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## RESEARCH NOTES AND COMMENTARIES

# FURTHER REFLECTIONS ON THE ELIMINATION OF FRAMING BIAS IN STRATEGIC DECISION MAKING

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*Wright and Goodwin (2002) maintain that, in terms of experimental design and ecological validity, Hodgkinson et al. (1999) failed to demonstrate either that the framing bias is likely to be of salience in strategic decision making, or that causal cognitive mapping provides an effective means of limiting the damage accruing from this bias. In reply, we show that there is ample evidence to support both of our original claims. Moreover, using Wright and Goodwin's own data set, we demonstrate that our studies did in fact attain appropriate levels of ecological validity, and that their proposed alternative to causal cognitive mapping, a decision tree approach, is far from 'simpler.' Wright and Goodwin's approach not only fails to eliminate the framing bias—it leads to confusion.* 

## INTRODUCTION

In recent years there has been much scholarly attention devoted to the application of theory and research from the field of behavioral decision making to the analysis of strategic decisions. This body of work suggests that when confronted with complex problems, involving risk and uncertainty, strategists adopt a variety of heuristic modes of thinking and reasoning, in an effort to simplify the processes of judgment and decision making. In so doing, however, they are also susceptible to a number of errors and biases (for recent reviews see Das and Teng, 1999; Hodgkinson, 2001a; Maule and Hodgkinson, 2002; Schwenk, 1995). To the extent that this is the case, a logical corollary is that decision-aiding techniques should be developed with a view to debiasing decision-makers' judgments, thereby enhancing the quality of strategic decisions (see Arkes, 1991).

One such bias, known as the framing bias (Tversky and Kahneman, 1981), arises when trivial changes to the way a decision problem is presented, emphasizing either the potential gains or potential losses, leads to reversals of preference, with decision-makers being risk averse when gains are highlighted and risk seeking when losses are highlighted. In the context of strategic decision making, previous research has established that both undergraduate and experienced decision-makers are susceptible to this bias (Bateman and Zeithaml, 1989). Building on this work, we recently reported two experimental studies (Hodgkinson

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*et al*., 1999), to establish: (1) whether or not the effects of framing, previously observed using simple decision problems, could be replicated using relatively elaborated scenarios, in which strategic decision choices are embedded in complex background information; (2) the extent to which such bias could be eliminated, or attenuated, using a form of causal cognitive mapping (Axelrod, 1976; Huff, 1990).

Our studies were motivated by a desire to address criticisms concerning work on heuristics and biases voiced from within the fields of strategic management and behavioral decision making. As observed by Maule and Hodgkinson (2002), an overdependence on simple laboratory studies might limit the usefulness of behavioral decision research as a basis for understanding more complex decision making in the real world. Conversely, an over-reliance on the secondary analysis of documentary evidence to establish whether heuristics (and attendant biases) are evident beyond the confines of the laboratory (as has been the case in the field of strategic management) might limit the confidence that can be placed in the findings. Typically, the documents employed for these purposes have been prepared for particular audiences (often to influence the perceptions of selected key stakeholders) and, as such, may not truly reflect executives' strategic thought processes (see also Hodgkinson, 2001a). Our overall aim was to develop an experimental approach for investigating framing effects that addressed these limitations, thereby generating robust findings of high relevance to the world of practice.

We presented elaborated scenarios under controlled, experimental conditions to samples of novice and experienced participants. Our findings confirmed our basic predictions. In both of the reported studies we established robust framing effects, which were subsequently eliminated through our causal cognitive mapping intervention.

Wright and Goodwin (2002) have attempted to partially replicate our studies, and in so doing have been critical of our lines of reasoning and the way in which we interpreted our findings. These criticisms can be broadly grouped into two major themes. The first concerns the conduct of rigorous, ecologically valid research, i.e., research that not only satisfies the requirements of theoretical and methodological rigor, but is also practically relevant. The second concerns the use of cognitive mapping techniques (and other procedures) as tools for debiasing, i.e., the underlying mechanisms through which debiasing occurs. In this reply, we show that Wright and Goodwin's criticisms of our work are found wanting and, therefore, that our original conclusions stand.

