POSTEVENT INFORMATION AND THE IMPAIRMENT OF EYEWITNESS MEMORY: A METHODOLOGICAL EXAMINATION

A thesis, submitted in partial fulfillment of the degree of Master of Arts (Psychology), in the Faculty of Social Science and Humanities, University of Cape Town, 1989,

by

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ACKNOWLEDGEMENTS

Several people have made this thesis possible, to whom I wish to express my gratitude.

My supervisor, Peter du Preez, contributed in no small measure to the inspiration that initiated the work, and to the perseverance that was required to complete what seemed to be a never-ending matter. Thank you.

To my brother Paul, for introducing me to the South African Law Reports, and focussing my attention on the legal aspects of the work reported here, I owe a special word of thanks.

Carolyn Standing contributed in important, (thankfully) non-academic ways to motivation often sorely needed, for which I am grateful.

The Human Sciences Research Council kindly provided financial assistance in the first year of study toward the degree. I wish to express my gratitude towards that body.

Finally, a word of thanks to the many people who contributed in much smaller, but important, ways to the final product, in seminars, tutorials, informal discussions, and so on: Michelle Slone, Susan Lea, Frank Bokhorst, Clare Timoney, Graham Perlman (among others) - thank you.

ABSTRACT

Recent work in the cognitive psychology of memory suggests that misleading information may permanently alter memory for an event. This work, which takes much of its impetus from the prospect of applying itself to the legal question of eyewitness evidence, has recently come under severe criticism. McCloskey & Zaragoza (1985a, 1985b) provide evidence to suggest that the experimental design used by almost all relevant studies is seriously flawed, and that results which appear to indicate the deleterious effect of misinformation on memory are artefactual.

An analysis of the misinformation paradigm is presented here, with particular attention being paid to the claim of artefactuality. Two lines of approach are adopted in the analysis. In the first, the misinformation paradigm is assessed for its theoretical basis. The notion of 'application' that informs the paradigm is subjected to conceptual scrutiny, and the body of research that constitutes the paradigm is reviewed in terms of its applied orientation. In the second line of approach, the claim of artefactuality is investigated directly. Three methods are devised to test the claim of artefactuality. In two of these, post-hoc analyses are performed, one of which suggests that the claim of artefactuality is incorrect in at least some respects. The third method is constituted by an experiment which submits the claim of artefactuality to exhaustive empirical test. The results of the experiment support the claim that findings of memorial alteration are artefactual.

The two lines of approach are united by showing how the experimental work developed out of the applied basis of the paradigm. It is argued that the inadequacies in the experimental design reflect the impoverished theoretical basis of the research. It is further argued that the question regarding the effect that false information has on memory for an event is one that is still eminently worth pursuing. A few preliminary remarks are made regarding applied considerations relevant to this pursuit.

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CHAPTER ONE: INTRODUCTION

In this dissertation I devote most of my attention to a methodological problem. The problem, which might be better described as a dispute, stems from an applied research paradigm, and concerns the validity of an experimental design used in the paradigm. I have attempted to resolve the dispute here, and the arguments that I deploy are usually derived either directly from experimental work, or from methodological considerations of previously reported research. The dissertation is thus a treatise in experimental psychology, but I certainly do not limit myself in it to experimental considerations. The dispute under consideration has important implications for the notion of an 'applied psychology', and for issues relating to the 'external' and 'ecological' validity of research, which are issues that I shall address in some detail.

But for the meanwhile the best place to begin is by having a preliminary look at the problem that motivates the dissertation.

The problem stems from a contemporary research concern with people who witness crimes (or other events of legal interest), and who are frequently called upon to deliver testimony about their observations. Accurate testimony by such 'eyewitnesses' usually requires that they retrieve their original observations from memory, which is not always an easy feat. Several things may adversely affect this ability. The case of particular interest here is that witnesses to an event may be exposed to information about the event they witnessed some time after the occurrence of the event, which may affect their ability to recall the original information correctly. Examples of this 'postevent information' are newspaper reports about the event (if the event attracts such reports), reports of other witnesses to the event, and suggestions put to witnesses during police or court interrogation. Witnesses will not necessarily be exposed to postevent information, but what happens to their memory for the original event in cases where they are exposed to postevent information is intuitively an interesting question. It is also quite obviously an interesting - and important - question from the point of view of legal practice, where the testimony of witnesses often has to be formally evaluated. Judge Boshoff, a judge of the Supreme court of South Africa, made the following observation in one of the cases brought before him, *apropos* of a consideration of some of the factors that affect the reliability of identifications:

> ...perhaps most crucial of all [the factors that affect the reliability of eyewitness identifications] is the extent to which the witness' original impression has been overlaid by subsequent suggestion and imagination. If a witness is shown a person who is alleged to have been the criminal, he is very likely to make a subconscious substitution of that person's features for those which he actually observed....The same process can happen if the witness is shown a photograph of the accused, or if it is suggested to him that the person whom he saw had certain features. (translated and paraphrased in Hoffman & Zeffertt, 1983: p 481; originally from R. v. Mputing, 1960[1]).

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1. R. v. Mputing 1960 (1) S.A. 785 (T)

The interesting question about postevent information, legally, is whether *inconsistent*, or *misleading* postevent information will interfere with the ability of the witness to remember what he/she originally perceived, and thus, the reliability of the testimony delivered by the witness. It is this question of the effects of postevent information that interests me here. In particular, I will be concerned to examine how psychological research has envisaged the problem, and how it has addressed it empirically.

The reliability of eyewitness identifications is an important question, and one that seems tailored for cognitive psychological investigation. Indeed, psychologists have concerned themselves with the problems of eyewitness testimony since the beginning of the century. The last decade, though, has seen an explosion of research on questions relating to problems with eyewitness reports. In particular, a substantial research paradigm has grown around the question of postevent information and its effects on eyewitness memory. By mid-1986 more than 50 experimental studies had been published, most of these after 1978. The most prolific research psychologist in the area is undoubtedly Elizabeth Loftus, of the University of Washington, and the paradigm exists largely as an extension, and recently, criticism, of a methodology devised by her and her coworkers.

Loftus' method of exploring the problem of postevent information has been described as "ingenious" (Neisser, 1983: 15), but recent opinions are somewhat less generous (McCloskey & Zaragoza, 1985a). As most of what is known about postevent information derives from her work, and as the dispute at the centre of this dissertation concerns her method, I want to briefly introduce in this introductory chapter the methodology devised by her to investigate the problem, and show how its results have come to be disputed.

