Managing Uncertainty Pragmatically in Private and Public Decision-making about Investment:

By

Bill Malcolm

Prologue:

There is no remembrance of things past, neither shall there be remembrance of things to come: Ecclesiastes

Anticipating the future today is unavoidable.

It is often thought that someone who knows what is coming is wise.

More accurately, the wise person knows that they do not know what is coming.

We can however expect that:

- The future will arrive
- The future will be a different world; they will do things differently there
- Much about the present will be present in the future
- Some important principles that work now will still work in the future
- Much of what we believe to be right now will come to be known to be wrong
- Major factors affecting future lives will be things currently unimagined

1. Introduction[1]

Achieving goals in private or public enterprise involves investing resources to generate consequences through time. There are many reasons why good investment decisions by private firms and by public organizations matter. Using resources well rather than badly is a good start. One further reason that ought to focus the attention of people in agriculture and agribusiness is that there are another 3 billion people amassing around the corner and soon to arrive. This will create challenges, and opportunities, aplenty for agriculture and agribusiness.

2. Investment Analysis and Decision-making

The consequences arising from investment cannot be known beforehand they can only be imagined. Business people and public sector managers have no alternative but to imagine the future, and so do, albeit mostly in implicit not explicit ways. In their imaginings, decision-makers form views about the looks of the future, by drawing on observations, knowledge and experience of how relevant parts of the world seem to work. As well as observations, knowledge and experience, and intuition, people making decisions about investments draw on specialist knowledge from disciplines and sub-disciplines such as the branches of science, engineering, economics, management economics, finance, accounting, sociology, psychology, marketing, risk.

In the context of decisions about public policy, the 1987 Nobel Prize winner in economics Robert Solow explained the strengths, and limits, of economic analysis; saying:

> The true functions of economic science are best described informally, to organize our necessarily incomplete perceptions about the economy, to see connections that the untutored eye would miss, to tell plausible sometimes even convincing causal stories with the help of a few central principles, and to make rough qualitative judgements about the consequences of policy and other exogenous events the end product of economic analysis is likely to be a collection of models contingent on societys circumstances and not a single monolithic model for all seasons (cited in Fitzgerald 1990, p. 21).

Arrow (1992) added the dimension of uncertainty, describing the environment in which policy decisions are taken and implemented:

> To me our knowledge of the way things work, in society or in nature, comes trailing clouds of vagueness. Vast ills have followed a belief in certainty, whether historic inevitability, grand diplomatic designs, or extreme views on economic policy. When developing policy with wide effects for an individual or society, caution is needed because we cannot predict the consequences.

The views of Solow and Arrow about the environment in which policy is formed and implemented can be applied equally to managing resources and making investment decisions, whether private or public.

Attempts to give useful meaning to the scope and focus of term management have long been fraught. Dillon (1968) possibly did it best:

> Our preference, if we want to go beyond saying "farm management is what farmers do", is for what - compared with the above definitions - is a more succinct but far more comprehensive definition. This is of farm management as *the process by which resources and situations are*

manipulated over time by the manager of the farm system in trying, with less than full information, to achieve his or her goals (Dillon 1980).

In contrast to the cited definitions, this statement either recognizes or better emphasizes: first, that farm management is not farm management research, teaching or consulting; second, the dynamic nature of the farm system and its environment; third, the fact that the farm manager deals not just with resources but also faces the challenge of situations to be met and opportunities to be seized; fourth, the active role of manipulation as distinct from the more passive role of merely organizing and controlling; fifth, the uncertainty and consequent risk present in the farm manager's decisions, thereby implying attempted (rather than sure) achievement of objectives based on personal preference and subjective judgement; sixth, by referring to goals rather than profit, the reality of non-profit goals is recognized; further, in the family-farm context, the nonsense of endeavouring to differentiate between the farm as a business and as an economic entity is done away with - the latter subsumes the former and, in our judgement, must be the context in which farm management operates. Too, not only does this definition better capture the challenge and excitement of farm management but, with the deletion of the word 'farm', it serves as an excellent definition of management in general.

The science and art of business management has a huge literature. So too does business planning, and also, the sub-component, strategic planning. Mintzberg (1994) in The Rise and Fall of Strategic Planning presented a comprehensive case as to why strategic planning sounded promising but delivered little; in essence because analysis of parts is not the needed synthesis into wholes and strategic planning is not strategy formulation. As he said, No amount of elaboration will ever enable formal procedures to forecast discontinuities, to inform managers who are detached from their operations, to create novel strategies. Ultimately, the term strategic planning has proved to be an oxymoron (Mintzburg 1994, p.321).

Theorist about the growth of the firm, Penrose (1959) asked the critical question: Why do firms grow or not grow? She explained that many firms do *not* grow because of unenterprising direction, inefficient management, insufficient capital raising, lack of adaptability to changing circumstances leading to frequent and costly mistakes, or simply bad luck due to circumstances beyond their control (Penrose 1959, p.8). Enterprising management was the one necessary condition that, if it was missing, meant there would be no growth. This begged the question what is meant by enterprise? Penrose (1959, p.41.) explained that enterprise services come both from the make up of the manager but equally important come from the innards of the firm itself. By this is meant the necessary entrepreneurial judgement. This involves more than the versatility, ingenuity, ambition and judgement of the management, more than a combination of imagination, good sense, self-confidence and other personal qualities. Entrepreneurial judgement relates closely to the organization of information gathering and consulting facilities within a firm, and it leads to the whole question of the effects of risk and uncertainty on, and the role of expectations in, the growth of the firm.

The case put by Penrose was that expectations of a firm - the way in which it interprets its environment - *are as much a result of the internal process and resources and operations of the firm as it is of the personal qualities of the entrepreneur*. Regarding risk and growth, Penrose said that the greater the risk and uncertainty the more difficult the managerial task and the more information that is needed up to a point. For any given amount of uncertainty the supply and capacity of managerial services will determine the amount of expansion undertaken by the enterprising firm (Penrose 1959).

The essence of decision theory is formal, structured approaches to analysing decisions, with emphasis on how to handle the risk, the uncertainty, the unknowns, the unknowables? Risk and uncertainty are the central and hardest task facing makers of decisions about investing private or public resources. Getting the comparison right is critical: here is how the world might look without the change envisaged and here is hit might look with the change. Compare alternative futures not the current situation without investment and an alternative future with investment. Defining futures, probability analyses, decision trees, scenarios, sensitivity testing, stress testing these are the methods of structured, formal decision analyses.

Simon (1959) developed the concept of limited or bounded rationality. This was about the notion that in the face of much that cannot be known and much uncertainty people opt for near enough (satisficing) rather than perfect (optimizing) decisions that are robust to a considerable range of uncertainty. This seems a good place to start.

Burgman (2005) tellingly concluded the risky analytical decision process and challenge was mostly about being honest and complete in risk assessments (p.ix). Writing about risk in the context of conservation and environmental management, Burgman (2005) talks of a general rule of abysmally poor performance in assessing risk (p.25). Some of this is due to common psychological disabilities of people when it comes to guessing how likely are things to happen, some due to the relative infrequency with which big decisions involving risk are made, as well as the reality that often uncertainty is the case, not risk that can be quantified (Burgman 205, p.24). However, Burgman agreed, as decision theory sets out, that probabilistic modelling can be useful and set out the following steps for building such a model:

1. Develop a deterministic (mechanistic) model.
2. Add stochastic elements to represent uncertainties
3. Add assumptions about uncertainties
4. Use the stochastic model to estimate the statistical distribution of the result
5. Compare the result with reality and update the model.

Importantly, once a probabilistic model of a business activity with investment is built, the key to its usefulness is calibration, sensitivity analysis and validation (Burgman 2005, p.266). Work the model over, seeking understanding and appreciation of risk and uncertainty. He says: done well, model-based risk assessments are transparent, relatively free from ambiguity and internally consistent. Explicit models can capture all available knowledge and be honest about uncertainty (p.313). Monte Carlo based methods, presenting results as cumulative distribution functions, revealing stochastic dominance among alternatives, are valuable supports to decisions. While these methods provide information and support decisions, the understanding of and intuition about the decision environment that derives from the *process* of constructing risk models of reality is one of the most valuable parts of this approach. Revision after results happen too helps the all important continuing learning part of the process (Burgman 2005).

