so. We should first ask just what the CIM can do, what its shortcomings are, and what contributions it can make to futures methodology.

In this paper I can only ask a few of the many questions that should be asked of CIM, and these will deal mainly with problems that have occurred to us in the two years we have been experimenting with the method at the Educational Policy Research Center (EPRC) at Syracuse. I feel that this kind of critique represents the sort of analysis we should make of many new futures research techniques before we decide how or whether they may be useful.

I hope that the paper does not have too negative a tone, for there is at least heuristic** value in playing with quantifiable models for studying the future. By developing and working with new techniques, as well as by criticizing those techniques which already have been developed, a contribution may be made toward the advancement of valid futures research methodologies.

I do object to the lack of stress placed by its developers on CIM's inconsistencies and flaws, and this is the motivation for this paper. To present CIM without emphasizing its drawbacks is in the long run to play on modern man's faith in the omniscience of the black box. The formulae upon

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*See Appendix A for a summary of work done at IFF to April, 1970, reported in the Use of Cross-Impact Matrices for Forecasting and Planning, by Rochberg, et. al.

**By "heuristic" in this paper we mean "stimulating greater insight into a situation."

which the model is based are not so complex that the layman cannot be made to understand them; it is therefore essential, since the model is so imperfect, that the formulae be explained to the user. This is especially important if there is a possibility that the user will, in his turn, educate others in this supposedly useful methodology. Perhaps this paper will clear the air so that we can begin to make some positive contributions to futures methodologies through the use of the CIM.

C. Focus of the Paper

In the time that the cross-impact matrix (CIM) has been at EPRC many questions have arisen about the usefulness of the CIM methodology, both as an heuristic tool to be used in exercises designed to enhance one's ability to think about the future and as an analytic tool to be used by planners to help in actually forecasting future occurrences. This paper makes no judgment on the use of CIM as an heuristic tool; that the method of cross-impacting can provide a stimulating and possibly positive learning experience has been shown many times in exercises conducted by Larry Hudson and Stuart Sandow of the EPRC and by the original developers of the method from the IFF. In this sense, CIM has been used successfully to "improve communications and understanding of the complexities involved in the planning and decision-making process." *

However, in my opinion, CIM has not yet been used successfully as an analytic technique. In fact, I feel that the problems with CIM are so basic that it will not be usable as an analytic tool until they are solved. The purpose of this paper is to explain some of these problems and their importance to the successful application of CIM.

(Note: It is not inconsistent that a method might be useful heuristically and yet be invalid analytically: it is one thing to stimulate people to sharpen and expand their thinking about the future by introducing a responsive, oracular black box--however valid it may be--against which they must defend their perceptions, but it is quite another to build a black box which not only is responsive and oracular, but also provides a consistent and correct analysis of possible

* S. Enzer, A Case Study Using Forecasting as a Decision-making Aid, p. 3.

futures. When the method is to be used for purely analytic purposes, CIM is supposed to provide useful and meaningful information upon which real policy analysis can be based. The student of policy is not seeking stimulation, or indeed education in how properly to think about the future but rather to find concrete answers to complex questions about the future.)

I have found that the problems with the methodology which need to be changed or replaced before it can provide a useful tool for policy analysis can be partitioned into two categories:

(1) underlying assumptions about (a) the meaning of the inputs, (b) the ability of experts to perceive and to accurately describe their perceptions of the inputs, and (c) the ability of mathematics to handle the inputs;

(2) those problems concerned with the present model's inability to do those things that it says it does. These include, for example, invalid mathematical formulas as well as questionable use of simulation techniques.

Though I have thought more about (1), I have written primarily about (2), because such problems are easier to specify and, indeed, to deal with. I do try to indicate some major questions inherent in (1) in the last section, but I must defer deeper discussion of these subjects until a later time.

N.B. After the first draft of this paper was completed I became aware of the Institute for the Future's Report R-12 by Gordon, et. al. Their report addresses many of the problems discussed in this paper, but in my opinion most remain unresolved.

II

PROBLEMS IN THE CIM MODEL

The main text of the paper is written under the assumption that the reader is familiar with the cross-impact matrix methodology. Those who need to familiarize, or re-familiarize themselves with CIM, should read Chapter I of the IFF Report on "The Use of Cross-Impact Matrices for Forecasting and Planning"* and/or Larry Hudson's paper "Uses of the Cross-Impact Matrix in Exploring Alternatives for the Future"**. The version of CIM referred to in this paper as "the present version" is that which is explained in those two documents.

A. The Mathematical Model

In this section I discuss some aspects of the formula*** which seem to fail to reflect reality or which are inconsistent in some other way.

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*Reproduced in Appendix B.

**Lawrence Hudson, Uses of the Cross-Impact Matrix in Exploring Alternatives for the Future, EPRC Working Draft, December, 1969.

*** $P_B' = P_B + \Delta P_B = P_B + P_B(1 - P_B)S_{AB}\dfrac{t - t_A}{t}$, where:

P_B = "initial probability" (for event B); probability of event B occurring by time t (see below) without consideration of the occurrence or non-occurrence of event A.

S_{AB} = "impact" of event A on event B.

t = "time horizon"; time in future for which the probabilities are being estimated.

t_A = year of 50% probability (yfp) of event A; i.e., the first year by which it is thought that the event has equal chance of occurring or not occurring.

P_B' = "adjusted probability" (for event B), assuming the occurrence of event A.

Note: $\Delta P_B = P_B' - P_B = P_B(1-P_B)S_{AB}\dfrac{t-t_A}{t}$.

1. The Time Factor Function.

The expression under discussion here is $\frac{t - t_A}{t}$, or <u>time factor</u>.

The present version of CIM seems to assume that the closer the yfp, t_A, of an event is to the time horizon t, the weaker will be its effect upon the other events (See Figure 1)*. If the yfp is year 0, the effect is complete (the

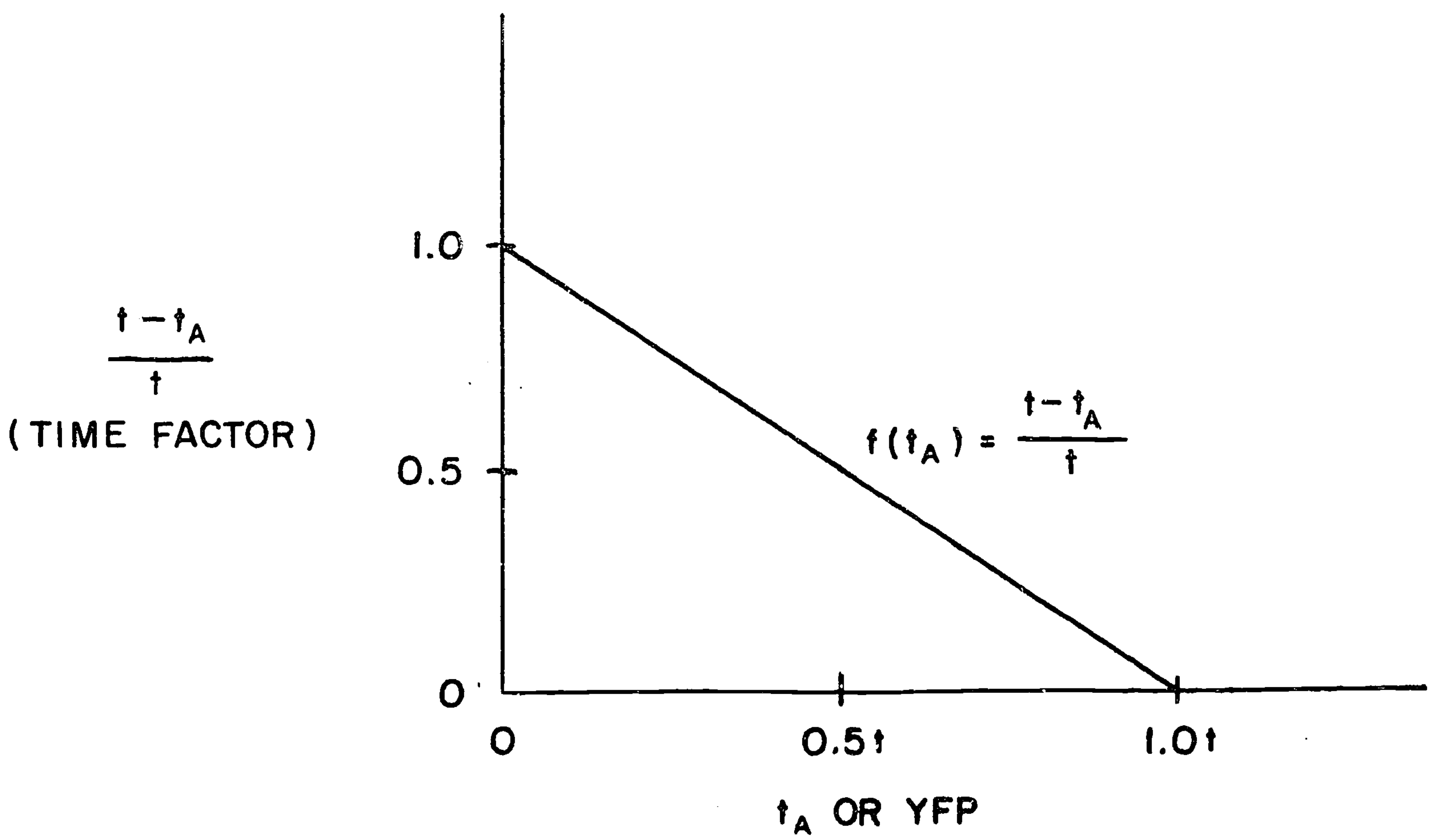

Figure 1. Time factor as a decreasing linear function of t_A.

time factor $\frac{t-t_A}{t} = \frac{t-0}{t} = 1$); if it is half way between year 0 and the time horizon, the effect is half of this ($\frac{t-t_A}{t} = \frac{t-(t/2)}{t} = 1/2$); if the yfp is

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*Since an event which occurs late in the time period has less time for its influence to spread, it is presumed to have less impact on other events.

the time horizon, the effect is zero $(\frac{t - t}{t} = 0)$. The assumption seems to be that in estimating the change in probability, we are assuming that the event has the full time between year zero and the time horizon to effect the other events, and that by naming yfp (t_A), we are saying that, on the average, the event will happen in year t_A instead of year 0, and therefore, the effect will need to be decreased somewhat, depending on how close t_A is to the time horizon.

In fact, however, it is possible that the dependence may not decrease with respect to time. For instance, event A might be so intimately connected with event B (B might be a highly desired goal whose attainment depends chiefly on the occurrence of A) that there is a high probability that B will occur immediately after A. It matters not whether A occurs close to year 0 or late in the time period; B will be impacted upon in the same way. In this case, the dependence of B on A is not decreasing but is essentially constant with respect to time. This sort of interaction might be represented by some function such as the one in figure 2a.