Loftus' 'method' consists, in its most elementary form, of a classically simple experiment. A group of subjects is shown a slide or film show depicting a complex event, which is often a simulation of an event that could occur as a real life scenario, and on which people could be called to testify[2]. Subjects are then randomly assigned to two groups, and are required, for example, to read a narrative description of the recently observed event, or to complete a questionnaire which ostensibly assesses their memory for the event. One of the subject groups is given *inconsistent* information in this phase of the experiment, usually through the embedding of a misleading detail in a subordinate clause in one of the questions in the questionnaire. The other group of subjects is not exposed to the misleading information (but may be exposed to information *consistent* with that originally observed). Both groups of subjects are then given a forced choice recognition test, usually in the form of a slide show, in which they have to indicate on a number of trials which of a number of alternatives corresponds to what they originally observed. Only two alternatives are normally presented, and these usually correspond to information originally presented, and information presented during the postevent information phase of the experiment, respectively.

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2. A favourite scenario is to show subjects a film or videotape recording of a motor vehicle accident. See, for example, Loftus, Miller & Burns (1978); Bekerian & Bowers (1983, 1984).

The experiment is schematically represented in Table 1.1 in terms of the different phases of the experiment and in terms of the type of information presented to subjects.

There have been numerous extensions to this simple experimental design, both by Loftus and by other researchers, which I will consider in detail in a later chapter. But for the moment, we need to take note of the results of experiments like that schematized above. These are usually that subjects given misleading information perform at a lower level of accuracy on the final recognition test[3] than subjects not given misleading information. The extent of the effect varies across a number of experimental manipulations, but is usually in the order of a 20 - 25% difference in accuracy between control and experimental groups. In addition, the effect is well replicated, having been reproduced on at least 35 occasions, by a number of investigators.

The results of these experiments are interpreted by Loftus and several others as showing that postevent information may impair memory for original information. More specifically, Loftus has interpreted the results as showing that postevent information may *destroy*, or *delete* memory for an event. She has used this claim, in turn, to make a number of theoretical assertions about both the permanence of human memory and the modality in which information in memory is encoded. These assertions have been very influential, and the article in which she first expounded these assertions

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3. On items that test memory for information that has been represented in a misleading way in the postevent information phase of the experiment.

TABLE 1.1 SCHEMATIZATION OF A POSTEVENT INFORMATION EXPERIMENT.

	SLIDE SHOW	QUESTIONNAIRE	TEST
CONTROL GROUP	ORIGINAL INFORMATION	———	OEI VS PEI
MISLED GROUP	ORIGINAL INFORMATION	POSTEVENT INFORMATION	OEI VS PEI

OEI = ORIGINAL EVENT INFORMATION
PEI = POSTEVENT INFORMATION.

(Loftus & Loftus, 1980) is one of the most frequently cited articles in recent cognitive literature.

In recent years, however, postevent information research has fared less favourably. Loftus' powerful claim that particular memories may be destroyed by subsequent information runs contrary to several contemporary theories of memory retrieval, and has been challenged very effectively by a number of researchers (Bekerian & Bowers (1983, 1984); Dodd & Bradhsaw (1980); Morton, Hammersley & Bekerian (1985)). Apart from this 'internal' dispute, which is limited to the question of whether postevent information experiment findings show deletion of existing memories by new information, or coexistence of original and new information, serious doubt has been raised about both the *utility* and *validity* of the research. There have always been doubts about the utility of the findings of eyewitness research for the legal profession (Clifford, 1978; Wells, 1978; Rabbit, 1979), but these questions have become far more persistent and searching recently, culminating in a series of confrontations between Loftus and her associates on the one hand and Michael McCloskey, of Johns Hopkins University, and his colleagues, on the other. While these confrontations have focused largely on the usefulness of eyewitness research, that is, on what currency psychological research has in legal settings, the dispute has swung more recently to the provocative claim by McCloskey & Zaragoza (1985a, 1985b) that postevent information research findings are artefactual. They argue that the paradigm of research that Loftus established rests on a demonstrably unsound experimental procedure. This claim is one of the most important matters to be addressed in the dissertation, and I will accordingly take a brief look at the rationale of the claim now.

I mentioned earlier that there has been some dispute within the postevent information paradigm about whether postevent information can be properly said to show deletion of original memories, or the coexistence of new and old memories. Both these claims depend on the assumption that memory has been impaired in some way by postevent information. In operational terms, the assumption is that the control - experimental difference observed in the experiment reflects a difference between the groups with respect to how many subjects are able to accurately recall information about the original event. In other words, the experimental effect means that a disproportionate number of subjects in the experimental group are unable to accurately recall the original event. McCloskey & Zaragoza argue that this conclusion is unwarranted, and that the typically obtained results will be obtained, using Loftus' paradigmatic procedure, regardless of whether memory is affected by misinformation or not. Their argument is very simple and very powerful - the conclusion that differences between control and experimental groups shows the impairment of memory in some experimental group subjects is in error because it fails to examine *the level of chance performance in the two groups*. McCloskey & Zaragoza start with the proposition that a certain proportion of subjects in the control and experimental groups will undoubtedly fail to encode the original information. However, these subjects will perform at different levels of chance on the forced choice recognition test depending on which group they've been assigned to. Those control groups subjects who don't encode the original information will be faced with two options on the forced choice test, and all these subjects should consequently perform at the level of chance. However, those experimental group subjects who do not encode the original information, unlike their control group counterparts, are exposed to *further* information after the original event, and some of these subjects, using the same premise outlined above, *will* encode this postevent information. Consequently, in the recognition test, these subjects will choose the item corresponding to the postevent information, thinking that it is information belonging to the original event. Because this postevent information is incorrect (it is purposefully designed to be misleading), experimental group performance will be lower than control group performance, but not because of any substantial effects on memory.

The claim that postevent information impairs eyewitness memory, then, is problematic, because the experimental effect used to support the claim is artefactual. Loftus' method shows neither that postevent information deletes memory for original information nor that it affects it in any other discernible way.

(McCloskey & Zaragoza's argument, as outlined above, limits itself to *a priori* considerations. But McCloskey & Zaragoza also bring empirical evidence to bear on the matter, gathered from a series of experiments that revised the paradigmatic postevent information methodology for experimental bias, and which failed to produce the paradigmatic 'misinformation effect'. I will consider this revision at some length in chapter 5).

Although McCloskey & Zaragoza's arguments are very convincing, it is not entirely clear that postevent information experiments produce artefactual findings. To start with, there are a number of experiments that report findings of memory impairment that are quite resistant to McCloskey & Zaragoza's explanation of artefact. This is because McCloskey & Zaragoza's argument is necessarily false under certain conditions, which they fail to make clear in their paper[4]. In the second place, at least two studies postdating McCloskey & Zaragoza's paper report findings of memory impairment in spite of correction for McCloskey & Zaragoza's hypothesis of experimental artefact. These studies call for independent estimates of experimental artefact in postevent information experiments. I shall attempt in later chapters to provide such estimates.