Burgman (2005) also talks of the role of expert advice in decision analysis; particularly the role as well as the frailty of experts (p.82). Evidence is not hard to find that experts think they know more than they do; they dont know as much as they think they know; and dont know that they dont know as much as they think they know. Despite this, there is no getting away from seeking as good a set of sources of good information as can be found and being honest about what is known, what is not known, and what can be known and what cannot be known. Overall, the approach to risk that Burgman advocates is being honest and complete about uncertainty.

Risk communication is part of this being honest and complete. To help with communication about risk, Gigerenzer (2002) advocates using natural frequencies instead of probabilities or odds. Most people, it appears, better understand ten times out of 100 something might happen better than a chance of 0.1 or odds of 11 to one against. Break the alternative future down into main component parts; ponder likelihood of events and combinations of events; describe the situations in raw frequencies (so many times in 100 etc).

Raiffa (1970) was an early contributor to the development of modern decision theory. When teaching about using structured, formal decision analysis to help make choices under uncertainty, Raiffa (1970) said about analysing risky decisions:

> It is often remarked that the most important part of a decision analysis comes in the first stage, where one considers the qualitative anatomy of the problem. Not so, one of my colleagues remarked, the creative stage is the one before that, the stage in which the decision maker decides he (sic) has a problem and decides to consider it in earnest (p.262).

Raiffa (1970) continued that there are a host of questions that precede the stage where there is an identifiable decision maker who has an identifiable decision problem. Who is the decision maker? What is the role and responsibility of the decision analyst? How does one avoid choosing the wrong problem? How can one get the decision maker emotionally, intellectually and administratively involved in a decision analysis? Is the cost of an analysis worth the benefits? How shall management decide whether they ought to adopt decision analysis? These questions apply equally to private or public investment analysis.

Raiffa (1970) outlined the pros and cons of the systematic, rigorous methods known as decision analysis (p.268). He, unsurprisingly, came down in favour of the systematic approach to decision analysis because it encourages the decision maker to scrutinize his problem as an organic whole. (His full discussion of these above questions is contained in the appendix). His final word was that regardless of whether someone analyses a decision using a formal structured approach or not, they must still act; the behavioural assumptions of the decision analysis methods are appealing; the approach is operational; and, finally, he says, What would you do otherwise? (p.272).

When the nature of the challenges decision makers face, and the sophisticated and often complex methods that have been developed to try and help overcome or at least reduce the problems risk and uncertainty creates for people making investment decisions are considered fully, the practical approach boils down to think hard, define perspective and boundaries, define alternative futures, describe well, do some sums, ponder probabilities, define critical thresholds and form sound judgements.

Taken together Solows story telling, Arrows clouds of vagueness about the current and future, enhanced maybe by adding a third metaphor about the mists of time past, explain much about the less than full information environment in which private and public investment decisions are made. The past helps determine the future by creating the present from which the future derives, in part at least.

Add to the mix Mintzbergs limitations of strategic planning. Throw in Penroses line that business success is determined by the qualities of the human capital *and* the insides of the firm the organization, involvement (compared with detachment), incentives for generating and harvesting information within the firm. There is distinct overlap between the Penrose thesis and the arguments of Mintzberg about the fallacies of strategic planning: both these researchers place great store on the role of what could be called the culture of the organization in determining directions, performance and ultimately the success of the firm.

Next, incorporate the insights of the Simon, Raiffa, Burgman, Giregenzer - decision theorists and masters of risk understanding. Finally, throw over the lot the blanket of Dillons definition of management - his comprehensive definition of (farm) management. Combined, the key ideas of these thinkers indicated above, provide the setting, valuable insights and methods for people running private and public enterprises and charged with making investment decisions.

The argument in this paper is that while the clouds of vagueness (Arrow) and the mists of time past are with us now and forever, in modern decision analysis of private and public investment choices, the perspective that defines the scope of the management domain (Dillon), the story telling (Solow), the organization culture (Mintzberg, Penrose), and the decision processes (Simon, Raiffa, Burgman, Giregenzer) all, often, need work.

Too frequently, the unknowing can hide behind the clouds of vagueness surrounding decisions about investment, leaving ample scope for proponents of particular investments to choose to believe almost anything, to hope that the past will be the future, to simply hope for the best. Less of this, and more rigour, a more structured and documented approach, more imagination and plausible stories of alternative futures, is to be preferred. Imagining the future with rigour means adopting the structured approaches to decisions developed in modern decision theory that emphasises probability analyses and alternative futures, with sound appreciation of the pervasiveness of uncertainty.

Learnings from the process

The process of imagining the future is useful in its own right. Francis (2007) makes a good point about the value of attempts to know about the future, even though we know these guesses will likely be wrong. Francis (2007) said:

> Arguably, reports by oracles and high priests have served a similar purpose in the past. In this sense, they are all important and useful, even if wrong.
>
> To illustrate the point, I am going to quote Kenneth Arrow out of context. In a very personal, worldview discussion of uncertainty and the hopelessness of accurately modeling what will happen in the real world of markets, Arrow offers the following anecdote from World War II:
>
> *Some of my colleagues had the responsibility of preparing long-range weather forecasts, i.e., for the following month. The statisticians among us subjected these forecasts to verification and found they differed in no way from chance. The forecasters themselves were convinced and requested that the forecasts be discontinued. The reply read approximately like this: ' The Commanding General is well aware that the forecasts are no good. However, he needs them for planning purposes.*
>
> Arrow's point is that, at one level, the weather reports really were useless because they were wrong. Yet he then goes on to say that:
>
> *Accuracy of prediction is a desirable aim, but it is not the only aim of economic theory. As in meteorology, understanding is possible, desirable, and useful even when predictability is very limited*.
>
> We stress accuracy and statistical robustness when we teach econometrics and modeling. We also get bogged down in debate on the merits of observation vs. simulation. Maybe, in paying attention to the accuracy of the art, we sometimes lose sight of its purpose. Accuracy is not the only important function of forecasting and numerical modeling. It may also be to create space for constructive argument.

Learnings from the Many

Furthermore, to construct plausible stories we need to draw on what is known (disciplinary knowledge, principles, everyday sense) and especially we need to *learn from the many*, drawing on all relevant sources of knowledge, and especially from the discipline of choice and risk, economics. In the world of investment analyses, the successful are relatively few but the failed are relatively many. A successful analysis manages to capture as well as can be done consideration and weighing up of the risk and uncertainty associated with the investment. Note that successful analysis of investments is different to the outcomes of the investments, where successful outcomes may be the result of unanticipated developments, or good or bad luck.

The reason Dillons definition of management is valuable is because it emphasizes goals, processes, making choices, and doing so with less than full information. Tellingly, whilst these notions are at the core of the material making up text books in economics and management economics, texts in management or accounting generally deal little, or inadequately, with these concepts. Without risk and uncertainty there would be no management planning would suffice. Economic ways of thinking are at the heart of successful analysis of investment.

Note: economic ways of thinking goes beyond the simplified caricature of the real world represented by the neo-classical framework. The neo-classical model is about: if the world was like this and people behaved like this then this is what would happen to achieve the best outcomes. It is a valuable construct. If this model is not involved at the abstract level for which it is designed, other implicit non-economic or less developed economic models are being drawn on. The trick is not to mistake the economic model for the reality. Understanding reality is about understanding dynamics and recognizing the existence of imperfect information, not just well informed static comparisons.

The genius of Dillons definition of management is that not only does it emphasize goals, process, choice and uncertainty, it is unequivocally forward looking. Contrast this approach with backward looking accounting with its incomplete stories about the past (e.g. tax accounting, historical cost accounting) and irrelevance to the future. Or, emphasis on cost of production, a concept lacking meaningful economic content (Campbell 1944). Or, management without knowledge from economics and finance. This too is incomplete necessary maybe, but not sufficient.

So while imagining the future with rigour to enhance decisions about investment choices most certainly requires learning from the past, and the learnings of the many, having accounting and management disciplines dominating organizational culture of enterprises, with inadequate incorporation of learnings from management economics, economics, behavioural finance, and the technical disciplines, makes whatever is the job of the private or public firm harder than it would otherwise be.