The time factor also assumes that the dependence between events is linear with respect to time. The question of whether the dependence should be linear has not been addressed. Linearity was chosen because it is simple and it makes some sense, but the following examples show that it might make no sense. For example, suppose there were (perhaps unstated) linkages between two events A and B (a) which took time to occur, (b) which would occur with high probability if A occurred, but (c) which would have to first occur before A could impact on B. In this case the effect on A on ΔP_B would be constant with respect to time, until that point in time after which the intervening events would not have time to occur before the time horizon, and hence A would not have time to influence B. The time factor function might look like the one in figure 2b. A similar example is the case where A's influence on B might be vitiated beyond a certain time due to some sort of "deadline" event, such as an election, whose occurrence was exactly predictable.

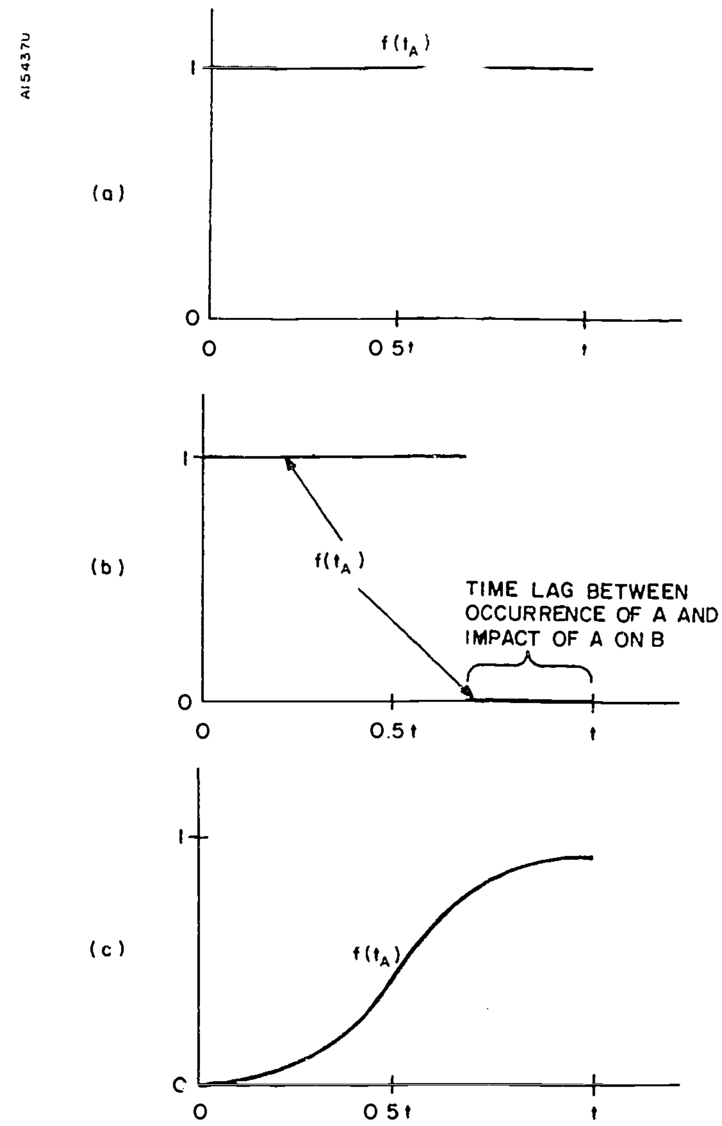

Figure 2. Alternate time factor functions when (a) there is no time lag between occurrence of A and its impact on B, (b) there is a definite time lag, and (c) impact of A on B increases with time.

As a final example, it is conceivable that the impact of A on B might even increase as the time of occurrence of A gets later, due to changing circumstances surrounding the events. For instance, if current studies on "total mastery learning" had occurred ten years ago, when the "spiral curriculum" was just coming into vogue and there was little thought about individualized instruction, these studies would not have had nearly the impact they promise to have today. The time factor function might look like the one in figure 2c.

Since, as the above examples show, it is not always the case that any given linear function, and in particular a decreasing one, represents the dependence of ΔP_B on time, it seems useful to consider the times of occurrence of events only if one looks at the changing probability as a function of time, each individual case determining its own function. This means that you would have to describe such a function individually for every non-zero impact in the matrix.

2. Initial Probabilities Less Than 0.5.

Another problem with the present linear function occurs when the impacting event (event A) has initial probability less than .5, so that its yfp must fall beyond the time horizon, and the time factor becomes negative ($\frac{t-t_A}{t} < 0$ if $t_A > t$). This causes ΔP_B to change sign, which is like saying that if A's occurrence "enhances" the probability of B (i.e., if the cell containing A's impact on B has a positive entry) and if A has initial probability less than .5, the actual occurrence of A will cause the probability of B to decrease.*

This paradoxical situation could be gotten around in at least 4 ways:

(1) set the time factor to zero whenever it is less than zero (Figure 3a);

(2) use another non-increasing function (necessarily nonlinear), which would approach zero only asymptotically, such as

$$f(t_A) = \begin{cases} e^{-at_A} & \text{if } t_A \geq 0 \\ 1 & \text{if } t_A \leq 0 \end{cases}$$ where a is a constant (Figure 3b);

---

*It could be argued that in some cases this is desired, but such cases probably imply a more complex set of interactions than CIM can handle.

(3) as discussed above, describe a function separately for every impact;

(4) don't consider the time dependence at all.

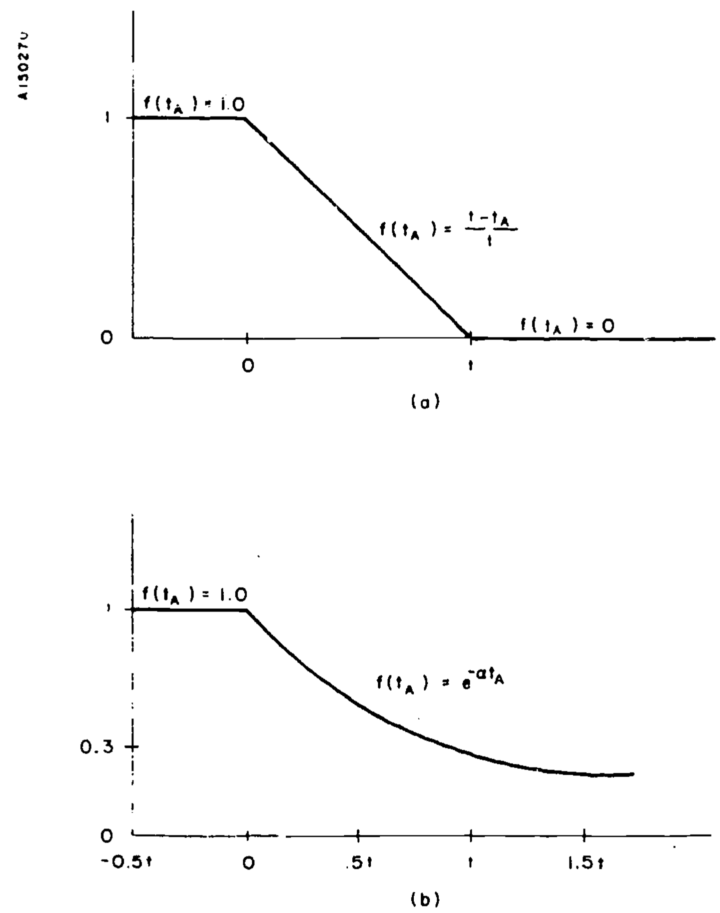

Figure 3a. Linear time factor function which does not change for $t_A < 0$, and $t_A > t$.

Figure 3b. Time factor function which approaches zero asymptotically as t_A increases, does not change for $t_A < 0$.

3. Year of 50% Probability (yfp) Less than 0 or Greater than 2t.

A further problem, related to the last one, occurs when the yfp for the impacting event (event A) occurs before time zero or after time 2t* (t is time horizon), assuming in the latter case that the problem mentioned above has been

*For example, suppose that year zero were 1970 and the time horizon were 1980 (i.e., t = 10). If a respondent felt that an event A had in all probability already occurred, then the yfp for A (t_A) would be less than 0. If he felt that A was not likely to occur before 1995, then the yfp for A would be greater than 2t.

solved. In either case, the time factor is greater than 1 in absolute value (Figure 4). This means that ΔP_B can be made as large as desired merely by adjusting the yfp of A. For example, let event A have yfp = t_A = −30, time horizon t = 10, and impact S_{AB} = .8 on B, whose initial probability is P_B = 0.9. If A occurs, the impact of A on B gives