## THE CONDUCT OF RIGOROUS, ECOLOGICALLY VALID RESEARCH

### Steps taken to enhance the ecological validity of our findings

As noted above, a primary aim of our studies was to enhance the ecological validity of behavioral decision research, and in so doing to increase its relevance to the field of strategic management. According to Wright and Goodwin (2002) we failed to achieve this aim, due to the fact that the experimental materials we employed in our studies were not representative of real-life strategic problems. In considering this issue, we must be mindful of the fact that ecological validity is a multifaceted, multidimensional construct. In the present context, there are several ways in which ecological validity might be enhanced. First, experimental tasks might be designed to resemble more closely the various aspects of strategic decision problems as they naturally occur, beyond the confines of the laboratory. Second, participants might be selected whose background characteristics (education, training and work experience) approximate more closely those of experienced strategic decision-makers. Our studies were carefully designed with both of these facets of ecological validity in mind.

Conventional studies of the framing bias have typically employed relatively simple decision problems, comprising circa 30–50 words, presented to psychology undergraduates, epitomized by the 'Asian Disease Problem' (Tversky and Kahneman, 1981), the example that Wright and Goodwin (2002) employ to illustrate various aspects of the framing phenomenon. In marked contrast, our own approach entailed the use of enriched case vignettes (Study 1 circa 500–700 words in length, Study 2 circa 1600–1700 words) concerning complex strategic dilemmas, based upon carefully researched, real business problems. In addition, in Study 1 we recruited advanced undergraduate management studies students who possessed a detailed conceptual knowledge of the strategic management field, reinforced through extensive

classroom experience in the analysis of strategic management case studies, similar to those used in the study. Study 2 participants were practicing senior managers, drawn from a large financial services organization. Moreover, the materials employed in our second study were developed in collaboration with senior personnel and were based on a current strategic dilemma confronting this organization. Our participants were thus highly familiar with this 'live' problem. In comparison to the wider field of behavioral decision making, ecological validity was thus enhanced both in terms of richness of the information presented and by ensuring that the decision problems were highly meaningful to the participants concerned.

## Taking stock of Wright and Goodwin's objections to our research design and materials

We contend that the above refinements represent a major methodological advance. Nevertheless, Wright and Goodwin (2002) highlight two specific features of our research design and materials, which they maintain limit the inferences that can be drawn in respect of our data.

First, they claim that strategic decision-makers rarely evaluate options in terms of probabilities and pay-offs. Rather, they 'try to control risk by gathering information, generating new options, delay, or passing responsibility to others' (Wright and Goodwin, 2002: 1065). This implies that we were misguided to have presented the outcomes associated with the strategic alternatives in terms of probabilities of gains and losses. Yet Wright and Goodwin's own data, presented in the Appendix of their commentary, seem to contradict the validity of this assertion. In response to questions about the extent to which a number of factors are key components of the strategic decision process (1 = 'minor component,' 7 = 'major component'), those factors reflecting probabilities and pay-offs yielded mean responses well above the scale midpoint ('probability of loss,' mean = 5.39; 'probability of gain,' mean = 5.48; 'amount that could be lost,' mean = 5.50; 'amount that could be gained,' mean = 5.40), suggesting that they are in fact important in the context of strategic decision making. Crucially, the mean responses associated with these items are generally higher than the mean responses associated with the various factors that Wright and Goodwin consider to be of greater salience—i.e., the avoidance of the risks by: (1) 'gathering further information' (mean = 5.54); (2) 'creating new decision options' (mean = 4.84); (3) 'delaying the decision' (mean = 3.89); (4) 'passing responsibility to others' (mean = 3.51); and (5) 'the creation of new strategies that perform well no matter what the future may hold' (mean = 4.71). Hence, Wright and Goodwin's own data actually support our contention that our experimental materials exhibited reasonably high levels of ecological validity, and negate their assertions regarding which factors are important in strategic situations.

Second, Wright and Goodwin argue that we were misguided to have changed the response mode associated with Study 2 from 'choice' to 'judgment' (Wright and Goodwin, 2002: 1061). Although they cite evidence showing that changes in response mode affect the way that individuals process information in general (Payne, Bettman, and Johnson, 1993; Tversky, Sattath, and Slovic, 1988), they fail to consider the implications of this work for framing in particular. Interestingly, a recent meta-analysis of framing effects (Kuhberger, 1998), which compared alternative response modes, revealed that in fact such effects do occur when judgment tasks are employed, albeit with smaller effect sizes in comparison to conventional choice tasks. The fact that we were able to replicate framing effects across choice *and* judgment tasks, therefore, is less remarkable than Wright and Goodwin imply. However, the real issue at stake here is the reason why we were motivated to change the response mode from choice to judgment. In developing the materials for our second study it became clear very early on in our dialogue with the Senior Management Team that the strategic dilemma upon which our materials were based would be modeled more appropriately as a judgment rather than a choice task. In this way we were able to enhance further the ecological validity of our experimental procedures.