The power of economic thinking is in making sense of resource allocation questions in systems characterised by much complexity and powerful dynamics. The logic and rigour of economic thinking act as antidotes to the merely intuitive (Malcolm 2004). Thinking hard about choices and futures will always beat simply hoping hard (irrational exuberance). In another context, the method advocated for analysing decisions for farm businesses in the face of great uncertainty, was summed up as**:** A Few Disciplines, a Few Perspectives, a Few Figurings, a Few Futures (Malcolm 2000).

Perspective: The world is round and the parts are connected

A starting point to analysing risky decisions is to recognize the environment in which the investment will apply. This means explicitly recognizing that the world is round and that the investors are dealing with whole slices of it. That is, the environment in which the investment will apply is not flat and linear, as the agribusiness sector is sometimes represented (Figure 1), but many-dimensional with multiple linkages every which way, as suggested in Figures 2, and 3 below. The more useful investment perspective recognizes that the world is global not flat, and the connections between resource uses and implications of investments flow many directions. Appreciate that (almost) everything is connected to (almost) everything else (Figure 2, 3,). The arrows go many ways.

Figure 1 Flat world View

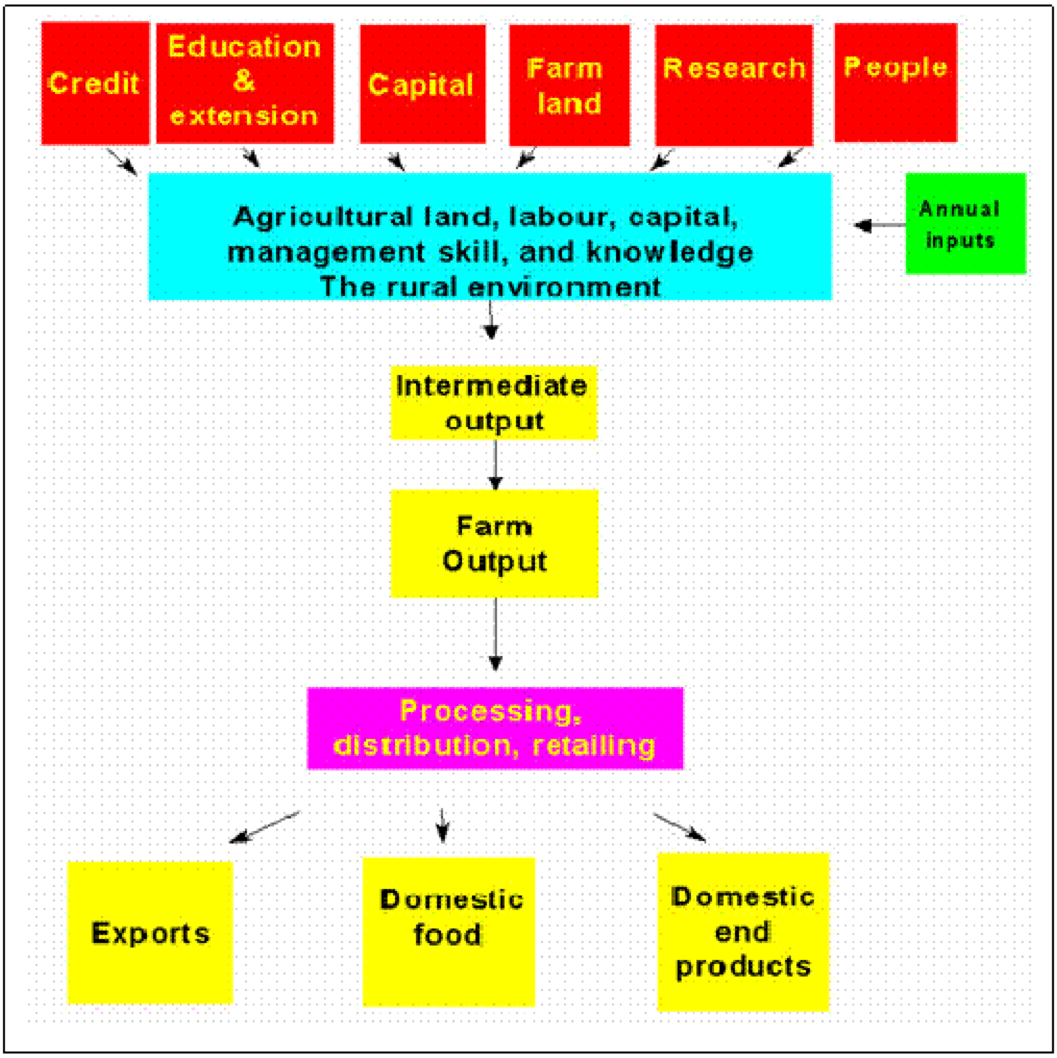
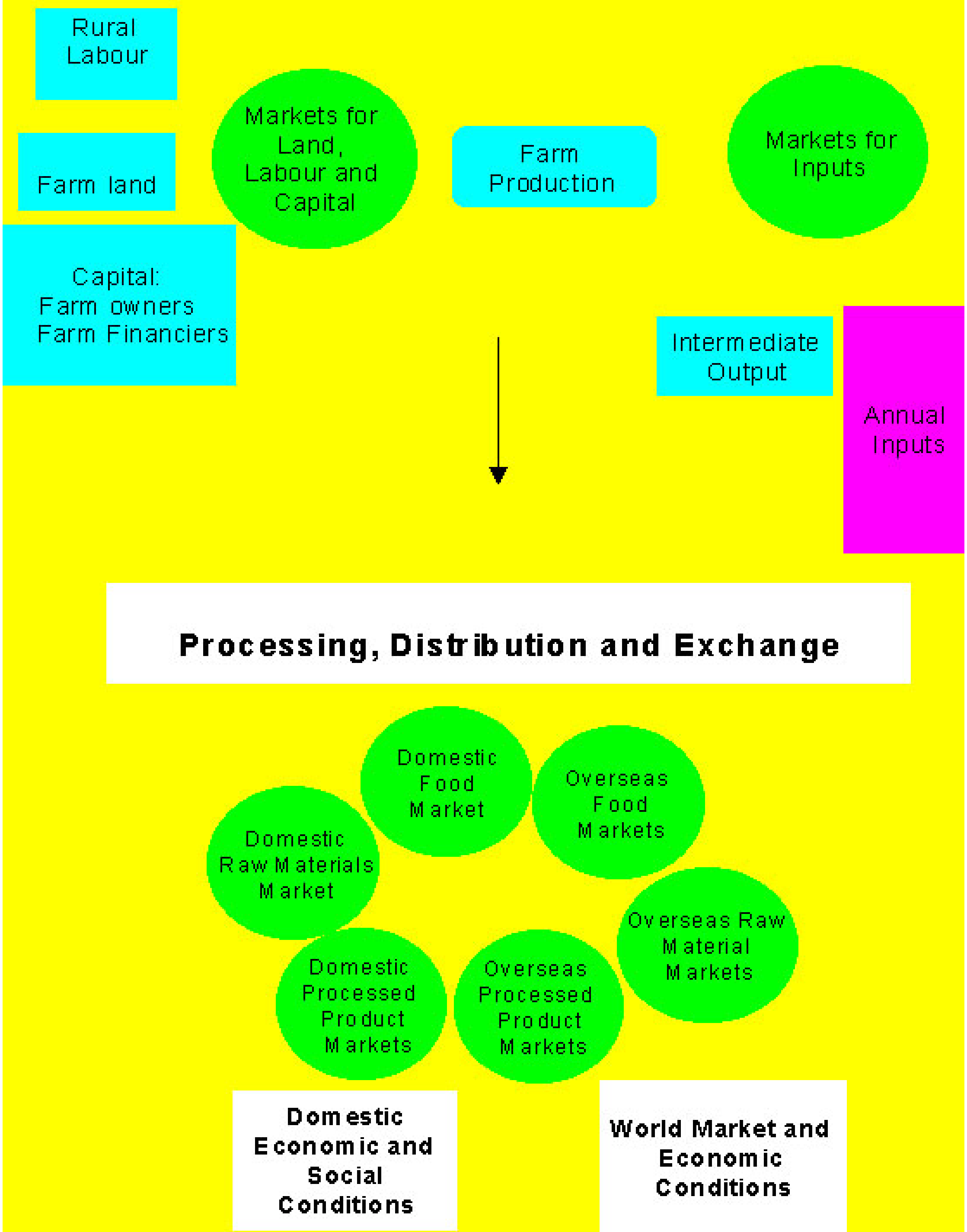


Figure 2: Sectors as Slices of the Economy

Figure 3 Agribusiness Slice of Economy Markets: Linkages would go every which way.