$$P_B' = P_B + \frac{t-t_A}{t} S_{AB}(P_B)(1-P_B)$$

$$= .9 + \frac{10 + 30}{10} \times .8(.9)(.1)$$

$$= .9 + 4 \times (.8)(.09)$$

$$= .9 + .288$$

$$= 1.188$$

A15029U

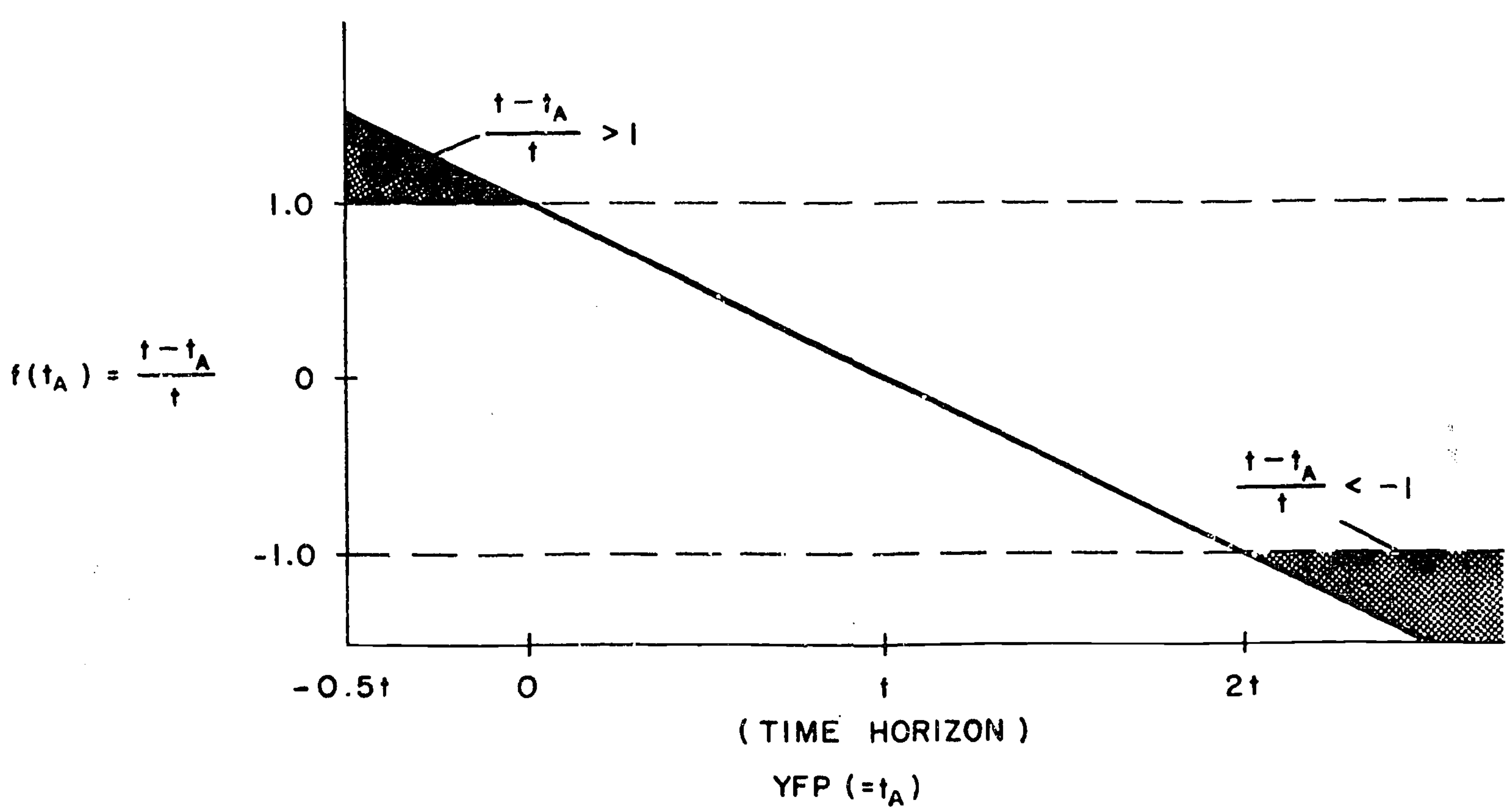

Figure 4. Time factor function. Function value falls out of the range −1 to +1 in shaded areas.

Hence, it is possible with the present program that if A and B are events such that A has non-zero impact on B, the adjusted probability for B may become greater than 1 or less than 0, depending only on the yfp for A. This has three interrelated disturbing aspects.

First: probabilities greater than 1 and less than 0 should not occur.* The fact that the given formula yields such probabilities might well lead one to doubt the reliability of the formula. Indeed, this situation is patched up in the present CIM program by resetting the adjusted probability to 1 whenever the formula yields an adjusted probability greater than 1, and to zero when the formula yields an adjusted probability less than zero. But why a formula which clearly yields unreasonable results at some places on the spectrum is considered reliable after alteration at those places (and without alteration elsewhere) remains to be justified.

Second: the set of situations in which the occurrence of an event B can be made certain or impossible by the occurrence of another event A is theoretically infinite in the program,** contrary to a reasonable expectation that such a set would be very small (if indeed it were not empty).

Third (and this is due to the linearity of the time factor function): the formula tells us that no matter how small the effect of event A on event B, if A occurs early enough then B's occurrence is forced, while if A occurs late enough, B's occurrence is made impossible.** Common sense tells us that this is unreasonable.

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*Allowing probabilities greater than 1 or less than zero is like saying an event is more likely than certain, or less likely than impossible.

**Depending only on t_A (= yfp of A), for $t_A < t - \frac{t}{P_B \cdot S_{AB}}$ yields the certainty of B's occurrence, and

$t_A > t + \frac{t}{S_{AB}(1 - P_B)}$ yields the impossibility of B's occurrence.

Again figures 3a and 3b offer possible solutions to these problems.

4. Constraints on Change in Probability (ΔP).

Ironically, the above aspect of the model gives the user a certain flexibility which, due to the quadratic nature of the impacting formulation, he would not otherwise have. Figure 5 shows that in a "normal" situation*

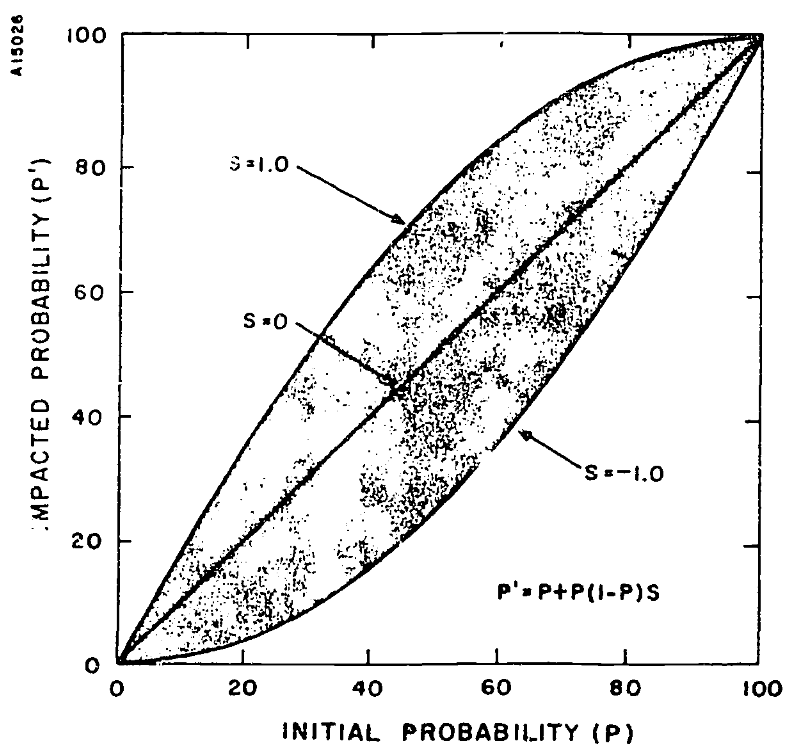


Figure 5. Effect of impact factors** (time factor assumed to be 1). Shaded area represents the amount the initial probability can change in "normal" circumstances. The three lines represent the maximum impacted probabilities for S = 1, 0, -1.

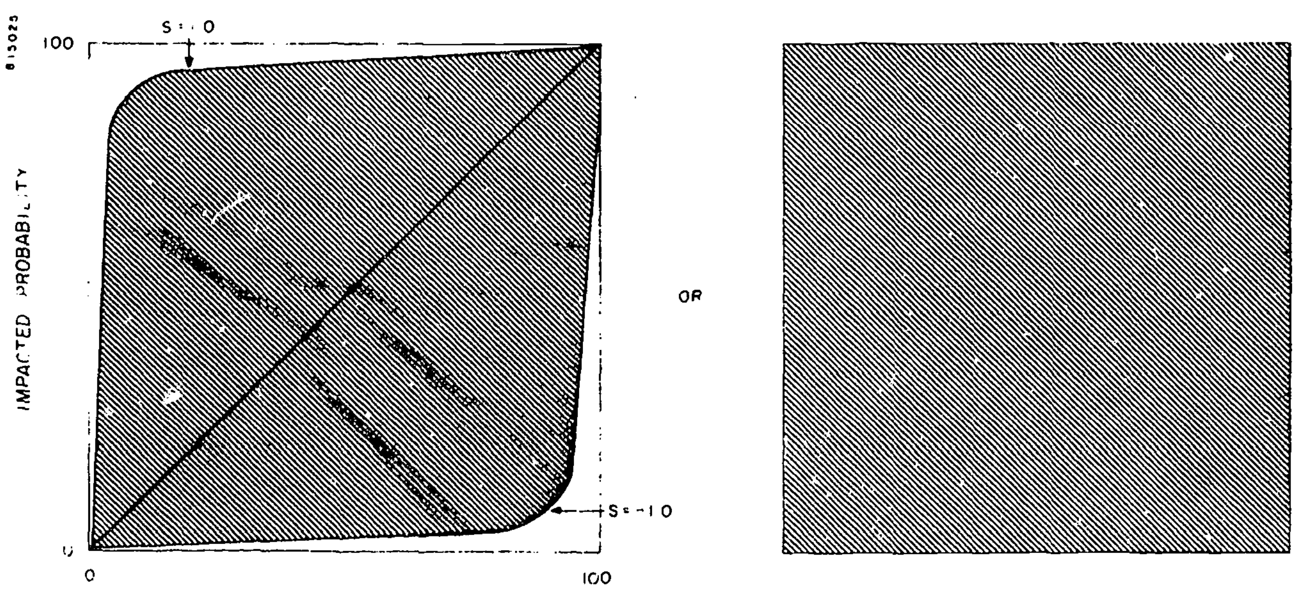
the amount of impact that one event can have on another is relatively small, a maxim ΔP*** of an event (due to the impact of one other event)

*By a "normal" situation I mean one in which the yfp of A is between 0 and 2t.

**Adapted from A Case Study Using Forecasting as a Decision-Making Aid, by S. Enzer. p. 24.

***As before, ΔP is that part of the formula which is added to or subtracted from the initial probability to get P'. Thus it is the "$P(1-P)S \frac{t-t_A}{t}$" part.

being .25, and this only when the initial probability is .5. This means that all the area above the S = +1.0 curve and below the S = -1.0 curve is unattainable in a single impact, under "normal" circumstances. It, therefore, means that necessary or sufficient conditions cannot be represented in the matrix.* I.e., if the occurrence of event A were sufficient, in the strict sense, to force the occurrence of event B, this could not be represented by a "normal" matrix. Of course, the restriction is much stronger than just not allowing necessity and sufficiency. In fact, in the present model the amount of change which is <u>not</u> possible under "normal" circumstances is greater than the amount of change that <u>is</u> possible (the unshaded area in Figure 5 is greater than the shaded area). What might be preferable would be for the upward limiting curve (corresponding to S = +1.0) to be made as close to the upper boundary as the user desires (see Figure 6), and similarly for the downward limiting curve.**



<u>Figure 6</u>. A more desirable range of effect of cross-impact factors.

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*The "priority level" parameter was introduced to take care of necessity, but it is hardly satisfactory as it stands since it requires either that all events, except the one that is necessary, depend on the necessary event, or that all events except the one which depends on the necessary event be necessary events. (See Hudson paper.)

**In S. Enzer, <u>Delphi and Cross-Impact Techniques</u>, a method is suggested for doing this by scrapping the quadratic formula and using "changes in likelihood." The computation procedure is not spelled out in the article.

The quirk in the model due to the time factor (last section) allows the user to force the total impact to be greater than the change shown by the limiting curves in Figure 5, simply by letting the pdo be either less than 0 or greater than 2t. For example, if the initial probability of event B is .5 and $S_{AB} = 1.0$, the occurrence of A could be made to raise the probability of B by .5 (to 1.0) by setting the pdo of A to -t.

I don't consider this a very good way of inducing a greater control over ΔP for several reasons:

(1) it perverts the meaning of yfp;

(2) all events are affected, so that one could not use it to induce a "strong" impact on one event without inducing a "strong" impact on all events;

(3) it further complicates a fairly simple idea;

(4) as stated above, it gives infinite domain to those impacts which force certainty or impossibility.