## The central issue

At the heart of this debate is a more fundamental problem concerning the conduct of research that is directed toward the development of knowledge that is methodologically and theoretically sound, but also of applied relevance (Hodgkinson, 2001b; Huff, 2000; Pettigrew, 1997; Tranfield and Starkey, 1998). In conducting the research reported in Hodgkinson *et al*. (1999) our ultimate aim was

to respond to the challenge issued by Schwenk (1982) to generate findings that sacrificed neither rigor nor relevance.

Recently, Anderson, Herriot, and Hodgkinson (2001) have devised a simple $2 \times 2$ framework that is useful in the context of this debate. They contend that research characterized by high rigor at the expense of relevance (as exemplified by many laboratory studies) reflects 'Pedantic Science.' Arguably much of the work in behavioral decision making that has been extrapolated to the field of strategic management falls into this category. 'Popularist Science,' by contrast, reflects work that is high on relevance at the expense of rigor. Clearly, much of the work using cognitive mapping and scenario planning as a basis for organizational intervention that has emerged over recent years falls into this category. As observed by Hodgkinson and Herriot (2002: 56–57): 'reliability and validity issues associated with these procedures have received scant attention, and virtually no systematic studies have been undertaken in order to formally evaluate their efficacy for use in practical settings' (see also Hodgkinson, 2001c; Hodgkinson, Herriot, and Anderson, 2001; Hodgkinson and Sparrow, 2002). If research is to have an enduring impact on the world of practice, it must be characterized both by high rigor *and* high relevance. Work that meets these twin imperatives has been termed 'Pragmatic Science' by Anderson *et al*. (2001) and it is this type of work to which we aspired. It is most unfortunate, therefore, that Wright and Goodwin should have focused almost exclusively on the relevancy dimension in their analysis and critique of our studies. In so doing, they are at risk of pushing the strategic management field ever further in the direction of Popularist Science. Such work satisfies no one, leading ultimately to an increase in Puerile Science, the inevitable outcome of 'promoting faddish techniques that have been prematurely foisted upon organizational systems' (Dunnette, 1990: 21).

## THE USE OF COGNITIVE MAPPING TECHNIQUES (AND OTHER PROCEDURES) AS TOOLS FOR DEBIASING

### Lack of a theoretical rationale?

Wright and Goodwin (2002) unfairly criticize our original article for failing to present an underlying theoretical explanation as to why cognitive mapping might overcome the framing bias. In our article we refer to Kahneman and Tversky's (1984: 344) suggestion that one way of debiasing is for decision-makers to adopt procedures that lead to equivalent versions of problems being transformed into the same canonical representation (Hodgkinson *et al*., 1999: 978). In point of fact, there is a paucity of research specifying what this involves and how it might best be achieved. However, we reasoned that it must entail developing a mental representation of the problem that goes beyond its surface structure, and that by asking participants to reflect upon their formulation of the problem, an activity that is an integral part of causal cognitive mapping, we might facilitate the required change in cognition (cf. Huff, 1990: 16).

We also argued that reframing the problem in this way would involve effortful thought, citing previous research showing that such activity is associated with a diminution of framing effects (Sieck and Yates, 1997; Smith and Levin, 1996; Takemura, 1994). Moreover, later in our article, we also acknowledged that effortful thought might not always act positively to eliminate biases in strategic decision making. On the contrary, it may actually increase some of them (Hodgkinson *et al*., 1999: 984).[1] Our studies were designed to begin addressing these hitherto neglected issues, not to provide a definitive solution to them.

### Reevaluating Wright and Goodwin's experimental evidence

As predicted, our results showed that mapping prior to decision making reduced or eliminated the framing bias, both for 'novice' and 'expert' decision-makers.[2] Wright and Goodwin (2002) conducted a study that they claim provides an explanation of our findings. They adopted a very similar research design to ours, using the same strategic problem that we employed in our first

[1] Recently, we have extended this line of reasoning, drawing upon a range of theoretical arguments including Janis and Mann's (1977) conflict theory of decision making (see Hodgkinson and Maule, 2002).