McCloskey & Zaragoza's arguments, then, far from settling the question of whether postevent information impairs memory or not, have simply produced more debate than existed before their contributions. This is totally in line with Popper's (1976) contention that the chief value to scientific answers lies in their ability to produce more problems, but a little disconcerting for people who might look (wistfully, no doubt) to psychological research for answers to 'practical problems'.

The problem that I wish to address then, is the disputed validity of postevent information study findings. I wish to bring evidence derived from methodological considerations and from direct empirical work to bear on the dispute. In addition, I wish to show what postevent information research

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4. I will take this point up in more detail in chapter 5.

can tell us about the notion of an applied psychology, and in particular, what the notion of applied psychology adhered to by postevent information researchers can tell us about the disputed validity of postevent information experiments.

In chapter 2, I attempt to locate the dispute under investigation in the contemporary landscape of cognitive psychology. In particular, I hope to show how the current interest in the psychology of eyewitness testimony takes much of its impetus from the concern among experimental psychologists to make cognitive research *ecologically valid*, by, among other things, applying research to practical problems.

In chapter three I provide a review of the substantial body of literature that addresses itself to the question of postevent information. This body of literature is clearly too large to be adequately reviewed using traditional review methods, and accordingly, I apply quantitative review techniques (so called 'meta-analysis') to the body of literature in chapter 6. These techniques are applied with the methodological problem raised by McCloskey & Egeth, and discussed at length in chapter 5, firmly in mind.

In chapter 4 I provide an evaluation of postevent information research specifically in terms of its applied orientation. The research problem, as I have argued in this chapter, stems from a concern with the reliability of eyewitness identifications, and is first and foremost an applied research problem. I argue that postevent information research pays lip service to this fact: in particular, the approach to the problem, and the conclusions drawn from the research, betray the token acknowledgement given the applied nature of the research.

In chapter 5 I present the methodological problem formally, covering the arguments lodged by McCloskey & Zaragoza in some detail. I also develop a formal model for dealing with the problem, which I use in chapter 6 to 'test' McCloskey & Zaragoza's arguments against the results of several postevent information experiments. Later in chapter 5 I review research published after McCloskey & Zaragoza's 1985 papers, which questions McCloskey & Zaragoza's arguments on empirical grounds. Consideration of these contradictory findings leads me to set out the problems that need to be resolved in order to decide the dispute.

In chapter 6 I report work from two *a priori* sources of evidence, and one experimental source, in an attempt to resolve the methodological dispute outlined in chapter 5. Although the experimental results seem to support the claim that results typically observed in postevent information experiments are artefactual, I argue that they do not resolve the dispute.

In chapter 7, the final chapter of the thesis, I draw a few tentative conclusions about why the study of postevent information has yielded such problematic results. I present the argument that the problematic state of postevent information research is largely consequent on the hotly pursued idea that *application* of research is a way to secure psychological research a measure of ecological validity.

There are two major lines of argument in the thesis then. In the first, which concerns the applied origins of postevent information research, I argue that prevailing conceptualizations of applied research are inadequate, and that postevent information research demonstrates this. In the second, which concerns the experimental validity of postevent information research, I pursue an existing dispute, bringing both *a priori* and *a posteriori* evidence to bear on matters. The two lines of argument turn out to be related: in particular, the methodological problems of postevent information research are shown to be a consequence of the (inadequate) conceptualization of applied research adhered to in the paradigm.

CHAPTER TWO: APPLYING PSYCHOLOGY.

1. INTRODUCTION

In the introductory chapter I indicated that the methodological dispute under scrutiny is not to be understood narrowly. It is not simply a question of how best to determine what postevent information does to eyewitness memory. There are, among other things, important implications for the notion of an 'applied psychology', and for the contemporary concern with the 'ecological validity' of experimental research. Accordingly, in this chapter of the thesis I introduce the methodological dispute in terms of these issues, and briefly trace the origins of the postevent information research tradition. Despite this attempt to 'locate' the dispute, I limit myself in the chapter, and in the dissertation generally, to theoretical issues more or less directly circumscribed by the methodological dispute. I consequently ignore much of what normally goes by the description of 'memory theory'. This is a necessary omission: nearly 100 years of research on human memory has accumulated, and indeed, we shall find that the literature around the question of postevent information already presents problems of manageability.

I begin by considering reasons for the contemporary enthusiasm for applying psychological research outside of laboratory settings.

2. THE CONTEMPORARY ORIGINS OF APPLIED COGNITIVE PSYCHOLOGY.

A decade ago, Ulric Neisser (1976), a leading cognitive psychologist long involved in experimental psychology, seriously questioned the scientific validity of cognitive research. He claimed that cognitive psychology lacked 'ecological validity', by which he meant that it failed to apply in important ways to the way people ordinarily lead their lives: it failed to acquire relevance outside of the laboratories in which it was constructed. Where psychoanalysis and behaviorism insured themselves, during their periods as hegemonic discipline, by making themselves *applicable*, cognitive psychology had failed to secure itself in this way. Neisser recommended a radical change in cognitive psychology's orientation if it wanted to endure as a psychological discipline - he predicted a brief and uneventful lifespan for the discipline if it failed to apply itself outside of its laboratories.

> Every age has its own conceptions - men are free or determined, rational or irrational; they can discover the truth or they are doomed to illusion. In the long run, psychology must treat these issues or be found wanting. A seminal psychological theory can change the beliefs of a whole society, as psychoanalysis, for example, has surely done. This can only happen, however, if the theory has something to say about what people do in real, culturally significant situations. What it says must not be trivial, and it must make some kind of sense to the participants in those situations themselves. If a theory lacks these qualities - if it does not have what is nowadays called "ecological validity" - it will be abandoned sooner or later. (Neisser, 1976; p 2)

Several years later, Neisser (1983) followed his original arguments up with an edited collection of memory research papers that addressed problems in ways that were clearly more applicable to conditions outside of the laboratory. He chose the papers as examples of what directions cognitive psychology should follow if it wanted to do 'ecologically valid' research, and commenced the collection of papers with a restatement of his 1976 argument, this time with the psychological study of memory particularly in mind.

> I think that "memory" in general does not exist . . . It is a concept left over from a medieval psychology that partitioned the mind into independent faculties: "thought" and "will" and "emotion" and many others, with "memory" among them. Let's give it up, and begin to ask our questions in different ways. Those questions need not be uninformed by theory, or by a vision of human nature, but perhaps they can be more closely driven by the characteristics of ordinary human experience. (Neisser, 1983, p 12)

The task is thus to make the study of memory applicable to our 'ordinary' lives. Although Neisser does not mean 'applicable' in the trivial sense that it is often used in in scientific circles ('the application of scientific principles to practical matters'), that knowledge generated by cognitive psychology might be used for practical gain is a theme that recurs in Memory Observed. One section of the collection, for instance, is devoted to cognitive psychological research on eyewitness testimony, and Neisser himself contributes one of the papers to this section of the book[1]. Here the point is stressed that research can very usefully be redirected for obvious and important practical gain.