5. Importance of Yfp of Event B.

Another problem with the present CIM program is that although common sense tells us that the impact of A on B is dependent not only on the yfp of A but also on that of B, in the present program the yfp of B is disregarded.* For suppose that A has given impact on B, B has given initial probability, and A has given, say early, yfp. Either of two situations could occur: either the yfp for B could be early, near A's, or it could be much later. Surely if the effect of the yfp of A on the

*Although we may assume that in general the yfp for an event with initial probability .8 falls before one with initial probability .6, we have no precise notion of when either yfp occurs. Indeed there is no reason to assume that two events with the same initial probability will have the same (or even approximately the same) yfp.

occurrence of B is indeed time dependent, these two situations ought not to be considered identical as they are in the present CIM.

So far I have done little actual work with the problems of the effects of yfp on the impacts, but, recognizing the trouble one can get into with the straight linear dependence, I have rewritten our programs so that they optionally either do not consider the yfp (set time factor to 1), or they consider any event with initial probability less than .5 to have zero impact on all other events. Neither method is totally satisfactory, though we are leaning toward more use of the former than the latter for the simple reason that events with low initial probability very often do come into consideration.

B. Order of Occurrence (ooo) of Events

1. Importance of ooo.

Except for priority levels, events are assumed to occur in a random order. E.g. in game 1, we may have the order A, B, C and in game 2, order B, A, C. Hence, in game 1 event A impacts on B and C but is not impacted upon. B impacts on C only and is impacted upon by A. C is impacted upon by A first, then by the changed B. In game 2, B impacts on A and C and is not impacted upon. A impacts on C and is impacted on by B. C is impacted on by B first, then by the changed A. Obviously the order in which the events are assumed to occur has some influence on the final probabilities of a given game. In game 1, for example, A is not impacted on at all. In game 2, it is by B. If the impact of B on A causes P_A ("probability of A") to rise by 20%, then A would occur 20% more times playing the game 2 than game 1. Event C, which occurs last in both cases, is affected differently. If we assume (for simplicity) that B is not changed by A, then C is impacted on 20% more times by A in game 2 than in game 1.

Clearly, the order in which events are handled by the program is very important.

2. Present Program and Assumptions.

The way the present program handles this problem is to play many games, generating a new random permutation of order N (where N is the number of events) for each game. There are some minor changes to this approach which, I think, will improve the outcome and perhaps cut down the number of games.

Some assumptions on which this procedure seems to be based are:

(1) that each permutation (within a priority level) is equally probable. That is, all events have equal probability of occurring at any given position in the order of events;

(2) that y playing a large number of games the permutations will balan e out around some average, i.e., that in the long run, the random generation of permutations will yield a set of permutations in which all different kinds of permutations occur with about the same likelihood.

Based on these assumptions (which can and will be questioned later) I have made some further assumptions:

(3) that, given (1), a complete game would involve each permutation of order N occurring a large and equal number of times. For instance, in the 3-event example, we would want each permutation (ABC, BCA, CAB, ACB, CBA, BAC) to occur enough times (say m) to yield accurate (within prescribed confidence limits) final probabilities. If a certain permutation were left out, the influence of the possible future represented by that order of events would be lost. Hence, in a 3-event matrix this would yield m x (3!) =6m games,* with each permutation occurring m times.

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*the number of permutations of order 3 is $3! = 3 \cdot 2 \cdot 1 = 6$. The number of permutations of order N is $N! = N \cdot (N-1) \cdot (N-2) \ldots 3 \cdot 2 \cdot 1$.

Such a "complete" game becomes unwieldy as N increases. For a ten event matrix, this formula would require m x 10! = 3,628,800 x m games. In actual practice, fairly good confidence limits can be achieved by playing much fewer games.*

(4) that, given (1), no permutation should occur more than once unless more than N! games are played;

(5) that, given (2), for every game in which an event occurs "early" in a permutation (hence impacts on most other events but is impacted on by few) there ought to be a "complementary" game in which the event occurs "late" in the permutation (hence is impacted on by most events but only impacts on a few);

(6) every event should impact upon every other event as many times as it is impacted upon by every other event.

If the permutations are chosen totally at random, the more games that are played, obviously the more likely it is that these assumed requirements will be satisfied within prescribed limits. One would suspect that one way of reducing the variance (or, alternatively, the number of games) would be to require the random permutations to satisfy as many of the above assumptions as possible. It turns out that by considering cyclic permutation groups of order N, one can do this fairly easily.

I define a cyclic permutation group (CPG) of order N as a group of permutations of order N all of whose elements are "cyclically equivalent."**

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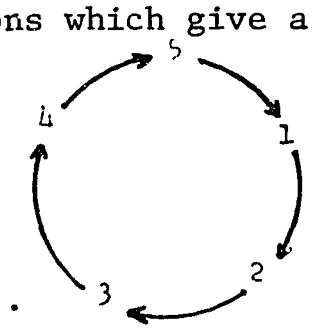
*Richard Rochberg, "Convergence and Variability Because of Random Numbers in Cross-Impact Analysis." Also Howard Johnson, "Some Computational Aspects of Cross-Impact Matrix Forecasting."

**I define cyclical equivalence: A set of permutations which give a circular arrangement, as in the figure on the right, can be used to generate the cyclically equivalent permutations. Any other permutation that can be got by rotating the <u>entire</u> circle is cyclically equivalent. If the figure above represents the permutation 12345, its cyclically equivalent forms are (12345), (23451), (34512), (45123), and (51234).



Every such group contains N distinct permutations. Therefore, since there are N! permutations of order N, and since every permutation is a member of some cyclic permutation group, there must be $\frac{N!}{N} = (N-1)!$ unique cyclic permutation groups of order N for every permutation of order N. No two different CPG's can have any permutation in common since if they did each element of one would be cyclically equivalent to each element of the other, and they would be the same. Hence, if one chooses random permutations from different CPG's for each game, there is no chance of repetition up to (N-1)! games. If less than (N-1)! games are played, assumption (4) is satisfied.

A simple way to satisfy assumptions (5) and (6) would be to do the following: for every game in which a certain permutation is used, play another game in which the order of the events is reversed. For example, if the permutation 6 1 5 3 2 4 is used in one game, another game would use the permutation 4 2 3 5 1 6.

Of course, if the permutations were generated in a completely random fashion, eventually one would be reasonably close to satisfying assumptions (4) (5) and (6), but if one could systematically see that they are satisfied throughout the running of the program, variance due to randomness could be reduced and fewer games would be necessary.

I have developed a procedure for doing this in a semi-random fashion. This procedure is based on an algorithm* which generates from a given permutation all the equivalent permutations (in the same CPG), then automatically generates a "next" CPG of permutations, and so on until

*See Appendix C.

have been generated. A sample run, with the permutation 1 2 3 4 of order 4 as input, would look like:

1 2 3 4
2 3 4 1
3 4 1 2
4 1 2 3
2 3 1 4
3 1 4 2
. (24 rows)
.
.
3 2 1 4
2 1 3 4
1 4 3 2
4 3 2 1

A nice feature of the order in which the permutations are generated is that the ith permutation in the list is the mirror image, or reverse, of the (24-i)th (in general, the (N!-i)th) permutation. Hence, the reverses of the permutations generated in the first half of the permutation list (therefore, of the CPG's also) are in the second half of the list.

Based on this information, the procedure goes as follows:

(1) generate a set of $\frac{G}{2}$ random integers R_i, where G is the number of games to be played, and $1 \leq R_i \leq \frac{G}{2}$

(2) randomly generate a permutation, whose CPG will be the "seed" CPG, or CPG_1

(3) set index counter i = 1.

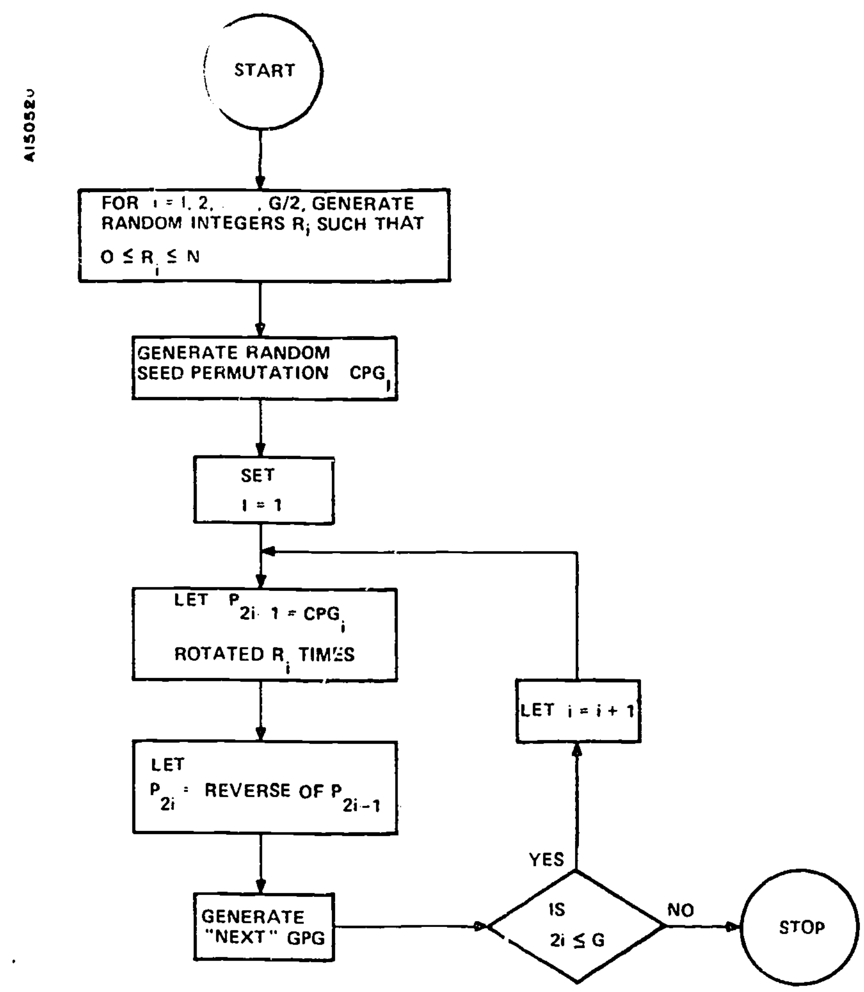
(4) "rotate" CPG_i, R_i times. This gives the permutation P_{2i-1} to be used in the (2i-1)th game.

(5) reverse P_{2i-1} to give the permutation P_{2i} to be used in the (2i)th game. (If $i < \frac{(N-1)!}{2}$ neither P_{2i-1} nor P_{2i} can have been selected previously!)

(6) use algorithm to generate next CPG.

(7) if G permutations have not been determined, return to (4). Otherwise, process is finished.

Symbolically:



If G<N! assumption (4) is satisfied; step (5) satisfies assumption (6), and the randomness of the procedure satisfies assumption (5) if we define "early" as among the first third to occur, and play enough games.*

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*We have found that for a 10 event matrix, 30 games is sufficient to ensure 95% confidence that a given event will occur in the first third at least once. For a 30 event matrix 100 games are required, and the number of games rises with the number of events.