Having defined the perspective and multi-dimensions of the environment into which an investment is being contemplated, remember that we know little for sure about much, especially about what is coming. Improving the guesses is the challenge of the disciplines involved in informing the decision about risky and uncertain decisions. Improving guesses about investments is the key to maintaining and improving the well-being of people and of the world. In doing so, we can draw on what is known, especially some big principles such as:

- The laws of supply and demand
- The principle of comparative advantage
- The principle of diminishing marginal returns
- The principle of increasing risk
- The probability principle
- The risk versus return principle
- The whole of business principle
- The all farm systems and their managers are unique principle
- The quality of management is the hidden, unmeasured input to success and failure principle, and
- The question is the answer principle

A Useful Approach to Analysing Risky Decisions: the threshold or breakeven method

A threshold is a marker that separates two phenomena. In the context of investment in Research, Development and Extension (R, D&E), threshold refers to a critical level of a key variable that separates an investment from being a bad investment to being a good one (the break-even point). For instance, often we have a reasonable idea of the costs that may be involved in a research project or program, and can define with reasonable probability the *nature* of the likely innovation outputs, such as increased yields or cost savings. To know the benefits we need to know how much yields or costs might change and how many users of the innovation there will be, and when they will adopt the change. These are the key numbers, and are unknowable before the event. A judgement about these key numbers has to be formed though, in order to assess the prospective net benefits from the investment.

This is where the threshold approach comes into its own. The question What are the expected benefits? is turned around to How big would the expected benefits need to be for this investment to be a good investment, defined as say earning 20 per cent return on capital. Then, some scenarios can be defined change in yield or cost, number of adopters, when adopted in the future- that, combined, would provide the annual benefits that would generate 20 per cent internal rate of return, given the costs of the investment. We now have some concrete numbers to think hard about. How realistic is it that these sorts of changes in yields, or costs would occur, and these sorts of numbers of adopters would happen over this sort of time horizon? We have key information and can form judgements about the likelihood of these outcomes occurring. If the judgement evolves that it is reasonably likely (or unlikely), for a range of combinations of outcomes, that the investment produce an acceptable return on capital (or unacceptable return on capital), then this is really helpful knowledge for the decision-makers. Simple, but quite a profound method.

For an example of a simple threshold tool, see White (1999) and an application of the method to a dairy program, see Malcolm and Paine (2006).

Investing to Procure New Information: research and development and extension/marketing

Some general observations relevant to both private and public research

Investing in research, development and extension/marketing is a process of investing to buy information. In science-based activities, the success of a firm or public organization depends on how well it conducts research, development and extension/marketing.

Imagining the future with rigour in investments in research, development and extension/marketing means acknowledging the role chance and uncertainty plays. The probability of success of a project to invest in research, development and extension is determined by the probability of technical success, probability of commercialization given technical success and probability of economic success given commercialization. Hence, risk creates return - but how much risk needs to be taken? Investments in research, development and extension/marketing are more risky than many other economic activities, so the portfolio approach to investing is a relevant guide to the range and combinations of these activities that are undertaken. The portfolio principle dictates combining investments with different riskiness, whose returns are not correlated, in order to enhance expected returns for any given level of risk, or reduce risk for any given level of return. The portfolio approach is nothing more than the old investment saws Dare to win and Dont put all your breakables in the one container. These work.

Some investments pay off in a big way; many have the only return of demonstrating that a particular line of inquiry is a dry hole. This is well worth knowing though. The benefit from investing in research that does not produce the anticipated results and payoff is that future investors know more about what not to do and where not to go. The value of knowing what not to do is not to be underestimated. The dichotomy of research being successful or unsuccessful depending on whether an innovation derives from it, that is widely adopted and makes a big difference, is too narrow a view. There is much to learn from failure! Also, even when, *ex ante*, there is one most likely avenue to tackle a research problem, history tells that parallel investigations of alternative approaches to solve the same problem proves invaluable surprisingly often the unlikely prospect comes through.

Success in investments in research requires spelling out business objectives to scientists. Research makes sense only when undertaken in areas relevant to economic goals. Economic evaluations force managers to make their assumptions explicit. Close links between staff who conduct research and development and staff who work closely with the end users is highly useful. As Mansfield (2002) reports: Numerous case studies of successful and unsuccessful innovation have concluded that the closer the link between marketing and research and development, the greater the probability of commercialization, given technical completion (Mansfield 2002, p.299). Even then, ultimately, some of the questions of innovation can only be answered by actual production and marketing.

Public investment in research

In modern mixed economies private firms do what firms do. Private entrepreneurs identify opportunities, obtain capital and take risks and prosper or decline. This is the role of the firm. Public sector activity is governed first by the criterion that something should be done by the public about something?

Neoclassical economic theory consigns the role of public economic activity to areas where the markets fail to generate the outcomes desired by the public, in either efficiency or equity dimensions, and the benefits of correcting for this failure exceed the costs of fixing the situation. An alternative explanation of market failure can be couched simply in terms of transaction costs something is not provided in the economy because the costs of doing so exceed the benefits.

In the neo-classical economic model, the main areas of failure of markets from the perspective of efficient use of resources are designated as situations characterized by goods and services that have the features of being non-excludable or non-rival in consumption; that are associated with external or spillover effects; that involve asymmetric information and one party to a transaction using this feature to advantage over others; or where market power is involved and inefficient outcomes result. In any of these situations the quantity and quality of goods and services supplied, and or the price at which they are supplied, are less than optimal in comparison to what would happen under conditions not characterised by these types of conditions. That is, compared to what would happen if the market worked in this area, not failed.

Once a market failure from the perspective of efficiency of resource use is identified, the key question then is: Is the problem (the market failure) worth fixing? Market failures abound throughout economies, precisely because they are not always worth correcting; the expected benefits do not exceed the expected costs. If the expected benefits of correcting a failure of a market exceed the expected costs, then it should be done because, by definition, the efficiency of resource use and welfare of the public will be increased by the resulting *net benefit* created by solving the failure.

Implicit within this definition and criterion for public action is that the action does not crowd out private sector activity that would otherwise occur. Also implicit in the conclusion that market failure should be corrected if expected benefits of doing so exceed expected costs is that the correct instruments are chosen to correct for the failure of the market. This is because the way the failure is fixed (choice of instrument) helps determine the size of the costs of fixing the problem and the benefits from doing so.

Having passed the efficiency oriented market failure tests (sub-optimal provision of goods or services by private market and benefits exceed costs of public provision of those goods and services) that justifies public resources being used to supply certain services or goods, the question arises as to what other similarities to and differences from private criteria might apply to public investment analysis. Certainly the economic criterion of expected benefits versus expected costs, opportunity costs and equi-marginal returns apply equally to public and private investment decisions. Certainly decisions have to be financed either from accumulated surpluses or from borrowings. One significant difference between the situation of the public and private sectors is the way exposure to risk and uncertainty can be treated.

As Arrow and Lind (1970) established, when it comes to risk and uncertainty, at a general level at least, the public is hedged against a wide range of risk and uncertainty because, in contrast to private firms, the public sector goes on forever and makes large numbers of investments across types of activities, across space and through time. This characteristic of public investment makes for a changed role of risk in analysis of decisions about investments. Public sector managers need not feel the burden of risk weighing as heavily in their prognostications (Alston *et. al.*1995). Indeed because of their hedging capacity they may be obligated to include a proportion of investments in their portfolio that have potentially very high risk and potentially very high returns, because the very high risk involved may mean that these investments will not be undertaken otherwise if left to the private financial sector. The scope of total portfolio balance across types of investments can be wider and riskier than would be the case if the public was not hedged across many types of investments and many generations of investors.

An implication is that public sector investors can put more emphasis on the expected value of an investment than the private investor who has to consider both the mean and variance of the investment prognostications (Alston *et. al.*1995). Expected value of a single investment makes private sense if you expect to live forever and do not much care what happens along the way. The public does plan to go on forever and makes many different investments - hence expected value is a useful criterion for public investment analysis.