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1. Although his treatment is somewhat different from that usually adopted. Most research on eyewitnesses has been experimental, but Neisser, interestingly, opts for a case study.

The message is fairly clear from Memory Observed: cognitive psychology lacks ecological validity and one of the ways of doing 'ecologically valid' cognitive research is to do research that can be used outside of the laboratories in which it is produced. The notion of 'ecological validity', of course, has ramifications far wider than the narrow connotations of 'generalizability' originally imputed to it by Brunswik (1956). Although it is not a simple notion, and Neisser is not altogether consistent in his use of it, for him it refers ultimately to the scientific respectability of a piece of research. . Ensuring this respectability is achieved, on one level, by making the results of the research apply outside the confines of its discovery.

Thus, by pinning his message of reorientation in cognitive psychology firmly to the tail of 'application', Neisser also (perhaps inadvertently) clothed the notion of an applied psychology with a certain intellectual respectability. That the 'tail' he pinned his message to happened to be not in the least like the pack horse he hoped (much more like a mule indeed), and that the 'intellectual respectability' has turned out to be a cloak for some really shoddy research, I will argue at a later place in the dissertation.

Neisser's argument has been very influential, but it is certainly not uncontested. One of the most frequently raised criticisms of his position is that it fails to see that very carefully controlled laboratory research is necessary before knowledge can be transported to natural environments: a universal functional theory is required before specific applications can be made. If laboratory work is essentially irrelevant, then field research is essentially miniaturistic.

Neisser responds to these criticisms[2] with the assertion that 'memory' is a generic term covering a diverse number of very different cognitive realities[3], and is thus (in its generic usage) not at all the appropriate object of a universal theory. We need to do much work simply describing 'memory', delineating the questions of interest. This process of delineation should proceed by examining the appropriate subject matter - the forms of memory that matter to us in our ordinary existence. Doing good research on memory is not so much a case of using more sophisticated methods as a case of finding the right questions to ask.

The argument is fairly complex, and to deal with it satisfactorily will take more than is worth to us here. I want to pass over an exhaustive treatment of the argument in favour of (a) a brief response to Neisser's dismissal of traditional 'laboratory' approaches to memory, and (b) a consideration of what 'applying psychology' means, in light of the fact that Neisser recommends it as a strategy for making research 'ecologically valid'.

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2. The criticisms of Neisser's position and the response to it that I present here are only abbreviations of much lengthier and more complex work. I cover them only insofar as they are required for the argument here.

3. For example, we say that we 'remember' how to play chess, and that we 'remember' the Lord's prayer. These activities surely involve very different processes, and to say that they're both examples of 'memory' is no more than to subsume them under a generic term.

2.1 A Response to Neisser

2.1.1 THE QUESTION OF LABORATORY RESEARCH

Neisser dismisses the 'laboratory' approach to the study of memory on the grounds that in the hundred years of its existence it has accumulated very little knowledge about the experiences we ordinarily think of as involving memory processes. Its findings are restricted not only in terms of generalizability - scenarios typically used to study memory in laboratories[4] rarely resemble those outside the laboratory - but their theoretical representation is usually also restrictively experimental. (Thus, 'memory interference' *means* performing in a particular way on a list learning task.) Laboratory research on memory has failed to accumulate the kind of findings that constitute a coherent and useful body of knowledge. What knowledge it has accumulated, anyway, presents little advance on what is immediately obvious - indeed, on what is immediately obvious to preschoolers.[5]

That the psychological study of memory has accumulated little coherent and really useful knowledge about memory processes does seem to be an indictment of the discipline, but it does not, I think, point to the need to abandon the laboratory as the primary research setting. Measuring the progress of a science by its accumulation of findings is a problematic business. This, of course, is Kühn's (1970) seminal point in The Structure of Scientific Revolutions.

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4. For example, situations in which subjects are required to learn lists of nonsense syllables

5. This point is less scathing than it might seem. Several studies show formally that children of preschool age know most of the important experimental findings in traditional memory research. (Kreutzer, Leonard & Flavell, 1975).

Although scientific knowledge does accumulate, the most important way in which science proceeds is by 'revolution' and not through accumulation. The history of science is not so much the history of methodical empirical work as the history of rare, exceptionally good, ideas[6]. Neisser's emphasis on the failure of cognitive psychology to accumulate any startlingly useful knowledge about memory processes may be a little misplaced: it may simply be the adherence to an outdated philosophy of science in which accumulation is the *sine qua non* of real progress. We may not have had many really good ideas, is all.

As far as Neisser's contention that we should dedicate ourselves to 'naturalistic' work on memory goes, the analysis presented here of an important contemporary methodological dispute shows that there is no substitute for the kind of rigorous intellectual activity that scientific method embodies. In the absence of this rigour, which is an absence which will be entailed (I want to suggest) by following Neisser's advice, our results will simply be less reliable and less valid, despite the fact that they may seem intrinsically more interesting. In the second place, although results derived from traditional experimental methods may seem intrinsically uninteresting, their application outside of the laboratories in which they were discovered is actually a more promising and significant

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6. A digression into Kühn's position is undesirable here. His work is so well known in contemporary academic circles that I take it for granted that his position is familiar enough to make my point understood.

matter than the application of results derived from methodological regimens tailored for application.[7]

This last contention leads us into the second part of the response to Neisser's position.

2.1.2 THE NOTION OF AN APPLIED PSYCHOLOGY

Neisser urges in several places that memory research address 'practical problems':

> . . . . some of the best minds in psychology have worked, and are presently working, in the area of memory. Why, then, have they not turned their attention to practical problems and natural settings? (1983. p 6)

Unfortunately, Neisser fails to address the many complex issues that are raised by the prospect of 'applying' research findings. Does one tailor the research for the application to minimize problems of generalizability? Or is it possible to adjust research findings to the applied setting? Does the research have to be exceptionally sound before it can be applied (to anticipate the obvious magnification of ethical problems)? A number of very thorny issues lie below the surface prospect of 'solving practical problems' with research. These need to be considered closely if we are to take seriously the suggestion that 'application' is a way of securing 'ecological validity' for a piece of research.

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7. I must admit that I am deeply uncertain about this point. I argue at several places in the thesis that a thorough analysis of the notion of an applied psychology is the key to the problems with postevent information research, but have profound reservations nevertheless. The present issue reveals one of these reservations.

As the research to be examined in this thesis takes much of its impetus from the prospect of applying its findings to legal matters, I will deal with the issues that 'application' raises at some length now.