3. Omission of Monte Carlo Procedure.

I refer to assumption (3) in which a complete game involves m x n! games, where m is sufficiently large to reduce the variance (in the interaction of events due to random tossing of weighted coins) to an acceptable level. I argue that at this point in the playing of a game a fairly simple analytic process yields the exact final probabilities that the random coin-tossing procedure converges to. Hence, m is reduced to 1 game and a complete game is reduced in length by a factor of m, to n! games.

Consider first a simple 3 event matrix (events A, B and C) with initial probabilities P_A, P_B and P_C, and impacts S_{AB}, S_{AC}, S_{BA}, S_{BC}, S_{CA}, S_{CB}. Since there are 6 possible ooo's, an ideal run would involve 6 x m games, or m games for each ooo, where m should be sufficiently large to yield accurate final probabilities. To show what we want to show we need only consider a single ooo, say ABC. (A considered first, then B, then C).

In each of the m games using ABC as the ooo, there are only two possible outcomes for event A: either A occurs (1), or A fails to occur (0). In the case of 1 we calculate $P_B' = P_B + P_B(1-P_B)S_{AB} \times 1$. In the case of 0, $P_B' = P_B + P_B(1-P_B)S_{AB} \times 0 = P_B$. The probability of 1 in any game (say the Kth game) is P_A, the probability of 0 is $1-P_A$.

Now let us look at the method used by the CIM to arrive at final probabilities (in this case, for the m games only). For each game a sample value, or random variate, (0 or 1) is generated according to the distribution described above. If $P_A = .7$, then about 70% of the time the variate would be 1, the other 30% would be zero. (If m were 1000, we would expect about 700 1's and about 300 0's.) To figure the final probability of A the program calculates the arithmetic mean (average) of the m variates. If our method of generating random variates were correct, we would expect that mean to equal approximately P_A. Hence, as far as the generation of a final probability for A is concerned, we had the best estimate possible before we began, namely P_A.

Now consider P_B'. We said above that $P_B' = P_B$ whenever A was assumed not to occur, otherwise $P_B' = P_B + P_B(1-P_B)S_{AB}$. If m games were played, obviously the increment $P_B(1-P_B)S_{AB}$ would be added about P_A (70% in the above example) of the time. Now, the final probability for B is found much the same way it is found for A, namely by generating random variates according to the distribution given by P_B'. For example, suppose $P_A = .7$ and $P_B' = .5$ when A does not occur and $P_B' = .5 + .2$ when A <u>does</u> occur. Let us also suppose 1000 games were played. In about 300 games $P_B' = .5$ would be used to generate random variates and in about 700 games $P_B' = .5 + .2$ would be used. Hence we would expect to turn up a 1 about $(.5 \times 300) + (.5 + .2) \times 700 = (.5 \times 1000) + (.2 \times 700) = 500 + 140 = 640$ times, yielding a final probability for B, P_{BF}, of .64. Obviously the <u>increment</u> (corresponding to occurrence of A) is added to P_B that proportion of the time that A occurs, so that $P_{BF} = P_B + P_A \times P_B \times (1-P_B)S_{AB}$ (in the example $P_{BF} = .5 + (.7 \times .2) = .64$) is the best estimate of the final probability of B and the one that the Monte Carlo procedure would converge to for m sufficiently large.

For event C the final probability, P_{CF}, would be similarly arrived at, though the computation is more involved since C is acted upon by both A and B. The reader may want to verify that

$$P_{CF} = P_C' + P_C' \times (1-P_C')S_{BC} \times P_{BF}$$

where $P_C' = P_C + P_C \times (1-P_C) \times S_{AC} \times P_A$.

For a larger matrix, a similar procedure as the one described here would be used.* Hence we have a way of assessing the exact impact of one event on another without using Monte Carlo methods, and we are able to reduce the number of necessary games by a considerable amount. Indeed, it turns out that most of the run-to-run variance is due to the random decision on whether or not events occur, and not on the order of occurrence, so the above procedure serves to eliminate most of the variance.

I have made several test runs using this technique together with the technique described above for generating order of occurrence, and tentatively can conclude that for a ten-event matrix ten games using these methods give as accurate results as 1000 games using the previous technique.

4. Alternative Ways for Generating ooo.

Assumption (1) is "that each permutation within a priority level is equally probable." The use of priority levels is itself questionable since, if used as it is described in the Hudson paper, it does not allow for anything but the simplest and most unlikely notions of priority, i.e., those in which one set of events must have priority over all other events. It does not allow, for instance, for the case in which one event has priority over another event and all other events are not involved in any way in priorities. IFF seems to have recognized this long ago, since they do not seem to use it in any of their programs.

But back to the question of all events having equal probability of occurring at any given position in the order of events. It would seem an obvious refinement of the method to assume that certain permutations of order of occurrence would be more likely than others. For example, an event with year 1 as its yfp would be assumed to occur before another event with year 10 as its yfp more often than not. Hence more games with permutations having the first event before the second should be played.

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*G. Kutsch, Mathematical Formulation of CIM Process as of November 1970, gives a description of the computational process involved.

One way to accomplish this (which we have not tried), would be to consider the distribution of yfp's for each event as estimated by the participants of the study. An example of such a distribution is the interquartile polygon commonly used in delphi studies. Figure 7 shows a time line with (a) polygonal and (b) continuous distributions, rather than points on the line, representing the yfp's. Note that sometimes they overlap and sometimes not. One could take a random sample from each distribution to get a sample set of "dates of occurrence," which in turn would determine the sample order of occurrence for a game.

Alternately, if the number of participants were small, one might rather play one game for each participant, using the yfp's of that participant to determine the order of occurrence for the corresponding game. Or similarly, one could play several games for each participant, sampling from some simple distribution about each yfp.

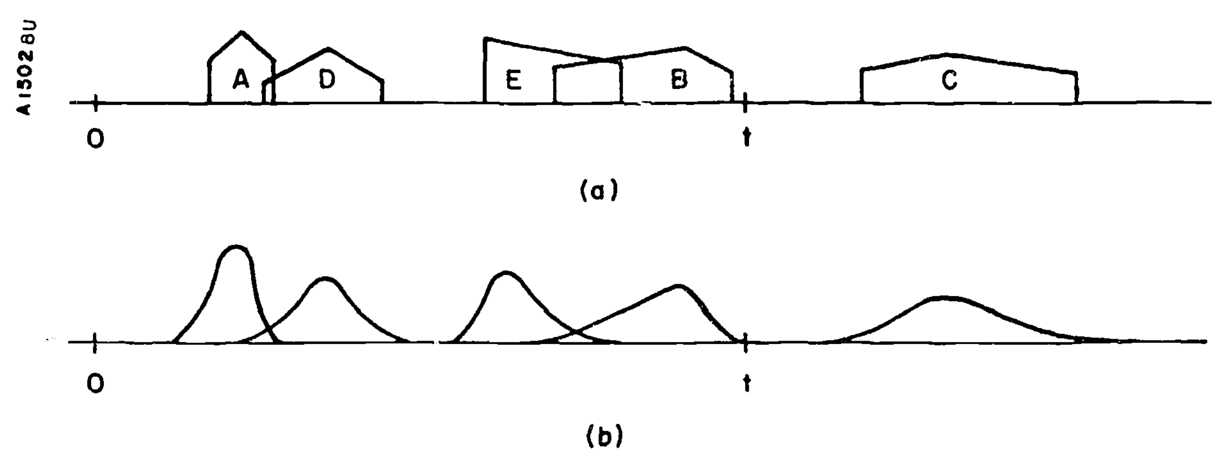

Figure 7. Time line with (a) polygonal distributions and (b) continuous distributions, rather than single points, to represent yfp's.

The above described alternative methods of generating ooo ("order of occurrence") permutations accomplish two things: they generate more likely ooo's of events, and they force the necessary number of games (to achieve a given confidence level) to be dependent upon the amount of agreement among the participants. If the participants agree completely, then perhaps only one game is necessary. Otherwise, the number of games could be greater than the number of participants. In any case, I would expect the number of games to be less than by previous methods. Furthermore, if one of the above methods did prove to be superior, assumptions 4, 5, and 6 would no longer be valid, greatly simplifying the concepts underlying the method.

C. Specifying Input Information

1. Assumptions Constraining the Formula.

There are two assumptions placed on the impacting function which force it to be at least quadratic: if $P_i = 0$, then $P_i' = 0$, and if $P_i = 1$, then $P_i' = 1$. If we retain these assumptions, we are faced with the problem (described in II.A.4) of an unreasonable limit to the maximum change in probability which could result from the occurrence of a single event. Perhaps one way to get around this problem is to keep the two assumptions and see how the concept of impact might be redefined so as to allow a greater range of change in probability due to the occurrence of a single event.

2. The Definition of "Impact."

It is never made clear exactly what is meant by a "strong" or "weak" impact of one event on another. The user is not expected to try to understand, except relative to his other impact estimates, how one event is really affecting another when he says it has a strong impact.* It would be likely that different users have different assumptions about what this does mean. One user might, for instance, think that an impact of +1 would cause the impacted event to be 100% more likely, while another might think it would cause the final probability of the impacted event to be equal to 1.

*The concept of "impact" is rigorously treated from a definitional angle, but without ever getting into the differences in meaning between different numerical values, in Gordon and Hayward, On Correlations Between Events. The Rochberg, et. al., paper begins to ask about these differences by suggesting "sensitivity analysis," but this is of no value to the user for his initial estimations.

Most users would probably expect an impact to have just as great an effect on a low or high probability event as on an event with probability .5, while others, who perhaps understand the formula, might understand that the strength of the impact is very dependent upon the initial probability estimate of the impacted event.

What is needed is a careful definition of the meaning of "strength of connection," or impact, one which is consistent with the formulas and which can be clearly understood by the user. Otherwise, the CIM is nothing other than a black box both to the participant, who doesn't understand what is being done with his inputs to arrive at a set of final probabilities, and to the formulator of the matrix, who does not understand what the inputs mean to the participant. (The problem of the meaning of strength has not escaped the concern of the IFF, and they have been toying with several alternatives, though they seem to have continued to use the standard formula in working with other groups.) One would hope that the impact would be defined somehow in terms of its use in the formula. Some straightforward examples:

a. In each cell of the matrix are entered two numbers, (1) the probability of B occurring if A occurs, and (2) the probability of B occurring of A fails to occur;*

*The initial probability of B is supposed to be arrived at without consideration of A's occurrence or non-occurrence, but as we can see from the following example this is not always practical or indeed possible. For suppose that event B were the winning of the 1971 pennant by the San Francisco Giants. And suppose the participant felt that the chances were good (say $P_B = .8$) that the Giants would win--but his prediction was based implicitly on his supposition (say he is 90% sure) that Willie Mays will play for most of the rest of the 1971 season (event A): if he <u>knew</u> that Willie Mays would (or would not) play then his selection of P_B might be more like .9 (or .3). This kind of situation cannot be dealt with reasonably by the CIM in its present form.

b. Again, two numbers in each cell, (1) the change in probability of B if A occurs, (2) the change in probability of B if A fails to occur;

c. One number, the change in the probability of B given that A will occur with the expected probability;

d. On a linear scale, the proportion of the difference between P_B and 1 (or 0) that the probability will change (1) if event A occurs, and (2) if A fails to occur.