This is not to say that risk is not part of the analysis of the public investment - risk has to be assessed in order to estimate the expected value of the outcome of the investment. The aim though is to identify the investment with the highest expected value, which will be the best one to choose as the public can more afford to prefer risk and take on more risk than can private individuals and businesses. The public need not be not as fazed as the private sector by whether the risk is high or low, and can focus mainly on prognostications as to whether the expected outcome (e.g. mean net present value) is high or low (Alston *et. al.*1995).

A similar story exists with uncertainty too. Uncertainty for the private sector - the rare events that have big impacts - are potentially more devastating for private firms than for the public sector, simply because of the capacity of the public to weather the storm. Does this mean that the private sector rule of considering well events in both the middle of distributions *and* the tails, especially joint events from tails of distributions, does not apply to public investment decisions?

The public is in a unique situation of having to act to correct market failure and doing so at least cost (greatest value for taxpayer dollars) while at the same time being able to prefer more risks/uncertainties than is the case for private investors. How are these two apparent contradictions the need to use taxes prudently and efficiently to correct market failures and being able to invest with greater risk and uncertainty - affecting the expected value of the investment- reconciled? A good start to fruitful public investment in these circumstances is to go to considerable trouble to ensure that the strict criteria about the role of public investment (the presence of market failure in the efficiency sense, Benefits exceed Costs) that justify the public activity, are met. This at least reduces the possibility of the public investing when it should not be investing.

To sum up: risk and uncertainty is part of the estimation of expected value of public investments but, having estimated the expected value, the public can choose the investment with the highest expected outcomes and ignore the volatility of outcomes around the means of the alternative investments. This approach is defensible as long as the criteria for public investment are strictly applied in the first instance in identifying the suite of prospective investments.

Portfolios of investments in research

The public good nature of findings from agricultural research makes the general case for public investment in agricultural research. There is a considerable literature on investments by the public in agricultural research, amidst growing concern about whether this investment is proceeding at a rate that is commensurate with the demands for new technology to lift productivity to the degree needed to feed the looming increase in world population. This concern points to the need to exercise much care in choosing public investments, to improve the odds that investments with high returns are included in portfolios.

The portfolio of research investments of public agencies has potential for greatest impacts when two characteristics of the system are maximized: economies of size and scope are exploited, as well as spill-ins from other research (Byerlee and Traxler p.164 in Alston *et. al.* 2001). Given this, public agencies have to decide whether to do more investment in one area and less in another, consistent with the concepts of opportunity cost and equi-marginal returns. The applicability of two concepts to the decision about the marginal investment can be tested by asking the question: is the marginal investment expected to earn a return comparable with an alternative investment that could be undertaken if this bet was not made? To do this public decision makers need to decide which investments are superior, meaning expected to contribute most to attaining goals, and which investments are inferior, meaning expected to contribute less than a realistic alternative.

One criterion for the choice of the portfolio of public investment in research is the expected value of the potential net benefits. The expected net benefits of new technology are determined by the change an innovation can make to individual production or market activities, the number of potential adoptees of the change, and the size of the economic sector in which the adoptees operate. Expected extra profits to producers or extra net value through marketing chains has to be assessed, along with the likelihood of these net benefits being realized.

Other, non-pecuniary benefits and costs have to be considered in forming judgements about the overall expected merit of the investment. Identifying, defining, and subjectively incorporating these hard-to-value matters into considerations and judgements is a better approach than saying they are too hard to measure so we will pretend they don't exist, that their value is zero or infinite.

Imagining and then telling plausible stories about the future. Transparency. Expert judgements. A few perspectives scientific and economic. This is what it comes down to despite the maybe first-look unsatisfactory-ness of this being about all we can do. Best available knowledge. Structured organization of information. Contemplating futures. Risks. Utmost honesty. Completeness. What else? This is better than unimaginatively allocating scarce resources by acting randomly or simply making decisions according to the narrow dictates of particular interest groups.

In practice, imagining the future with rigour mean private and public investors doing what exactly?

The argument is that probably the best we can do in decision-making under risk and uncertainty is to use what we know about the past and present, imagine what might happen, and form judgements about how the world may look and work in the future. To do this it is necessary in an organization to divine the important principles (ways of thinking) that are likely to continue to determine significantly what happens how and why, counting on those principles still working to a reasonable extent in the future, and maximize the opportunity for these principles to work in organizations in the future.

Attempting to imagine the future and doing so in a rigorous way, has some similar benefits it makes possible better understanding. Burgmans (2005) honesty and completeness in analysing decisions and making sound investment decisions under unavoidable conditions of risk and uncertainty is the key here. This may mean, at the least, being explicit about the implicit. Document scenarios and their risks and uncertainties. Test sensitivity of critical numbers. Recognize that a small number of parameters determine outcomes. In a sound process, in a well-run organization, imaginative, honest, complete, and rigorous approaches to the questions permeate the organization and the judgements that emerge from the decision processes of the organization. In practice, this means using approaches to forming judgements and making decisions that explicitly incorporate good understanding of the following:

- the clouds of uncertainty surround decisions; this means the least, and best, we can do is undertake serious decision analysis, using structured, formal and well-documented approaches.
- economics is the discipline of choice and risk
- without risk and uncertainty thered be no need for management; planning would then suffice
- apply the correct perspective; place the organization for which the decision is being made at the centre of the environment and consider the many directions and forms of connection with the world
- the folly of focussing too much on the past while recognizing that the past has created the constraints and possibilities of the present and future
- the creative enterprise of individuals in the organization as well as the innards of the organization are the keys to the knowledge needed for growth of organizations (Penrose 1959)
- a clearly defined role of government justification is the best defence against wasting scarce public resources in the face of risky and uncertain investments
- a whole-of-system not partial focus is necessary
- looking forward (management economics) not backward (accounting). We can do better than hoping the past will continue in the future
- it is useful to explicitly imagine a small number of futures we do this implicitly anyway, so make it explicit, and even though this does not tell us much about likelihoods, what else can we do?
- compare alternative futures, not a future compared with the current situation
- economic analysis (efficiency, opportunity cost, equi-marginal returns, is it worth doing) and financial analysis (cash flow, who funds it) growth in wealth (net worth, balance sheet structure) are different, necessary parts of analyses
- understanding about the key elements of systems and the basic sources of net benefits, and implications of changes to systems
- understanding of what the organization can be best at, in some domain
- doing what the people in the organization are passionate about
- being mindful of the dictates of the principle of increasing financial risk that constrains size (Kalecki 1937)

- it is important to focus on distributions not averages, and especially on events in tails *and* middles of distributions
- that errors compound in budgeting; variance around the means of two variables (e.g. price and quantity) combine into wider variance around the sum of the two variables (Income)
- the *nature* of main benefits and costs have to be defined well, even if we cannot measure them
- benefits and costs should be valued if it can be done; remembering that putting a number on something may create an impression of precision, but it isnt necessarily so
- thinking hard about benefits and costs we cannot measure is worthwhile
- when costs are knowable and benefits are unknowable, use the threshold/breakeven approach. For these costs and this required return on investment, the benefits would have to be of this size. Benefits of this size could/could not be achieved in the following manner.
- Compile a plausible story about the investment in question, with a few angles, exploring a few futures, and encompassing a few calculations.

Who bets?

References

Alston, J.M, Norton, G.W. and Pardey, P.G., *Science under Scarcity*, Cornell University Press, 1995.

Arrow, K.J. and Lind R.C., Uncertainty and the Evaluation of Public Investment Decisions, *American Economic Review*, Vol.60, No.3, June 1970, p.364-378.

Arrow, K. 1992, I know a hawk from a handsaw, in M. Szenberg (ed.),

Eminent Economists: Their Life and Philosophies, Cambridge University Press, Cambridge.

Bernstein, P.L. 1996, *Against the Gods: the Remarkable Story of Risk*, John Wiley and Sons,

Byerlee, D. and Traxler, G., *The Role of Technology Spillovers and Economies of Size in the Efficient Design of Agricultural Research Systems*, in Alston, J.M, Pardey, G.W. and Taylor, M.J., *Agricultural Science Policy*, eds, John Hopkins University Press, Baltimore, 2001.