2.1.2.1 ORTHODOX CONCEPTUALIZATIONS OF APPLIED PSYCHOLOGY

'Applied psychology' is one of the oldest formally recognized and institutionalized branches of psychology, at least in name. As early as 1908 a chair in Applied Psychology existed in an American institution (the Carnegie Institute of Technology), and by 1917 the American Psychological Association had established the Journal of Applied Psychology. Thus, by the early decades of this century, applied psychology was considered to be a firmly established subdiscipline: institutes existed for applied psychological research, chairs of applied psychology existed in several universities, and a burgeoning technology (mental testing) was associated with applied psychology[8].

But exactly what 'applied psychology', considered as intellectual practice, is, is not a matter given much attention in the writings of this period. This is an odd state of affairs from an historical point of view, for the psychology of this period is notoriously reflective, and often difficult to distinguish from academic philosophy of the same period (cf. William James' Principles of Psychology, for instance).

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8. The historical account presented here is taken from a reading of the accounts given in Anastasi (1964) and Dudycha (1963). As an historical presentation, it is quite obviously inadequate, but it does make the point fairly clearly that 'Applied Psychology' has long been considered a respectable subdiscipline of psychology.

For the purposes of the discussion here, some idea of what 'applying psychology' means, at least in conventional terms, is needed, and so I will introduce a number of conceptualizations from prominent 'applied psychologists'.

Dudycha (1963), likens the applied psychologist to an engineer: in the same way that the engineer applies 'theoretical physics'(sic), the applied psychologist applies the findings of psychological science. This allows the rather mundane reading of Applied Psychology as 'psychology in use':

> . . . the applications of psychology to the various areas and aspects of individual and social life. (p 4)

Notice how Dudycha displaces the onus of the justification: applied psychology is sound insofar as psychological science is sound. (How application, on the other hand, is justified, is unconsidered).

This link between application and the extension of the scientific method is made even clearer by Anastasi (1964). The applied psychologist's contribution stems from 'his research approach to the problems of human behaviour': he takes the scientific method common to all sciences into applied contexts. Insofar as applied and basic research differ, the essential differences concern:

1. How the problem is chosen. Basic research chooses problems to help in the construction of theories; applied research chooses problems to help in administrative decisions.

2. The specificity and generality of results. Basic research is more generalizable; in applied research generalization is of limited validity

> (as applied research is less concerned with theoretical and causal relations). (p 5).

The problem, for Anastasi, is to distinguish 'applied' from 'pure' without sacrificing the legitimizing claim on the 'scientific method'.

Both conceptualizations considered thusfar attempt to distinguish applied science from pure science formally: the difference between the two endeavours resides finally in the *locale of practice*, and whatever other differences there are may be specified from knowledge of this fact.

In much the same vein is the following conceptualization (vd Velde (1982)), which appears in an influential European series of monographs.

According to vd Velde, applied and pure research differ in the way they treat their respective independent and dependent variables. Where pure research concerns itself principally with the *relationships* between independent and dependent variables (say, in the rate of change of the activity of the sweat glands under controlled conditions of stress), applied research is seen to be concerned exclusively with a *particular* dependent or independent variable (say, in the efficiency of a managerial training program). Pure science serves to increase our knowledge about a theoretically interesting relationship; applied science serves as the basis for decision making with respect to a concrete dependent or independent variable.

The relationship of this view to the views espoused by Anastasi & Dudycha is fairy transparent and the observations

I made earlier apply equally here. I single out vd Velde's treatment because it makes the pointed claim that 'applied research' is so named because of its application: later I will show that the 'naming' of applied research is a much more arbitrary matter than this.

Applied research is the extension of pure research into 'ecologically real' locations. This is the line of argument pursued in all of the accounts I've considered thusfar, but subtler formulations are naturally possible. So, for example, Warr (1978) denies the applied/pure dichotomy, and instead asserts that it exists as a dimension, stretching from the completely pure investigation to the completely applied project. Where, on this dimension, a particular psychological enterprise falls, depends on (i) the population studied, (ii) the research setting, and (iii) the intended outcome of the work. The problems to be studied are taken directly from real life situations, although they may be studied on the spot or in laboratories.

> . . . the applied nature of an investigation derives from the fact that its proximal origin is in a sense external to the discipline. (p 11)

whereas

> Pure psychological research aims to deal with an issue raised by the results, theories or ideas of psychologists themselves, being part of a shortcycle feedback system feeding directly upon its own outputs (p 11).

I have considered a number of attempts to identify the intellectual pursuit that constitutes 'applied psychology'. I want to show in the next section of the chapter that these accounts, which I claim are representative of widely held notions about the nature of applied psychology, serve little more than an ideological[9] purpose, and that the notion of an applied psychology is consequently better treated as a metatheoretical problem than as a category of research.

2.1.2.2 PROBLEMATIZING THE NOTION OF AN APPLIED PSYCHOLOGY.

In a provocative and incisive paper, John Potter (1982) argues that most discussions of applied science can be subsumed under a more general ideological practice which attempts to present 'science' as socially useful, as the origin of many of the things that improve our lives. The notion of 'applied science' serves this broader function by contributing to it an 'ideology of application' (1982, p 24): the intimate relation held in scientific cultures to exist between science and technology. The first two conceptualizations of applied psychology that I discussed - those espoused by Anastasi and Dudycha - are exemplary instances of this: applied psychology is the transportation of the scientific method to locations proximal to the discipline.

But just how close is the relationship between science and technology? The suggestion of an intimate relation between science and technology - as inscribed in the ideology of application - is, to say the least, problematic: at any rate, the relationship is not of the direct form suggested. An increasing body of research in the sociology of science and philosophy of technology suggests that technology is not simply applied science (in the sense given to the term in

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9. I mean 'ideological' here in the (currently) archaic sense in which it denotes 'false consciousness'.

the ideology of application). For example, research on the U.S. weapons industry shows that 91% of innovations in the technology originated from inside the technology itself, and only 9% from scientific research (Potter, 1982). Similarly, studies using citation analysis find that

> . . . science seems to accumulate mainly on the basis of past science, and technology primarily on the basis of past technology. (Mulkay, quoted in Potter (1982):28)

This is not to say that technology and science bear no relationship to each other: the sense in which technology and science do relate is best taken as a case of enablement, but this enablement is in a direction contrary to that hypothesized by the ideology of application. Ihde (1979), for instance, argues that the history of technology shows that technology of a particular form is a *prerequisite* for science of a particular form[10]. That knowledge "flows" from the pure pole of the pure/applied dimension to the applied pole is an untenable thesis. The claim serves clear ideological interests: connotations about the social utility of 'science' slip into the way we think about our lives, attaching a particular valency to the idea of 'science'. It is precisely to maintain the implications attendant upon the idea of a flow of knowledge from 'pure' to 'applied' that prevailing conceptions of 'applied psychology' identify the origin of the research problem as the feature that distinguishes pure from applied research.