Undoubtedly many more formulas could be suggested for defining a strength of connection. The important points to keep in mind are that the user must understand what he means when he defines a strength of connection, he must understand what that number does in the formula, and he must understand how its role changes when the matrix changes. Also, the strength of connection must give him complete power to alter initial probabilities as much as he thinks they should be altered. If he does not have this power, there is really no reason to set up the matrix in terms of probabilities.

3. The Meaning of Probability.

Certainly one of the major drawbacks of a technique such as cross impacting is that the procedure tends to keep the user in the dark about what the machine is really doing with his information, and he may not even understand what the information he is giving really means. This is especially so when the user is asked to give an estimate of "initial likelihood of occurrence," or initial probability. It is absolutely essential that the user knows exactly what he means by probability, since his estimates are to be treated in a very strict, mathematical way, and if the meaning of the estimates is to be fuzzy, then we can only expect the output of the treatment to be even fuzzier.

The question of subjective or personal probability is a very important one to much of the futures methodology, and one which we should give careful attention to, but not one I am prepared to get into in this part of this paper, since this section is primarily about the tactical procedures involved in the CIM.

D. Summary of the Problems

At least one thing should be obvious from the preceding discussions of various components of the CIM methodology. There are so many questionable areas which require either further refinement or complete conceptual change that it is foolish to think of the method as producing anything but the roughest estimates.

Initial probabilities are very personal, very biased estimates of likelihood, and only in the very loosest statistical sense can they be called probabilities. At best, they give us some idea about how people perceive the relative likelihoods of each of a set of events. The set of events itself represents a personal attempt to describe an environment in terms of all important events and how they interact. Which events are implied or assumed without ever being stated is an open question, and probably different for every user. We have to take on faith the participant's ability not only to perceive correctly the complex interconnections in the environment, but also to make explicit his perceptions, although in fact he may not even be aware of some of them.

The yfp has all the disadvantages of the initial probability which, added to its very influential role in the methodology makes it even more in need of better definition and understanding. Because of the confusion about how to handle the time path of probability, as a function of yfp, we usually do not even use it in our tests. There are indications that IFF also does not employ it in some cases.

The impacting formula as it now stands, with the factors P and 1-P, for P less than one and all other factors less than one, forces undue restrictions on the amount of influence one event can have on another.

Priority levels, as defined in the original method, just do not make sense in most cases, so they have been dropped altogether and in their place I suggest a parameter for handling necessity and sufficiency for each pair of events.

I feel that there are far too many questionable factors in the method to generate any kind of meaningful probability information. I would feel much better about the method if it did not pretend to give results in terms of 'final probabilities' or 'changes in probability,' but simply gave the output as a kind of final score to a game, without any implication that one could know the real likelihood of occurrence of a set of events from using the CIM any better than he knew it before using the CIM.

III

THE USE OF CIM

A. The Heuristic Value

It has been suggested that the most valuable part of the CIM method is the determining of the inputs. This is the stage when the participants are forced to examine very carefully the events which are most important to consider, all the complex interactions among the events, and the likelihood and importance of each event within the whole environment. The next stage has the computer doing all sorts of questionable manipulations of the carefully thought out input data, and generating sets of output which force the participants to rethink their original assumptions and hopefully sharpen their understanding of the complexities of the environment. The important contribution of the method is not that this latter stage generates a more accurate picture of the environment, but that participation in the CIM stimulates improved perceptions about the environment.

For heuristic uses, it may not matter that the method does not work properly as long as the people using it believe that it works correctly. That is, the goal is usually not to find the right answers, but to stimulate the "right" kind of approach to futures, to enhance the "futures perspective," as it were, of the participants. In fact, one wonders whether it might not be better that the technique works improperly, at least some of the time, making it more likely that the participants will be impressed by how different their perceptions of the future can be from the "correct" perceptions.*

It may not matter, for heuristic purposes, whether or not the method provides us with new knowledge, as long as we <u>think</u> it does. CIM may tell us only things we already knew, but if it tells us those things in such a form that we think we <u>didn't</u> know them, then we are impressed, and

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*I have seen this very phenomenon work very well in several CIM exercises. Of course, it always worked best when the leader did not himself realize that the cross impact analysis was incorrect.

perhaps stimulated more towards the "right" kind of thinking. In this case we see how analytic techniques can help us correct our own "mistaken" perceptions.

It may also not matter that the implied philosophical postulates upon which the method is based have never been justified, as long as the participants accept them and the method as valid, and hence "learn" that the discipline of futures research has been generating numerous analytic techniques for studying the future. An aim of an heuristic futures exercise is, after all, to help break the mental set of the participant who has not developed a futures perspective.

In short, there may be strong justification for the use of the CIM as an heuristic tool, and until it can be demonstrated that the method gives much better than "ball park" answers, it should not be used for anything else.

Furthermore, if the method is merely an heuristic one, I suggest there are much simpler procedures for accomplishing the same thing, without the need for questionable formulas, fancy Monte-Carlo techniques, or perhaps even a computer. I outline one such procedure now.

B. Simple Alternative Procedure

Generate the same input information as for the CIM (except for the priority levels and yfp's). For each column, multiply the contents of each cell in the column by the corresponding initial probability and add up all results in the column. Mathematically, this means taking the cross-product of the vector of initial probabilities and the matrix of impacts. The result is a vector of positive and negative numbers, with each element of the vector giving a 'total impact' on the corresponding event of all other events. The meaning of the results is quite simple. It is not a probability, nor a probability change. It is a final score, the result of giving events which are more likely more weight, and giving the heavier impacts more weight. The events which have the highest positive or lowest negative scores are those which are influenced most by the totality of interactions.

Now, if one is interested in those events which are more influential, one could generate another vector by summing the impacts in each row and multiplying that sum by the initial probability of the corresponding event. This would give the "total impact" of each event on all the other events. Sensitivity analysis could easily be done from outputs by minor adjustments to the above procedures. Importance analysis is also quite easy: one need only look at the relative sizes of scores in the two output vectors. For example, an event which had low relative scores in both vectors can be assumed neither to have great influence on, nor be greatly influenced by, the other events.

I have played with this procedure, using the inputs from IFF's case study done at the college of Europe and Bruges in 1969*, and found that this method provided the same information as they were able to garner by the CIM. The advantage of the simpler system is that it is not a black box to the participants and that it does not produce results in a form that is likely to be misconstrued.

*Selwyn Enzer, A Case Study Using Forecasting as a Decision-Making Aid, Institute for the Future Report WP-2, December, 1969.

IV

IMPLICATIONS FOR FUTURES METHODOLOGY

I would now like to discuss the contents of this paper in light of the whole area of futures methodology. I have said that I see CIM useful as a pedagogical device for raising questions about likelihood and order of occurrence of sets of events, about interrelationships among events, and about the consequences of policy actions which could affect the values of these parameters. I have serious doubts about whether CIM could ever be used as a reliable decision-making tool.

Many of the limitations of CIM that we have discussed in this paper arise generally in the area of futures methodology, and I think it is important to examine other research methods in the light of these limitations. Unless we can get a more analytic command of the concepts underlying the techniques we use*, the future of futures research may be limited to the development of purely heuristic methods. I will illustrate the kinds of problems I am thinking of by briefly discussing three problem areas.

1. Simulation. A great deal of success with methods of predicting through computer simulation of simple problems has led us to transfer simulation techniques to more complex problems. But although much useful work has been done in simulating small uncomplicated environments (such as factory operations), there is little evidence to convince skeptics that this can be done on anything but the most macro scale for more complex, more dynamic social and technological societies.

I am not saying we should not study the future by using simulation. I am only saying we should not delude ourselves into believing that we can get precise forecasts by applying rigorous techniques to poorly understood models of an environment.

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*E.g., probability, the variables (exogenous and endogenous) that constitute an environment, trend extension, interactions among events, time, and tacit knowledge.

Consider, for example, a societal model which includes a set of alternative policy decisions. One could try to simulate the making of policy decisions (1) in terms of mathematical formulas based on, say, a delphi analysis, or (2) by bringing together responsible policy-makers who would "play at" making policy in a simulated environment.* If the mathematical simulation could be shown to give (consistently) about the same results as the human one, it might be reasonable to say that that tool was of such a level of sophistication as to be a useful decision tool. If it was not able to foretell how people reacted when faced with a "real" situation, (i.e., if (1) were not able to consistently simulate (2)), we should conclude that the method should never be allowed to dictate policy, though it might be used to shed light on possible policies.

2. Probability. In section II.C.3 I mentioned the problem of CIM dealing with subjective probability estimates in a very strict mathematical way. I will elaborate a little on that now.

Morris Cohen nicely describes the distinction between subjective and objective probability:

> Modern theories of probability may be generally characterized as either subjective or objective, i.e., dealing either with the character of our beliefs or opinions, or with the character of the objective evidence for these beliefs or opinions. . . . The whole modern psychologic tendency puts the emphasis on the mental phase of the beliefs called probable, and this is reinforced by popular discourse, which has many expressions for degrees of probability, such as, "Highly probable," "very likely," "almost certain," "improbable," "not at all likely," and others. We say, "I am almost certain;" "I am quite sure;" "I am convinced;" "It seems to me;" etc. But the whole tendency of modern logic and exact science demands a definiteness in probable judgments which does not seem to be offered by any difference in the intensity of belief.**

*See Bertrand de Jouvenel, The Art of Conjecture, translated from the French by Nikita Lary (Basic Books, Inc., New York, 1967), pp. 288-290, for an interesting discussion of the complementary use of computer and human simulation.

**Morris R. Cohen, A Preface to Logic, (Meridian Books, Inc., New York, 1960), pp. 114-115.

A key concept in futures methods seems to be that of subjective probability. Very many techniques seem to involve guessing about the likelihood of events which can occur, usually only once. I just want to point out here the need to recognize that this type of probability estimate must necessarily involve a vague understanding of such concepts as wishful thinking, incomplete knowledge of both the system under study and how the system would respond to different conditions (as opposed to how one thinks it would respond) and many other "soft" variables. Bertrand de Jouvenel describes the "'mores' that our minds conform to in fore-thinking," which give "rise to very strong feelings of subjective certitude."* He says that "because of its hidden nature this psychological process can have no place in a field of activity that is to be systematic, disciplined, justifiable and discussible."

While I am not ready to discount any use of what de Jouvenel terms "pregnant forecasts," I will insist that a useful decision tool must not only recognize these, it must take them into account. A temptation might be to design a rigorous model that does not need to recognize the "soft" variables, but this would be a foolish path to follow if it is the "soft" variables that the future in question is made of.

Some very lively and fruitful discussions on subjective probability have been going on for a long time among scholars and practitioners in many disciplines. It is generally agreed that we can often organize our thoughts better through the use of this concept than through simple informal intuition.** However, students of subjective probability have not yet determined just how it fits into statistical theory. We may be able to apply relevant research

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*Bertrand de Jouvenel, op. cit., p. 127.