Burgman, *Risks and Decisions for Conservation and Environmental Management*, Cambridge University Press, Cambridge, 2005.

Campbell, K.O. 1944, Production cost studies as a field of research in agricultural economics, *Journal of the Australian Institute of Agricultural Science*, vol. 10, pp. 3137.

Dillon, J.L. (1980). 'The Definition of Farm Management', *Journal of Agricultural Economics* 31:257-258.

Francois, J., *We know the Weather Reports are Wrong*, The Random Economist Blog, January 27th 2007.

Gigerenzer, G, *Reckoning with Risk: Learning to Live with Uncertainty*, Penguin Books, 2002.

Hill, D.J., Piggott R.R. and Griffith, G.R., Profitability of incremental generic promotion of Australian dairy products, *Agricultural Economics* 2000:114

Kalecki, M., The Principle of Increasing Risk, *Economica*, Vol. IV, 1937, pp.440-447.

Malcolm, B, Farm management analysis: a core discipline, simple sums, sophisticated thinking, *Australian Farm Business Management Network Journal*, 2004, vol. 1, pp. 4556. [Online] at http://www.afbmnetwork.orange.usyd.edu.au

Malcolm, B**,** A Few Disciplines, a Few Perspectives, a

Few Figurings, a Few Futures, Invited paper presented to AARES Annual Conference, Sydney, 2000.

Mansfield, E.Allen, W.B., Doherty, N.A. and Weigelt, K., *Managerial Economics: Theory, Applications and Cases*, 5th edition, W.W.Norton and Co., 2002.

McConnell, D., and Dillon, J., Farm Management in Asia: a systems approach, FAO, Rome, 1997.

Mintzberg, H., *The Rise and Fall of Strategic Planning*, Prentice Hall, Great Britain, 2000.

Penrose, E., *The Theory of the Growth of the Firm*, Oxford University Press, 1959.

Raiffa, H., *Decision Analysis: Introductory Lectures on Choices Under Uncertainty*, Addison-Wesley Publishing Co., California, 1970.

Simon (19 Theories of decision-making in economic and behavioural science, *American Economic Review,* 1959, reprinted in Microeconomics Selected readings, 3rd edition, E.Mansfield, W.W.Norton and Co. 1979.

Solow, R., Fitzgerald, T. 1990, *Between Life and Economics*, Boyer Lectures, ABC Enterprises, Sydney.

Wright, V. 1983, Some bounds to the relevance of decision theory, *Australian Journal of Agricultural Economics*, vol. 27, pp. 221230

Appendix

Extract from Raiffa (1970) Decision Analysis: Introductory Lectures on Choices under Uncertainty

Chapter 9 The Art of Implementation and a General Critique: p.262-272

5. GETTING STARTED ON A DECISION ANALYSIS

It is often remarked that the most important part of a decision analysis comes in the first stage, where one considers the qualitative anatomy of the problem. Not go, one of my colleagues remarked, the creative stage is the one before that, the stage in which the decision maker decides he has a problem and decides to consider it in earnest. So far in these lectures, we have always assumed that there is an identifiable decision maker who has an identifiable problem. There are a host of intriguing questions that precede this stage of development, and in this section we shall consider such questions as these:

Who is the decision maker? What is the role and responsibility of a decision analyst? How does one avoid choosing the wrong problem? How can one get the decision maker emotionally, intellectually, and administratively involved in a decision analysis? Is the cost of an analysis worth the benefits? How shall management decide whether they ought to adopt decision analysis? I wish I had a good set of answers for these questions. Perhaps the best I can do is offer a few rather disconnected comments to make you sensitive to some of the issues.

Who Is the Decision Maker?

Suppose Mr. Smith holds a place in the hierarchy of management of a large corporation and the corporation calls an analyst in to help him with a specific problem. Smith wants to know what he should do. So as far as the consulting analyst is concerned, Smith is the decision makerthe analyst must use Smiths values and judgments and the analyst must solve Smiths problem. Of course, Smiths values and judgments might derive from the expertise of others. Now Smith is not the corporation, and Smiths motivations may not be in agreement with those of his immediate superior or of the corporation president, the board of directors, the totality of stockholders, or of the society at large. An analysis of the same problem from the perspective of Smiths superior, say, might be quite different; and not only might the assessments of probabilities and utilities be different, but the structure of the decision tree might also be different. Still, Smith is the decision maker. In analyzing his problem it might be appropriate to bring into consideration the motivation and feelings of others; but then again it might not be.

To take a slightly different tack, let us suppose that Mr. Smith does not decide on acting but recommends action to be taken by Mr. Jones. The analyst, however, is working for Smith and not Jones, and lets add that Jones cant be bothered with details. Here Smith and his analyst must be concerned not only with what Smith thinks but also with what Jones is likely to do, and the analyst might be wise to carry out an uncertainty analysis of Jones reaction to Smiths specific recommendations. Jn addition, the analyst must help Smith assess preferences for consequences that are a composite of what Smith recommends, what Jones is likely to do, and what actually happens. They may have to incorporate uncertainties about political realities as chance moves in Smiths problem. Of course, all this is more easily said than done.

As a third case, suppose that Smith asks an analyst to study a company problem and suppose that Smith does not think of himself as the decision makerin fact no one seems to want to take ultimate responsibility. Here the identity of the decision maker is in limbo. In some mysterious way a decision will eventually be made, and after everybody learns how it has turned out the identity of the decision maker will suddenly come to the surface. In such a situation the analyst himself may be the fall guy if his recommended strategy turns out to be a poor choice after the fact; and equally, the analyst may not gather any glory even if his strategy turns out well.

We have assumed throughout that the analyst helps organize and structure the decision makers thought process, elicits judgmental information from him and from his delegated experts, checks the internal inconsistencies of judgmental inputs, assists the decision maker in bringing these judgments together into a coherent whole, and finally processes this information and identifies a best strategy for action. Nowhere in these functions is the analyst supposed to inject his own personal views or biases. Of course, this demarcation of the role of the analyst is not always so clear-cut. In some circumstances, for example, the decision maker may ask his analyst to incorporate his own judgments. More importantly than this, however, the analyst can influence the outcome in a myriad of subtle ways: by what he chooses to incorporate in the analysis, how he phrases questions, the grimaces he makes in dialogue with an expert, the tone of voice he uses in an oral presentation, and the issues he may conceal behind a barrage of mathematical mumbo-jumbo. Indeed, in some circumstances it turns out that the analyst is the real decision maker and the alleged decision maker is the front man. ft is therefore crucial for management to comprehend and intimately involve itself in the process of analysis.

A last point is that an analyst may find himself in violent disagreement with the preference structure of his client. He may also think that its better for society if certain misguided individuals are inefficient rather than efficient in their behaviour. Hence the analyst sometimes faces a vexing moral problem: Should he or should he not work for that immoral Mr. Smith?

Where Smith is a paragon of virtue and the analyst shares a common sense of moral values with him, things are perhaps easier. Still, if Smith insists on a critical assessment that the analyst finds really absurd, the analyst is once again in a difficult position. Should he use Smiths judgments? Or should he disassociate himself from Smith lest he be party to an action which might reflect unfavourably on his professional reputation? If the analyst cannot opt out, should lie try to influence the results by the type of analysis he makes? Let me duck this question and merely remark that this is a grey area in which moralistic sermons tend to be a bit too simplistic.

Analyzing the Right Problem

In my first operations research problem, I fell into the trap of working on a wrong problem*. I was given a free hand to investigate how a department store could become more efficient in its sales effort. I very quickly became interested in bringing order out of the chaos that was a daily affair in the womens blouse sub-department. On one counter, in particular, blouses were strewn about everywhere and the poor shopper was beside herself trying to locate her size.

She wasted precious minutes because of the inefficiency of management. How easy it would be to arrange the merchandise neatly, and inaugurate a simple inventory replenishment scheme that would cut down the service times and make an orderly queue possible! After writing what I considered a masterful analysis of the problem I was invited to visit the store at opening time to see how the chaotic melange developed over time. Just before opening time, after the employees had got the entire stock neatly arranged and checked styles and sizes very carefully, they took the blouses out of their boxes, threw them on the counter, and very methodically mixed them up. Things were so inefficiently arranged that half an hour after opening there was a crowd of women milling about the counter, and this crowd, like a magnet, lured other bargain hunters into the melee. I learned. Now I make other mistakes.