The matter does not end here. Potter makes a useful and very relevant distinction between *applied* psychology and *applicable* psychology. The point is that most of what we

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10. In the same way that watermills (among other technological innovations) existed before, and were prerequisites for Newtonian mechanics.

call applied psychology is really only applicable psychology: findings made under the name of applied psychology are generally not applied - the label is assumed only because of a superfluous concern with issues or problems in society. In most 'applied research', all that happens is that academics pluck problems from the outside world and justify their work in academic terms. Thus, a substantial amount of work in environmental psychology addresses the relationship between urban noise levels and psychological stress, but it's difficult to see that the research goes any further than the examination of this relationship. The research is called 'applied research' only because it addresses a social problem.

This being the case, it does not take much to see that the formal attempt by vd Vlist, addressed earlier on, to distinguish pure from applied research in terms of concern with dependent and independent variables must be mistaken - applied research certainly can't be concerned with either dependent or independent variables for purposes of making administrative decisions, because applied research has so little consequence outside of academic settings.

This discussion of the problems in the orthodox conceptualizations of applied psychology has drawn attention to the ideological notion of the pure-applied split and the pertinent distinction between applied and applicable psychology. There is a further point I wish to make about these conceptualizations, which is the extraordinary way in which the 'applied' in 'applied psychology' is treated.

The 'applied' in applied psychology, is read, in all the accounts I have considered, as the name of a subdiscipline of Psychology. To put it clearly: the question of an applied psychology is treated in terms of what makes applied psychology a discipline, not in terms of why applied psychology is an applied endeavour. Instead of focusing on how psychology is applied, elaborate attempts have been made to show how applied differs from pure. 'Applied' is taken as an unproblematic qualifier of psychology, but in fact what is meant by 'applying psychology' is extremely complex.

This omission is the nominalist error of assuming the paradigmatic stability of the referent of a description: because a name exists, an entity is assumed to correspond to it. In this way psychologists have taken for granted the 'applied' nature of applied psychology, and have failed to ask important questions about *if* psychology is applied, and *how* it is applied. Thus what is really only applicable psychology at best has come to constitute what goes by the name of applied psychological research.

So we can see that the notion of an 'applied psychology' is a very problematic one indeed. Neisser's recommendation that memory research apply itself to practical problems then, is certainly not the guarantor of scientific respectability that he suggested it might be. In point of fact, the issue of 'ecological validity' is even more relevant here when we think what it might be to 'apply' psychology, than it is to traditional memory research. This is because so little of what is ostensibly applied psychology qualifies in any way as applied research. There is nothing more 'ecologically valid' about research that exhibits a superfluous concern with social problems than research that utilizes traditional methods of laboratory experimentation.

This is not to say that applying psychological research, or doing applied psychological research, is an impossible prospect. Examples of situations where research findings have been applied, and situations where they've even been incredibly useful are not difficult to find. A case in point is the 1978 United States Supreme Court ruling that the optimal size of a legal jury is between 6 and 8 people. This decision was based on an in-court appraisal of simulated jury research (Loftus and Monahan, 1980). Similarly, the close cooperation of Canadian police and research psychologists has resulted in the formulation of a number of identification parade guidelines currently in use by the Canadian police (Rule and Adair, 1984; Yuille, 1984).

The point here is that it is not acceptable to discard the notion of an 'applied psychology', even though the way in which it is ordinarily conceptualized is unsatisfactory. It is beyond the scope of this dissertation to attempt a solution to this problem, but I shall make a brief observation that might help us think about matters more clearly.

The observation concerns what determines whether research gets applied or not. One useful exploration is to systematically examine examples of research that do get applied and to compare them to research that fails to get applied. One such attempt exists in the applied behavioural science literature (Stolz, 1981), which I wish to consider briefly.

2.1.2.3 DETERMINANTS OF APPLICATION

Stolz (1981), after a scrutiny of innovations from applied behavioural research, isolated the following determining variables in the successful dissemination and application of research.

1. Research data showed that the innovation was effective.
2. The technology met the continuing mission of the adopting agency.
3. The potential adoptor had a pressing management problem.
4. The availability of the dissemination to the potential adoptor was timely.
5. Potential adoptors were able to view ongoing model programs.
6. The adoption was proposed by policy makers, rather than by the researchers who developed the technology.
7. The intervention was tailored to local conditions.
8. Those who would have to implement the program were involved in the preliminary research and in asking for the adoption. (Stolz 1981, p 498-99).

There seems to be a persistent theme here: what is required for the execution of successful applied research is the creation of an appropriate infrastructure. If we consider what it means to make 'application' we see that it requires a certain jurisdiction: thus, application that results in the promulgation of new laws is enacted by the appropriate authorities, application that produces a change in the policy of a company toward its employees has to go through particular channels, and so on. Applications are made by policy makers, by a body that possesses the appropriate authority. For application to succeed, it is necessary that those who authorize the application are intimately involved in the research process. This is why establishing an appropriate infrastructure is important.

The terms on which applied research should be conducted are consequently the applicant's, and not the discipline's.

In a later chapter, when I evaluate postevent information research in terms of its applied orientation, I will attempt to show what this means, but a full exploration of the principle is pointless here.

(A fairly long digression now. Although I think that the notion of an 'applied psychology' is worth pursuing, and indeed, formalizing, it is not final for me that this is the best way to pursue application (I hinted at this in earlier parts of the chapter). Establishing an appropriate infrastructure might be one step towards ensuring that research gets applied, but it also leaves the impression that the pursuit is, finally, intellectually valueless - a wasteful appropriation of sophisticated research methods to an ultimately trivial end (in the same way that teflon frying pans are a trivial application of research in aeronautics). The sense of 'application' that the discussion in this chapter raises is a rather subdued characterization of the way in which scientific thought affects our lives. Valuable application[11] doesn't spring from making research 'work' in 'ecologically real' nooks and crannies, but from the import of truly profound ideas on the

11. I mean 'application' here as a notation for the effects (on both intellectual and material levels) that scientific ideas have on our lives.

intellectual climate of an era[12]. This is the sense in which Copernicus' ideas about the nature of the universe constituted an intellectual revolution, and deeply affected most scientific thought in fundamental ways for centuries afterwards, or in which Freud's thoughts and writings on infantile sexuality revised some of our cherished notions about human development. Ideas like these have the ability to change the whole intellectual ethos of an age. If we consider the 'application' that ideas like these sustain, then it's quite apparent that the type of applied research that I outlined above results in rather trivial application.

To cut a long aside short, if we're looking to application as a justification for certain types of research, then it may be best to pursue this application not by transporting crude, but workable, ideas into 'communities', but by going about in the way that traditionally results in profound application: the remorseless hunt for good ideas. It's difficult to see that application in the limited sense outlined above can serve to guarantee research 'ecological validity'. If we pursue this path we are more likely to make ourselves technicians to the whims of those who can afford to employ us than to produce a scientifically respectable body of knowledge about human memory processes.