**L. J. Savage, Statistical Inference (Wiley, 1959), is a good introduction to the important issues in the study of subjective probability and its potential influence on statistics.

that has gone on in fields only peripherally related to the study of the future, but this must be done rigorously if it is to form the basis for accurate methods.

3. Two kinds of knowledge, and the comparability of judgments. We have to face the possibility that we may never be able to get a handle on some concepts. I question whether it is possible, for example, to build into any forecasting technique a means for taking into account tacit knowledge. By tacit knowledge I refer to the kind of knowledge which we may use in understanding a process and in making judgments, but which we may never be able to specify.

We make judgments on the basis of a set of mental models that we use to understand the world. We may be able to articulate exactly some of these models, while others may be only subconscious and hence untellable. I will first consider the problems of making judgments on the basis of "tellable" models.

When we make a statement about a future phenomenon we are making it on the basis of our personal understanding of that phenomenon. It is unlikely that our perception of any given phenomenon is exactly like someone else's perception, though they may be very similar.* This is especially true if the phenomenon is complex and the judgment subjective, as is often the case in futures research.

If perceptions of an entity are different, then it may not be possible to compare judgments about the entity, for suppose two people with very different perceptions about a future event each make some judgment about

*See Weaver, Delphi as a Method for Studying the Future: Testing Some Underlying Assumptions, for a discussion of the roles of perception and judgment in delphi.

the event. Even if the two judgments are exactly the same statements, they are not the same judgments.*

Suppose, for example, that I am interested in when, if ever, 50% of elementary school learning will be done by television at home, and I am also interested in the most important consequence of this to society. And suppose Alpha, who (subjectively) sees schools as increasingly oppressive and hence as growing centers of unrest, thinks that policy-makers will look to home TV as an alternative to bringing children together in school all day. He also sees television as more of a mesmerizing than stimulating medium, so he expects children to be less excited and hence less concerned about whether the educational system is giving them what it ought to. Alpha's judgment: "50% of learning will be done by home TV by 1990 and as a consequence there will be fewer dropouts and greater satisfaction with the educational process."

Beta, however, sees greater movement toward open learning systems as schools become more flexible. Policy-makers will decide to offer optional elementary educational programs through home TV. Since certain subjects can be learned better by TV, Beta feels, many children and parents will opt for the TV programs. Beta's judgment: "50% of learning will be done by home TV by 1990 and as a consequence there will be fewer dropouts and greater satisfaction with the educational process."

The judgments of Alpha and Beta are represented by exactly the same statements, but they are essentially different judgments since they are based upon completely different perceptions of the future. We should never say "Alpha and Beta agree on this future event." We should not even say that Alpha and Beta agree on the likely date when the event

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*The distinction here is between the <u>name</u> of a thing (judgment) and the thing itself. I may call my cat Rosebud and you may call your dog Rosebud, but although they have the same name they are very different things, and neither of them is a rosebud.

will occur, for even though they use the same name for the date of occurrence, 1990, their estimates are inferred from contradictory premises and hence might even be contradictory.

My point in this example is that if two people do have different perceptions, different mental models about the future there is no logical reason why their judgments should be comparable, even if their statements make them appear comparable.

The problem of incomparable judgments is, in theory, soluble if we can only get the judges to make known the mental models upon which they base their judgments.* If this can be done, and this is one of the things the delphi method tries to do in a very superficial way**, then at least two advantages accrue. First, there results a better understanding of the different possible perceptions about the future; second, judgments which might otherwise seem to differ only in degree might be found to differ in kind. What does the latter imply for methods, such as delphi, which express a distribution of opinions in terms of a central tendency? How do you express a central tendency among incomparable estimates? What is the average of three cats, two dogs and a rosebud?

Up to this point I have only discussed judgments based upon mental models that can be explained by the judger. These carry with them many problems, but even more perplexing is how to deal with those models which

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*There is an interesting discussion of how one might go about doing this in Stuart Sandow, The Pedagogical Structure of Methods for Thinking About the Future, especially his chapters on futures history analysis.

**Howard Johnson has noted that this superficiality is due not so much to the delphi concept as to "insufficient commitment on the part of the designers, referee(s), and panel members. Designing and executing a Delphi properly requires a great amount of effort, and in most cases the parties concerned are either unwilling or unable to give the required time and effort for unambiguous results." (personal communication).

I described above as untellable.* If certain of the perceptions upon which the judgments of Alpha and Beta are made cannot be made known, then it is impossible to know whether the judgments are ever comparable. Furthermore, since the constructs are subconscious we may never even know whether tacit perceptions exist (see last footnote), so strictly speaking we may never know whether judgments which _might_ involve untellable perceptions are comparable.

The foregoing discussion may seem irrelevant to real life situations, but I don't think it is. I see too many arguments which are eventually resolved when the antagonists finally realize that they are not disagreeing on information or logic, but on their perceptions of a problem. And I see too many problems solved by "intuition" to believe that we do not hold valid logical mental models which we are not able to articulate.

By examining these three problem areas, and similar ones, I think students of futures methodology can improve understanding and credibility of futures research methods. The future of futures research may lie not in the development and dissemination of more of virtually the same techniques, but in the identification of those basic concepts which keep cropping up, and in doing the research required to gain a defensible command of those concepts.

Many of us feel intuitively that we can find ways to get a better handle on the future. If our intuition is wrong, future history may see us as another of those determined movements which tried to achieve the impossible. Leonardo da Vinci compared those who would build the perpetual motion machine with the alchemists who would change the base metals to gold**: "for [their oversight of] a little detail, [friction], everything was lost. . .I

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*See Michael Polanyi, _Personal Knowledge_, and _The Tacit Dimension_, in which he discusses the "inarticulate manifestations of intelligence by which we know things in a purely personal way." He even argues that there can be no knowledge without tacit knowledge. (p. 64)

**Lasidae Reti, "Leonardo on Bearings and Gears," _Scientific American_, February, 1971. p. 103.

remember that many people, from different countries, went to Venice with great expectation of gain to make mills in dead [still] water, and after much expense and effort, unable to set the machine in motion they were obliged to escape.

"O speculators about perpetual motion how many vain chimeras have you created in the like quest? Go and take your place with the seekers of gold!"

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APPENDIX A

Summary and Conclusions on Work Done at IFF, to April, 1970:

A good summary of the work done at IFF is the paper The Use of Cross-Impact Matrices for Forecasting and Planning, by Rochberg, et. al. On pages 42-43 the major strengths of CIM are summarized:

1. It prompts meaningful questions about interrelationships among events under test.

2. It serves pedagogical purposes in raising these questions, in acquainting the expert participants with certain potential future occurrences, and in producing forecasts.

3. It provides a new tool for determining the comparative plausibility of scenarios.

4. It serves as a method of organizing judgment to provide a predictive device in areas in which exact causal relationships are extremely difficult to discern.

5. It provides a method of stimulating certain policy actions (by comparing a "base-line" run with one in which the initial conditions are changed in order to simulate alternative action programs).

While I agree with (1) and (2), I hope this paper will indicate a need to question the validity of (3), (4) and (5).

The IFF paper indicates that research is continuing in the following areas:

1. Development of analytic methods which would allow direct computation of the results (to replace the current Monte Carlo analysis).

2. Exploration of schemata under which probability adjustments are made in the negative sense for those events in the matrix which are judged not to occur.

3. Introduction of sequential computations by which the matrix solution is accomplished in discrete steps, each building on the one earlier.

4. Review of historical data to discover a rational basis, if one exists, for forecasting diffusion time.

5. Development of improved methods of using expert knowledge to fill the cells of the matrix.

Of these problems, only (1) and (2) are directly addressed in this paper, but I feel that (3), (4) and (5) are equally important. I see this list as only a small subset of the questions that need to be addressed.

APPENDIX B

The following is a reprint of most of Chapter I of IFF Report R-10, The Use of Cross-Impact Matrices for Forecasting and Planning, by Rochberg, Gordon and Helmer.

I. *INTRODUCTION*

DEFINITION OF CROSS-IMPACT ANALYSIS

A cross-impact matrix is an array consisting of a list of potential future developments and two kinds of data concerning these developments: first, the estimated probabilities that these developments will occur within some specified period in the future, and, second, estimates of the effect that the occurrence of any one of these events could be expected to have on the likelihood of occurrence of each of the others. In general, the data for such a matrix are obtained by collating expert opinions derived through the use of methods such as the Delphi technique [1,2].* Such a matrix is analyzed in order to:

- Revise the estimated probabilities of occurrence of each development in light of the expected cross impacts of other events on the list.
- Discover how a change in the probability of occurrence of one or more events (by virtue of a technological breakthrough, a social change, a policy decision) might be expected to change the probabilities of occurrence of other events on the list.

Although the computational techniques used in a cross-impact analysis are sometimes complicated, the basic concepts underlying the technique are straightforward. Suppose that the probability of occurrence of each of a set of developments by some year in the future has been forecasted. If these developments are designated $D_1, D_2, \ldots\ldots, D_n$ with associated probabilities $P_1, P_2, \ldots\ldots, P_n$, then the question can be posed: "If $P_m = 1$ (that is, if D_m happens), how do the other P_i change?" In other words, we speak of a cross-impact effect if the probability that one event occurs varies either positively or negatively with the

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*Numbers in square brackets refer to publications cited in the list of references presented at the end of this Report.

occurrence or non-occurrence of other events. Assume, for example, that the following developments, with associated probabilities, were forecasted for a given year:

Development D_i	Probability P_i
1. One-month reliable weather forecasts	.4
2. Feasibility of limited weather control	.2
3. General biochemical immunization	.5
4. Elimination of crop damage from adverse weather	.5

These events might then be arranged in matrix form (as in Figure 1).

If this development were to occur:	Then the probability of D_1	D_2	D_3	D_4
D_1		—	—	↑
D_2	↑		—	↑
D_3	—	—		—
D_4	—	—	—	

Figure 1 - BASIC FORM OF A CROSS-IMPACT MATRIX

The upward arrows indicate an increase in probability. (Decreases would have been indicated by downward arrows.) Thus, if D_2, "Feasibility of limited weather control", were to occur, D_1, "One-month reliable weather forecasts", would become more probable, as noted by the upward arrow.

This kind of array is called a "cross-impact matrix".

Interactions between events are much more complex, of course, than those that can be indicated by an arrow. The arrow denotes only that there is a linkage between events and the direction of the influence one event has on another. In addition, it is necessary to identify the linkage strength (how strongly the occurrence or non-occurrence of one event influences the probability of another) and the diffusion time (how long an interval is required after the occurrence or non-occurrence of one event before another event is influenced).