In large hierarchical organizations there is often a tremendous organizational gap between the analyst and the decision maker. More than one analyst has made a complete study of the wrong problem because of lack of feedback between the decision maker and the analyst. Usually the analysts work will not have been entirely wasted, but a precise answer to the wrong question is not nearly so desirable as an incomplete answer to the right one. In some circumstances, also, the analyst may isolate the real problem only to find that the decision maker is not sophisticated enough to recognize it, or to find that the decision maker is no longer emotionally involved in the problem and will not meet the analyst halfway. The communications gap must be closed if an effective relationship is to be established between decision maker and analyst. At each stage of an analysis it is critical that one check and recheck to see that the analyst is working on the problem that the decision maker wants him to work on,

In the initial stages of an investigation, the analyst should acquaint himself with the general qualitative nature of the problem. To gain some reasonable perspective and sensitivity for a problem area, he might then write out a few plausible scenarios: If we do this and this occurs, and then if we do this, and . . .He might construct a few non-surprise scenarios, and then some on the pessimistic side and some on the optimistic side. Still in the initial phase, he might construct some fairly crude decision trees, put in ballpark guesses for some of the uncertain quantities that are not very important, indicate the sources of objective data that bear on the uncertainty at each chance fork, list the experts who perhaps know something about the uncertainties at the various forks, and record any apparent conflicts of interest that might distort these experts judgments. He might write out descriptions of the consequences and implications that certain paths through the decision tree would have for the company, its competitors, and society in general. (We can associate each of these more detailed scenarios with a particular path through a decision-flow diagram) At the end of this initial phase and before he assigns any hard numbers, before he probes experts for their judgments, before he begins to worry the decision maker with critical tradeoffs or substitution rates between diverse attributes, before he gives dollar values to intangibles, before he investigates attitude towards risk, and before he makes any tentative analyses of the crude decision tree, he and the decision maker ought to review the analysis thus far to see that it is addressed to the real problem.

The Involvement of the Decision Maker and the Need for Documentation

Management and the analyst should use this preliminary qualitative investigation as a communication vehicle, to get on the same wavelength, as it were. (One company I have consulted for insists on written documentation of this introductory qualitative report.) If a broad, qualitative, comprehensive description of the problem area is available, then it is easier for the decision maker and his analyst to jointly choose the facets of the problem they wish to incorporate in a more formal quantitative analysis, and to set priorities for the analysis of different sub-problems. After they have completed the formal analysis, they can then investigate informally whether certain qualitative considerations that they omitted from the formal quantitative analysis tend to reinforce or to weaken the general conclusions. Documentation is important at each stage of analysis because it serves to facilitate the communication process, to crystallize agreements, to invite constructive criticism from impartial outsiders, and not least of all to train others in the necessary techniques and to record the development of the analysis for the use of other managers and analysts who may be involved with the same problem at a later date.

Finally, a critical ingredient that determines whether or not management will ever implement an analysis is the quality of the involvement of the decision maker in the analytical process. Of course, this involvement can be overdone. It can become too demanding.

Is It Worth the Effort?

People often ask, How do you know whether or not it is worth the effort to make a formal analysis of a decision problem? Is this a decision problem itself? Can you do a decision analysis of whether it is worth doing-a decision analysis? I dont know anyone who can give definitive answers to these questions, and I suspect one runs into a messy and explosive infinite regression if he tries to incorporate considerations of these questions into the formal structure of a decision-theoretic model. Nevertheless we can make some common- sense remarks: If the problem involves millions of dollars, you probably cannot go too far wrong if you spend a few pennies, comparatively speaking, on systematic review and analysis. At the other obvious extreme, it takes a peculiar mentality to justify hiring an expensive consultant to handle a non-repetitive situation in which the monetary issues are paramount and in which the monetary gains of analysis cannot possibly pay the consultants fees.

The situation is a lot trickier in repetitive situations. It might be quite

expensive to analyze the first one of a series of similar problems, but after that subsequent analyses might be routine and comparatively inexpensive form. These issues are also intimately related to other issues we have already discussed in this chapter. Consider, for example, a situation in which a decision maker has three alternative branches to choose amongst at the very start of a decision-flow diagram. Suppose he has formally investigated two of these branches, and the question under review is whether it is worthwhile to formally analyze the third branch. The analyst may believe that in a formal analysis the third alternative will prove to be worse than the best of the other two alternatives, but he may think that even if he is wrong, the possible merits of the third alternative cannot be large enough to warrant the expense of the analysis. In this case, surely, he should merely prune this third branch, unless, of course, he can gain some insight into the evaluation of the third branch by a cruder and less costly mode of analysis. Is it worthwhile analyzing this? Maybe yes, maybe no; it depends. Its better, I believe, to keep such considerations outside the formal theory and handle them in a pragmatic, informal manner.

The Decision to Adopt Decision Analysis

As a member of a business school faculty, I am occasionally asked to lecture on decision analysis to top executives of large corporations. Invariably these executives ask if I can cite some success stories and some stories of failures. A few years ago I was rather hard pressed to furnish them with meaningful sketches but my repertoire has grown with the growing number of companies that are now adopting these techniques. After a while, one of the executives is sure to ask, How should I decide whether or not this is some fad that will do more harm than good? Im not worried about the costs of analysisthats the least. However, I am worried about getting sucked into making a really stupid error. Would decision analysis, or whatever you call it, have prevented Ford from making their terrible mistake with the Edsel? Could you analyze whether or not I should give orders to adopt these techniques in my company?

It is tempting to say that our method couldnt have cost any more. Seriously, however, one clearly cannot answer a question like this in very definitive terms. What is called for is a bit of weaseling. At one lecture I gave, an executive in the audience gave a good answer to a question very similar to the one above, and since that time I have often quoted his remarks with a few modifications of my own. He said, My company feels these ideas are too new and too radical to use today on really big decisions. But these ideas are too promising to ignore altogether. We are encouraging our management to experiment with them and to actually carry out decisions based on these analyses for selected medium-sized problems in some departments where middle management feels that it makes sense to do so. We are keeping tabs on these developments. At the same time, we are monitoring some really important decisions on paper, insofar as that is possible, to see what decisions our management comes up with using our old seat-of-the-pants techniques and what suggested strategies come out of these more formal procedures. As yet, we dont allow the formal procedures to contaminate our intuitive analyses of major problems, but after a decision has been made we sometimes like to compare notes with those fellows who have been formally analyzing the problem on the side. When theyre way off they complain they are not privy to the counsel of the top people, but when theyre right they sometimes can raise some embarrassing questions. I think well adopt these techniques for some big problems in the future. Which ones these are will largely be determined by our internal politics and personalities, and by the luck of the draw. I think this is a good answer; perhaps a bit weak, but I suppose anything more requires a great deal more detail of specific circumstances.

You cant get around it, though: So long as men engage in big activities they will occasionally make costly mistakes. We can hope to reduce these costs and their frequency, and increase the benefits of the successes.

6. PROS AND CONS OF DECISION ANALYSIS

The systematic approach of decision analysis has its merits and demerits, and vivid testimony appears on both sides of the ledger. Also, what is a merit to some is a demerit to others. Someone might wax eloquent and say, Decision analysis is great because it encourages the introduction of subjective judgments and preferences into the formal analysis. But others might retort, Thats a disadvantage, as I see it. Managers can now legitimatize their prejudices and misconceptions. It is true that dwelling on potential pitfalls and citing stupid abuses can undermine almost any list of favourable features, and on the other hand there are those who can see the brighter side of any sordid picture, who can even see the civilizing effects of war. Be this as it may, here are some of my rough evaluations on my score card. Obviously, of course, my tally comes out in favour of decision analysis; otherwise I should never have had the ambition and motive to give these lectures.

The Favourable Side

- The methodology of decision analysis encourages the decision maker to scrutinize his problem as an organic whole. The systematic approach forces him to come to quantitative grips with the interactions between various facets of his problem.

- The systematic approach helps communication. It allows each expert to give testimony about his area of expertise in an unambiguous quantitative manner, testimony that can be incorporated in the overall analysis.