Although bearing this in mind, I will proceed in the dissertation along the lines set down earlier: that is, along the lines of the ideas about 'applied' research that I introduced earlier in the chapter).

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12. This is an elaboration of a point made by my supervisor, Professor Peter du Preez, during a seminar in which I first presented some of the ideas about 'application' that appear here. I think the point is an astute one, and quite in line with some contemporary views on the development and practice of science.

3. CONCLUSIONS

This dissertation looks critically at a body of 'applied' psychological research and attempts to resolve a methodological dispute currently at the centre of this body of research. In this first chapter of the dissertation I have attempted to situate issues to be raised in later chapters in a manner that will allow me to develop a particular line of argument. Thus, I argued that postevent information research stems from a contemporary concern to 'apply' memory research and that this concern may be due to the *zeitgeist* in contemporary research circles to make research 'ecologically valid': that is, to make it address problems that are more clearly relevant to situations outside the laboratory. This chapter has attempted to expose the dangers hidden in this intellectual manifesto. In particular, I have argued that those who clamour for application usually give no consideration to the several and varied problems that precipitate around the prospect of applying psychology. The point to reiterate in summary is that the study of postevent information must be considered in the light of its stated 'applied' orientation. Postevent information research was not designed to address fundamental theoretical issues, but it has ended up drawing highly influential theoretical implications from research findings. A critical analysis of the paradigm must, however, evaluate the research with respect to its ambition to *apply* its findings.

I will take this line of argument further in a later chapter, where my conjecture will be that the problems with postevent information research stem from its applied orientation, and in particular, from the impoverished notion of 'application' that it takes bearings on.

CHAPTER THREE: LITERATURE REVIEW

1. INTRODUCTION

In the previous two chapters I introduced and attempted to theoretically situate a body of applied research. In this chapter I review the published literature constituting this body of research. A reasonably thorough (but not exhaustive) search of the literature reveals that there are at least forty directly relevant published studies reporting, in total, more than 70 experiments, and well over 100 experimental - control comparisons (the fundamental 'measure' in postevent information experiments). Traditional qualitative review methods are patently incapable of dealing with a body of research this large[1]. Consequently, I will approach the review of the literature by (i) identifying and attempting to integrate findings from a number of experimental paradigms that address the problem of postevent information, and by (ii) (in a later chapter) providing a quantitative review of the same body of research with specific theoretical and methodological questions in mind (using meta-analytic methods developed by Hunter, Schmidt & Jackson (1982)).

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1. See Hunter, Schmidt & Jackson (1982) on a formal statement Of the contention that traditional review methods are unsuited for the evaluation of large bodies of research.

2. THE STUDIES TO BE REVIEWED

The studies to be reviewed here address themselves to the effect that information acquired after an event may have on memory for the event. Research methods utilized have been almost solely experimental: only one *in situ* study, to my knowledge, has been conducted (Yuille & Cutshall, 1986), and no formal archival studies are reported in the literature, although Loftus, in several places, illustrates the phenomenon of postevent information by referring to court records (see, for example, Loftus 1979a, Chapter 7). Four experimental paradigms are identified (on the basis of methodological and theoretical criteria), several of which, in turn, are broken down into subparadigms.

I will examine each of these paradigms in turn, paying special attention to the methodological innovations that each introduces into the research program, and to the historical connections between the innovations. Throughout the review I shall use the 'hash' character (#) to assist with the categorization of research paradigms.

#1. EXPERIMENTS THAT EXAMINE THE EFFECT OF THE PHRASING OF QUESTIONS ON EYEWITNESS RESPONSES.

These experiments have their precedent in the very early work by Muscio (1915) at Cambridge, whose study embodied an experimental design that has subsequently informed much of the research conducted in the postevent information paradigm. Muscio showed a group of subjects a series of cinematographic recordings ("moving films") and soon afterwards questioned them about what they had seen. Using eight discrete forms of questions, he found that questions embodying the definite article ('the') and questions employing 'negative terms' were more likely than direct questions to produce incorrect responses.

Muscio's work was largely ignored in the years after his 1915 study, and the question that intrigued him suffered a consonant amount of neglect. Harris (1973) revived interest in the original work with an experimental study that, again, showed the effect that question wording may have on answers given to questions.

In Harris' (1973) study, subjects were questioned with sentences using either marked or unmarked modifiers. Marked modifiers suggest an answer to the question posed (as in, for example, "How *short* was the man?"), while unmarked modifiers don't (ie. "How *tall* was the man?"[2]). In the experiment reported in Harris' study, subjects were read a 32 item questionnaire. Each question allowed the use of a modifier; for half of the subjects the questions contained a marked modifier, and for the other half an unmarked modifier. Questions were constructed so that quantitative responses were elicited in relation to a film seen by subjects.

Subjects questioned with marked modifiers produced numerical estimates that differed significantly from those produced by subjects questioned with unmarked modifiers. The difference was always in the direction of the suggestions contained in the questions, supporting the idea that subject reports may be influenced by subtle suggestions.

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2. Although an unmarked modifier also seems to suggest an answer to the question, this suggestion isn't alive in ordinary usage.

Although the question that informed Muscio's original work has passed down to current research, a second and more provocative question has attached itself to the paradigm. There are thus two separate questions asked in paradigm #1:

(1) Can (suggestive) question wording elicit answers biased in the direction of the suggestion?

(2) Can question wording affect memory for an event?

The second question is conceptually and historically contingent on the first: answers biased in the direction of suggestions embedded in questions need show little more than a type of compliance[3], whereas structural changes in memory for an event suggest a theoretically more interesting process. Question (1) is the question that Muscio addressed, and question (2) is largely Elizabeth Loftus' innovation.

Attempts to answer questions (1) and (2) above use a method much the same as that used by Muscio in his early study. Subjects are shown a cinematographic representation of a complex visual event, after which they are allocated to experimental and control groups. Each group is then interrogated (usually by questionnaire) about the event they have just observed. A number of these questions are phrased differently for experimental and control groups. The effect of the question wording is determined by observing the differences between experimental and control groups in responses given to the questions. This is usually

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3. Suggestions may reveal the experimenter's intention: "How short was the man", apart from suggesting an answer, may, as a consequence of its unusualness, alert the subject to the experimental motive behind the question.

simplified by structuring the questions so that subjects have to provide numerical estimates. In Loftus & Palmer's (1974) study, for instance, the effect is measured by requiring subjects to estimate the speed of a motor vehicle. Results of experiments typically show that subjects exposed to suggestive questions tend to deliver answers that are biased in the direction of the suggestion. So, for example, Loftus & Palmer found that estimates of speed made by subjects interrogated with highly suggestive verbs were significantly higher than those made by subjects interrogated with less suggestive verbs.