Once the arrows have been replaced by the requisite numerical data and the relevant formulas have been developed for calculating the changes in probabilities, such a matrix can be analyzed on a computer. Details of the techniques of analysis are presented later. Briefly, the following steps are involved:

1. Assessing the potential interactions (that is, the cross impacts) among individual events in a set of forecasts in terms of:
   - Direction, or mode, of the interaction.
   - Strength of the interaction.
   - Time delay of the effect of one event on another.
2. Selecting an event at random and "deciding" its occurrence or non-occurrence on the basis of its assigned probability.
3. Adjusting the probability of the remaining events according to the interactions assessed in step 1.
4. Selecting another event from among those remaining and deciding it (using its new probability) as before.
5. Continuing this process until all events in the set have been decided.
6. "Playing" the matrix in this way many times so that the probabilities can be computed on the basis of the percentage of times that an event occurs during these repeated plays.
7. Changing the initial probability of one or more events and repeating steps 2 to 6.

By comparing the initial probabilities and those generated in step 6, it is possible to determine how the initial probabilities might be modified to take into account the cross impacts of other events on the list. By comparing the results of step 6 and step 7, it is possible to determine how a change in the probability of occurrence of one or more events would, through the cross impacts, affect the probability of the other events.

QUANTITATIVE TECHNIQUES

To ascertain the cross impacts, the quantitative nature of the interactions must be specified. The following brief discussion presents the formulas that are used and the reasons why these formulas have been chosen. (An alternative computational method is presented in Appendix A.)

Suppose that P_i is the probability of D_i before the occurrence of D_m and that P_i' is the probability of D_i some time after the occurrence of D_m. Then:

$$P_i' = f\ (P_i, M, S, t_m, t), \qquad (1)$$

where:

M is the connection mode,

S is a measure of the strength of connection,

t_m is the time of the occurrence of D_m, and

t is the time in the future for which the probabilities are being estimated.

We know that both P_i and P_i' must lie between 0 and 1; furthermore, where the occurrence of an event increases or decreases the likelihood of occurrence of another event, if $P_i = 0$, P_i' must equal 0, and if $P_i = 1$, P_i' must equal 1 (as indicated in Figure 2).

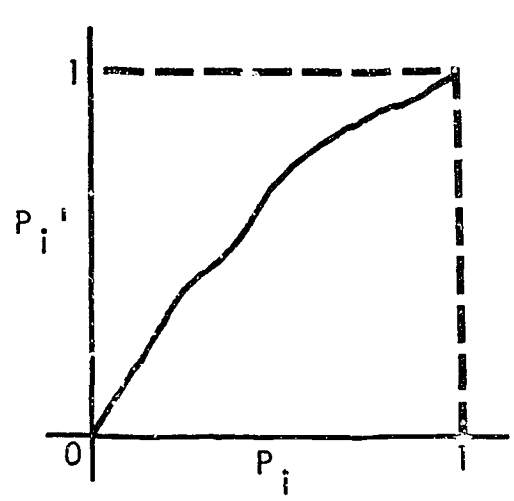

Figure 2 - GRAPH OF A POSSIBLE CHOICE FOR THE FUNCTION IN EQUATION 1

When $t_m = t$, no time is allowed for the adjustment of probability of P_i to P_i', so P_i must equal P_i' (as indicated in Figure 3).

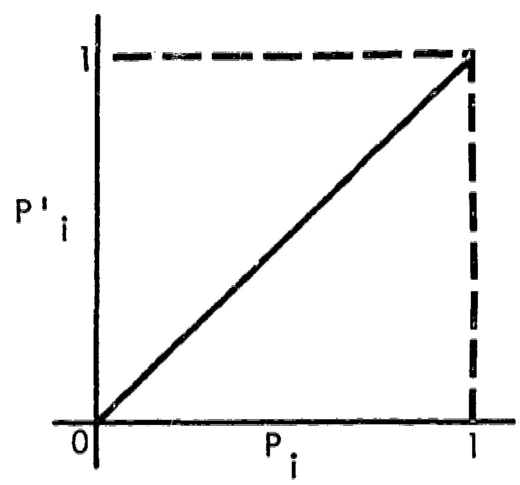

Figure 3 - THE FORM OF THE FUNCTION IN EQUATION 1 OF $t_m = t$

Above the diagonal $P_i' > P_i$ and below $P_i' < P_i$; thus, in the area above the diagonal the impact of an event will be to enhance the probability of occurrence of another; below, it will be to inhibit this occurrence.

It is reasonable to assume that the relationship between P_i and P_i' is monotonic. As a first approximation we will assume that the relation is quadratic:

$$P_i' = a\,P_i^2 + b\,P_i + c. \tag{2}$$

Then, substituting known end conditions, we obtain:

$$P_i' = a\,P_i^2 + (1 - a)\,P_i, \tag{3}$$

or equivalently:

$$P_i' = P_i - aP_i(1 - P_i). \tag{3'}$$

For the inhibiting case:

$$0 < a < 1. \tag{4}$$

and for the enhancing case:

$$-1 < a < o. \tag{5}$$

The question still remains as to how t_m, t, and S are related to a. Although greater sophistication is possible, we make the simple assumption that the relation is linear:

$$a = kS \frac{t - t_m}{t}, \qquad (6)$$

where:

- k is +1 or -1 as determined by the mode,
- S is a number between 0 and 1, a smaller number representing weaker strength (zero designating an unconnected pair), and
- t's are as previously defined.

Now substituting back into Equation (3):

$$P_i' = P_i - P(1 - P)\ ks \frac{t - t_m}{t}. \qquad (7)$$

FIRST APPLICATIONS

The cross-impact method was developed by T. J. Gordon and O. Helmer; it was first tested experimentally by Gordon and Hayward [3]. Gordon and Hayward considered two cases in detail. One was historical, involving 28 events judged relevant to the decision to deploy the Minuteman missile system; the other was futures-oriented, involving 71 events forecasted to occur within the next 20 years and considered likely to have a bearing on future transportation services. In both cases, the events were arrayed in a matrix and estimates were provided for the direction, strength, and time-phasing of the effects of the events on each other. Each matrix was then run 1000 times on a computer, according to the steps outlined earlier, and the results were averaged to produce new estimates of the probability of occurrence of the events in the matrices. The probability shifts identified in this way provided a measure of the combined cross-impact effects implicit in the original matrix.

The situation depicted by the results in the Minuteman analysis indicated that the mutual interaction of the events strongly enhanced the likelihood of a decision to deploy the system. Additionally, a ranking of the events in terms of their final, cross-impacted probabilities provided the ingredients of a scenario quite descriptive of the technological and political environment of the 1950s. Thus, despite the simplifications made in this case, the findings were consistent with what actually occurred.

The probability shifts apparent after the transportation matrix had been run pointed to substantive issues of considerable interest, since there were several significant changes in the original values, but the primary findings were methodological. Particular attention was given to testing the sensitivity of the cross-impact effects to changes in the initial probability levels, thus simulating the influence of conscious policy decisions or of unexpected breakthroughs. The initial probability assignment of certain events was raised arbitrarily by 20 percent, but in no case allowed to exceed .95. When the new run was compared with the earlier "norm", there were a number of differences. Most of them were intuitively plausible, although in at least one case a change occurred that could not readily be explained, thus suggesting that cross-impact analysis may also have a value in indicating unexpected causal linkages among particular events...

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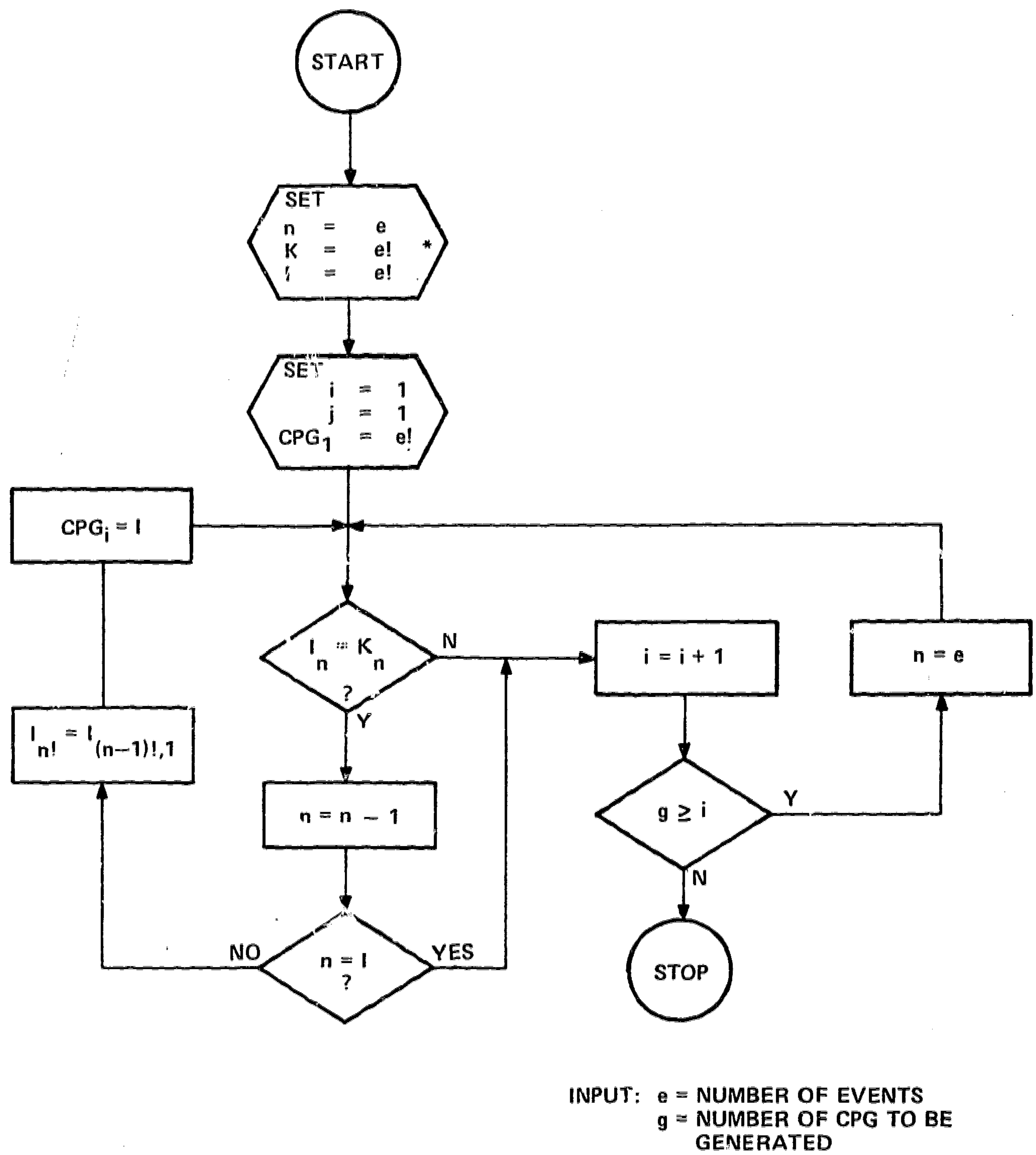
APPENDIX C



Algorithm referred to in section B.2 for generating CPG's.

*e! means the permutation vector 1, 2, 3, . . .,e, rather than, as usually defined, the product 1·2·3···e.