- Systematic examination of the value of information in a decision context helps suggest the gathering, compilation, and organization of data from new sources.

- Analysis distinguishes the decision makers preferences for consequences, including his attitudes towards risky situations, from his judgments about uncertainties.

- Analysis serves as a stimulus for the decision maker and 1iis staff to think hard, at the time when it counts, about new, viable, alternative actions.

- A hard analysis helps the decision maker emphasize the point that the decision has not been made on frivolous grounds; he can use it to communicate the rationale of his adopted strategy and rally support for it. Analysis helps put the arguments of an opposing point of view in perspective. Yes, these factors are cogent but we incorporated them in our analysis and found that they were outweighed by consideration of this, this, and this. By the same token, if the factors have not been included, this is immediately laid bare.

- The methodology of decision analysis is useful as a mediating device in situations in which the advisors to a decision maker disagree about an appropriate course of action, provided that the advisors are men of goodwill who want to get at the heart of the matter and are not concerned with dysfunctional polemics. By decomposing the problem into its basic parts, they can quickly focus on those issues on which they have fundamental disagreements. Even though these advisors might not be able to agree on a course of action, they might be able to agree on the qualitative structure of the problem, or perhaps on the evaluation of consequences, or on the assessments of probabilities. Hopefully they might even agree on why they disagree. If they disagree on assessments of probabilities, are they all privy to a common pool of information? Can they agree that certain data gathering, sampling, and experimentation will furnish objective information that will bring them closer together? This process of deliberation need not necessarily culminate in sweet agreement (indeed, it may heighten differences), but it surely will sharpen the specificity and sophistication of their arguments and may engage them and the decision maker in the kind of constructive dialogue that will bring them all to grips with the complex issues of his problem.

- Systematic analysis provides a framework for contingency planning and for the continuing evaluation of new facts that is necessary as the dynamics of a problem unfold. Not only does it suggest which alternative action should be chosen presently, but it suggests what could happen in the future and prepares a rationale for ensuing action. It provides a framework for continuous revaluation of a decision problem that has a distant time horizon. The documentation of an analysis can serve as a briefing report for a new decision maker or staff man who is assigned to a problem area at a time when the denouement is still in progress. This documentation can also provide a dated record of expert testimony that can be used for calibration purposes; for example, Jones record in the past is better than Smiths, so perhaps Jones should have more influence now, all other things being equal. (Of course, this type of calibration might find divisive uses in an organization, and prudent executives should balance the benefits against the liabilities.)

The Unfavourable Side

- In hearings before a subcommittee of the Committee on Appropriations, House of Representatives, Eighty-ninth Congress (second session), Admiral Rickover unmercifully criticized the narrow advocates of cost-effectiveness studies. Since his statements are eminently quotable and apply with equal vigour to the methodology of decision analysis, let us start the testimony on the unfavourable side of the ledger by seeing what he has to say.

On a cost-effectiveness basis the colonists would not have revolted against King George III, nor would John Paul Jones have engaged the Serapis with the Bonhomie Richard, an inferior ship. The Greeks at Thermopylae and at Salamis would not have stood tip to the Persians had they had cost-effectiveness to advise them, or had these cost-effectiveness people been in charge.

Since the calculations are extensive and complex, the experienced people in positions of management responsibility do not have the time or the detailed understanding to review them. Judgment as to the weight that should be given to various factors in the analysis is left to the analyst himself instead of to the judgment of people who have experience in the field that is being analysed.

The basis for using cost-effectiveness studies as the rationale on which to make a decision is the assumption that the important factors can be expressed in numerical form and that a correct judgment of the situation can then be calculated mathematically. But for most complex situations this is an unrealistic assumption. Frankly, I have no more faith in the ability of the social scientists to quantify military effectiveness than I do in numerologists to calculate the future...

Considerations which cannot be quantified are necessarily left out of the calculationin my opinion the ability of the social scientists to calculate numerical values for military effectiveness is even less than our ability to calculate a numerical basis for many of the engineering decisions we are forced to base on judgment, experience, and intuition. To make the correct engineering decisions requires extensive knowledge and experience in engineering. Mathematical ability alone will not suffice. .

(Who knows how much informal cost-effectiveness analysis the colonists, or John Paul Jones, or the Greeks actually did? Perhaps formalizing their analysed might have led them to other choices, less desirable ones after the fact; and perhaps not. But what does this prove? Historians can provide us with loads of examples, on the other side, examples of actions that were undertaken for highly emotional or mystical reasons or on the basis of wishful thinking, that have resulted in disastrous consequences which any reasonable, systematic analysis could have foreseen. Would Napoleon have tried to carry out his grand scheme of conquering Russia if he had given full weight to the Russian climate and geography? Still I believe Admiral Rickover is quite correct to caution against relinquishing control of a military study to analysts (social scientists or mathematicians) who are not themselves men of experience in military matters. Certainly a poor analysis can be far worse than no analysis at all. I suppose I differ with the Admiral about the extent to which it is possible to quantify intangibles. After all, if he prefers one bundle of intangibles to another bundle of intangibles, as indeed he seems to do, he has started already on the road to quantification).

- The spirit of decision analysis is divide and conquer: Decompose a complex problem into simpler problems, get ones thinking straight in these simpler problems, paste these analyses together with logical glue, and come out with a program for action for the complex problem. Experts are not asked complicated, fuzzy questions, but crystal clear, unambiguous, elemental, hypothetical questions. The trouble is that *these basic questions are the most diffiicu1t to answer*, and many decision makers shudder at the idea of thinking about these starkly simple, hypothetical situations. Indeed, in some circumstances it would be political suicide for an administrator to disclose how he would choose in a classically simple situation. Often he needs to take refuge in the complexity and fuzziness of real-life situations. It is true that one can often impute basic values and judgments to a decision maker on the basis of his revealed choices in complicated situations, but one can never be quite sure whether he chose A because of consideration of X or Y, or Z.

- Decision analysis requires the explicit articulation of a thought process. A decision maker may be able to grapple unconsciously with a myriad of interconnected considerations, but if he is forced to give a verbal description of his thought processes, it may appear that he is much more restricted in the complexity of his analysis than he really is in practice. The human brain can be a magnificent synthesizer of disparate pieces of nebulous information, and often formal techniques and procedures thwart and inhibit this mysterious mechanism from operating efficiently.

- Many critics of formalized, systematic analysis suspect that the breed of individuals who elect to go into this sort of work lack heart; that they are so concerned with putting numbers on everything that they bias a study in a direction that leaves out many human and artistic qualities and that analysis therefore inhibits creativity. Everything is reduced to dollar signs or lives saved. But what about the quality of life? Sure, you say that this could be scaled and incorporated into the analysis, but do you do it? No. The methodology you espouse seems to narrow the focus and the hard tends to drive out the soft, even though the soft might be far more important in the long run. You seem always to feature those aspects of the problem that are readily amenable to analysis and to ignore like the plague those intangibles that really count.

(I dont disagree fully with this accusation: Much more attention should be given to quality of life, and measurement techniques should be developed to (statistically) harden the soft. It is heartening to note the rising interest of some of our governmental representatives and academics in the development of a series of social indicators* designed to measure quality of life.)

7. ONE LAST REMARK

Even if you dont analyze your decision problem by the methodology described in these lectures, you still must act. What will you do? In my personal opinion, one part of the justification for adopting the methodology of decision analysis is that the underlying behavioural assumptions are appealing; a second part of the justification is that this methodology is an operational mode of analysis (at least for many problems, and the class is widening); and the final part of the justification is, What would you do otherwise?

* See (1) Bauer, Raymond A. (Ed), Social Indicators, The M.I.T. Press, 1966, and (2) Social Goals and Indicators for American Society, The Annals (of the American Academy of Political and Social Science), Volume I (May 1967) and Volume II (September 1967)

---

[1] I acknowledge the role of many colleagues in informing this discussion. In particular, in recent months I have divined and distilled much about public investment in research and risky decisions from discourse with Bob Douglas, Bill Fisher, Chris Langford, James Heath, Anthos Yannakou and Gavan Dwyer (Victorian DPI);John Mullen, Garry Griffith (NSW DPI); Mark Burgmann Melbourne University); and Julian Alston (University of California)..They are not to be blamed though.