[2] Wright and Goodwin (2002: 1062) claim that our 'respondents were given no special training in causal mapping.' In fact, this assertion is not correct. All participants in both studies were trained in causal mapping, using a simplified problem that was drawn from a domain unrelated to the focus of our research, a fact that we did not report in our original article, due to restrictions on length.

study. However, they replaced our causal cognitive mapping intervention with 'a simple device designed to encourage subjects to think harder' (Wright and Goodwin, 2002: 1062). Participants were presented with a decision tree representation of the strategic choice and asked simply to write in the monetary values associated with the various outcomes. In keeping with the findings of our own work, they showed, using a sample of undergraduate participants, that if this activity is undertaken prior to choice it eliminates the framing bias. From this they concluded that 'cognitive mapping was achieving its success by simply requiring subjects to think harder and longer about their choice. There is nothing inherent in the method itself that overcomes framing bias and the same effects can be obtained using much simpler devices' (Wright and Goodwin, 2002: 1062). If this conclusion were valid, then it would suggest that we might facilitate strategic decision making simply by ensuring that the individuals concerned think harder and longer about the problem, making more resource-intensive decision-aiding procedures, such as causal cognitive mapping, unnecessary.

However, we believe that Wright and Goodwin's interpretation of their own experimental evidence needs to be treated with extreme caution. Their intervention may not be as straightforward as they claim, since there are likely to be crucial differences in the ways in which individuals applied the decision tree procedure across the different versions of the decision problem. In the case of the positively framed version of the problem, completing the decision tree is relatively simple, since the outcomes that need to be written down are exactly as presented in the accompanying description of the problem (£1 million for the safe option, £3 million or £0 million for the risky option). However, this activity is not so straightforward when applied to the negatively framed version of the problem, since outcomes are described with reference to target profits (£2 million below target for the safe option, on target or £3 million below target for the risky option). How should participants represent these outcomes on their decision trees? Consider the safe option, for instance. Should participants write down £2 million below target, −£2 million, +£1 million or something else? In short, we were unsure what should be the appropriate response to the negatively framed version of the task.

Wright and Goodwin kindly made available their participants' decision tree responses from their first study, which we reanalyzed, to help clarify this point. The findings were most revealing. We were able to classify their participants' decision tree responses into four types. Some participants responded by including the values provided in the positive version of the problem: £1 million for the outcome associated with the certain alternative and £3 million and £0 million for the two outcomes associated with the risky alternative (1, 3, 0). Other participants either left the decision tree completely blank (Blank), included some information but did not complete the tree (Incomplete), or completed the tree using some other formulation (Other). No two formulations in the Other group were identical. Indeed, responses included outcome values as diverse as (1, 3, −3), (2, 3, 3) and (1, 3, .9). Table 1 reports the frequencies of these formulations for participants choosing the safe or risky alternatives, when presented with the positively or negatively framed versions of the problem.

When presented with the positively framed version of the task, 73 percent of participants modeled the problem by enumerating outcome values in the decision tree structure identical to those presented in the problem description (1, 3, 0). Only 9 percent of participants left the decision tree blank. When presented with the negatively framed version of the task, by contrast, 23 percent of participants enumerated the outcome values as presented in the positively framed version of the problem description, 41 percent failed to complete the decision tree at all, the remainder (36%) enumerating a wide variety of alternative outcome value formulations. We believe that this pattern of responses mirrors

Table 1. Frequency of the four types of response to the decision tree outcomes (see text for explanations of these types) for participants choosing the risk averse (RA) or risk seeking (RS) options in response to either the positively (+) or negatively (−) framed versions of the problem

| Problem version | Choice | Response type | | | |
|---|---|---|---|---|---|
| | | 1, 3, 0 | Blank | Incomplete | Other |
| + | RA | 12 | 2 | 1 | 1 |
| + | RS | 4 | 0 | 0 | 2 |
| − | RA | 5 | 3 | 1 | 3 |
| − | RS | 0 | 6 | 0 | 4 |

Source: reanalysis of data reported by Wright and Goodwin (2002).

our own confusion regarding how to complete the decision tree in response to the negatively framed version of the problem description. Indeed, a significant minority of participants appears not to have resolved this confusion, leaving the decision tree blank. Given this pattern of findings, it is difficult to know what effect, if any, the decision tree intervention had on those participants exposed to the negatively framed version of the problem.

What is particularly striking is that 23 percent modeled the negative version of the problem as if it were positively framed and all of these participants chose the risk-averse option. The responses of these individuals are sufficient to explain the reported elimination of the framing effect when using the decision tree. In their Table 1, Wright and Goodwin showed that preferences for the risk-averse alternative in response to the positively framed version of the problem are almost identical with and without completing the decision tree (73% and 68% respectively), suggesting that the decision tree intervention had little effect here. The crucial changes occur in response to the negatively framed version of the problem, where there is an increase in risk-averse choices when required to complete the decision tree (55% vs. 36% with and without the decision tree requirement respectively). This change can be accounted for by those participants who resolved the confusion about how to complete the decision tree under the negatively framed condition by modeling it as if it were a gain problem. Thus, in resolving the confusion some participants have apparently reformulated the problem as a gain problem and then fallen foul of a positive framing bias! This does not appear to be an example of debiasing, so much as a change in bias.

In the case of our own intervention, by contrast, based on causal cognitive mapping, there are no a priori reasons why drawing cause maps should be qualitatively different across the two versions of the problem—a conclusion supported by our preliminary analysis of the causal cognitive maps, which indicates that they are broadly similar in content and structure, following exposure to the positive or negative versions of the problem (Hodgkinson and Maule, 2002). Thus it seems unlikely that the reduction in the framing bias that we reported could be attributed simply to confusions in applying the method, leading some participants to remodel the problem as a gain, thereby exhibiting a positive framing bias, as per Wright and Goodwin's participants.

The confusion arising in respect of the interpretation of Wright and Goodwin's results highlights a more fundamental issue. Like a number of behavioral decision researchers seeking to eliminate, or attenuate, the framing bias (e.g., Arkes, 1991; Russo and Schoemaker, 1989), Wright and Goodwin have failed to specify a clear theoretical rationale in respect of the underlying mechanism(s) through which their intervention procedure might work. It is this lack of underlying theoretical precision, together with the inadequacies associated with their intervention procedure, that renders their data ambiguous.

In the final analysis, we believe that the only way that we can ultimately be sure that debiasing has genuinely occurred is to assess directly the decision-maker's representation of the problem at the moment of choice. To this end, Maule (1989) has used concurrent verbal protocols to assess participants' decision frames. He has shown that decision-makers who frame problems in terms of either gains or losses exhibit framing bias, whereas those framing in terms of both gains and loses do not. As our more recent work has demonstrated (Hodgkinson and Maule, 2002), causal cognitive mapping might perform a similar function, thereby providing much-needed insights into the cognitive changes necessary for debiasing to occur and how these changes might best be achieved.

## CONCLUDING REMARKS

In a field in which replication and extension research is greatly needed, yet rarely undertaken (Hubbard, Vetter, and Little, 1998), we welcome Wright and Goodwin's critical interest in our work. If the strategic management field is to develop tools and techniques that are of practical utility, it is vital that the evidence base supporting such developments is amassed in such a way that outcomes other than those intended are both allowed for and tested, through rigorously designed studies (Hodgkinson and Herriot, 2002). Such falsification attempts are the bedrock of scientific method (Popper, 1962). Unfortunately, however, as demonstrated above, the way in which Wright and Goodwin (2002) have set about this task is found wanting, on a number of fronts.

While causal cognitive mapping, and related procedures such as scenario planning, are gaining increasing popularity as a basis for intervening

in the strategic management process (e.g., Eden and Ackermann, 1998; Fahay and Randell, 1998; van der Heijden, 1996), as noted by Hodgkinson (2001c), there is a dearth of empirical evidence concerning the extent to which their use in fact yields positive benefits for the organizations concerned. (For a counter-veiling instance see Hodgkinson and Wright, in press). In the final analysis, as acknowledged in Hodgkinson (2001c) and Hodgkinson and Sparrow (2002), the work reported in Hodgkinson *et al*. (1999), and subsequently extended by Hodgkinson and Maule (2002), represents but the first steps to address this vitally important issue. Nevertheless, the implications of our initial findings, as reported in Hodgkinson *et al*. (1999), are clear. Our data support the use of causal mapping as an aid to strategic decision making, showing that such intervention can be justified on the grounds that this technique helps to overcome framing biases of the sort identified by behavioral decision researchers, under controlled, experimental conditions, and subsequently replicated, using experienced strategic decision-makers.

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