This differs little from the procedure originally used by Muscio. What is of considerable interest here, though, is an innovation to Muscio's method, in a second experiment reported in Loftus & Palmer's 1974 study. In this experiment, subjects were shown a film of a motor vehicle accident, and exposed to suggestions in the usual manner (one group of subjects was asked to estimate the speed of a motor vehicle that had *crashed* into another vehicle; the other group was asked to estimate the speed of the same motor vehicle, on the understanding that it had *contacted* with another vehicle). One week later, however, subjects answered a further questionnaire, in which they were asked whether there had been broken glass at the scene of the accident. (There had been none). Subjects who had been questioned with verbs that suggested breakage ("smashed", as opposed to "contacted") tended to answer in the affirmative more often than subjects who hadn't. This particular modification moves the focus of the research away from the question of whether suggestive wording can affect answers to questions, to the more provocative question of whether suggestion can *change*, or *alter*, memory for an event.

A number of studies utilizing comparable methodology reproduce experimental - control differences along the lines reported by Loftus & Palmer (experiment 1) (eg. Thorson & Hochaus, 1977). Nevertheless, there is an interesting and important failure to replicate reported by Read, Barnsley, Ankers & Whishaw (1978).

Read et al. used the methodology typically employed by Loftus and others: subjects viewed videotaped sequences of three incidents (a car accident, a street fight, and a scene depicting police harassment), after which they answered questions which varied with respect to wording. The questions varied parametrically across subjects with respect to the severity of verbs included in the question[4] (thus replicating and more strictly controlling the manipulation of question wording). Immediately following the questions subjects were asked to estimate the speed of a vehicle involved in the scenario depicted in the film. One week later all subjects returned and answered "outcome" questions related to each viewed incident: these attempted, as in Loftus & Palmer (1974), to determine whether suggestive question wording given to subjects during the questioning phase of the experiment had changed the thematic structure of memory for the scenarios originally viewed.

Both tests of Loftus & Palmer's (1974) contentions that (i) systematically biased question wording elicits answers biased in the direction of suggestion, and (ii) that question wording can adversely affect the thematic structure of witness memories, were disconfirmatory. That is,

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4. Read et al. standardized the severity of verbs used in their study by getting a group of subjects not involved in the experiment to rate a number of verbs for severity.

varying the severity of verbs used to question subjects neither affected the estimates of speed at the time of the questioning, nor influenced subjects' recall of the event one week later.

This particular non-replication casts suspicions on the reliability of results reported by Loftus and others within this paradigm, because it is the only study that attempts to manipulate question wording parametrically. It is also a relatively unknown study: its failure to replicate is never included in synopses of postevent information research, and indeed, its absence from Loftus' (1979a) résumé of postevent information research is quite conspicuous. (Elliot (1985), for one, has drawn attention to this 'oversight'[5] on Loftus' part). Nevertheless, the Read et al. study exists alongside a body of studies that *do* show effects of question wording on witness reports, and it's difficult to reject these demonstrations solely on the basis of a single failure to replicate.

#2. THE SUGGESTION OF NON-EXISTENT DETAILS BY MANIPULATION OF QUESTION WORDING.

This research paradigm developed out of paradigm #1 both historically and conceptually. The experiments in paradigm #1 left unresolved the question of whether the alterations in witness reports produced by experimental manipulations showed alterations in memory for the original information, or were simply the upshot of response bias or demand

5. Interestingly, Read et al. acknowledge Loftus' commentary on their results in a footnote to their article (p 795).

characteristics[6]. For example, subjects could simply have estimated the speed of the vehicle in Loftus & Palmer's study *in response to* the verb used in the question: being undecided about the speed of the vehicle, the verb "smashed" provided a cue for them to make up their minds about what answer to give to the question.

In paradigm #2, questions are phrased such that the presence of a nonexistent event is suggested to the experimental group. For example, in Loftus & Zanni's (1975) experiment, the indefinite article "a" is pitted against the definite article "the": "Did you see *the* red car?" (experimental group), vs "Did you see *a* red car?" (control group), when in fact no red car was seen in the represented event. As Loftus & Zanni (1975) point out, the experimental group wording of the question asserts that there was a red car, while the control group wording makes no such assertion. The experiment is for Loftus an attempt to establish whether suggestion can bring about "...a reconstruction...in original memory...for an event" (1975, p 88).

Subjects interrogated in this experiment with questions that suggested the presence of a nonexistent object were more likely to report the nonexistent object than subjects not questioned in this way. Similar results have accrued from replications with different materials (Loftus & Greene 1980), and in more naturalistic scenarios (Schiffman & Davis 1985).

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6. Loftus addresses this point at some length in her 1975 paper with Guido Zanni. 'Demand characteristics' is meant here in the sense imputed to the phrase by Martin Orne: "the totality of cues that convey an experimental hypothesis to the subjects".

Despite these experimental corroborations, as in paradigm #1, an important non-replication is reported in the literature. Zanni & Offerman (1978), using very similar methodology to the original study, failed to find any effects of question type on witness reports. The authors conclude that the difference in findings between the two studies may be due to demand characteristics. Loftus & Zanni used filmed automobile accidents, incidents which contain strong demand characteristics concerning the perception of certain items, while their study used films that were 'neutral' in tone. They speculate that the definite article might only produce a greater number of errors when a witness is interrogated about incidents containing biased demand characteristics. They conclude:

> ...it appears that the differential effects of the articles employed in question wording upon recall are far more subtle and complex than previously assumed. (Zanni & Offerman, 1978, p 166)

Paradigm #2 thus suffers from a similar, single failure to replicate the results that marked the initial study. Whether the non-replication in this case (or in #1, for that matter) is due to a critical, small change in experimental procedure or is perhaps explicable at the level of random sampling variation is not an issue that we are equipped to answer using traditional review methods. We have simply to treat it as an anomaly.

#3. THE EFFECT OF INFORMATION PRESENTED AFTER AN EVENT ON MEMORY FOR THE EVENT.

Paradigm #3 incorporates a substantial number of studies - more than 30 in all - which are subsumed under the aegis of #3 as they share critical methodological features. These features are simple but conceptually and experimentally important developments of the experimental procedure utilized in paradigm #2. The studies to be considered are grouped as a body within one paradigm, but they are best dealt with as separate subparadigms. Before we deal with these subparadigms however, we need to deal with the fundamental methodological innovation that the paradigm rests on.

In paradigm #2, subjects (i) see an incident and are (ii) then asked questions that suggest that a (nonexistent) entity was present in the incident. The measure of interest is (iii) the response to this question. Stages (ii) and (iii) are thus temporally proximal. In #3, stages (ii) and (iii) are separated out more clearly: the design is fully temporalized. First, subjects are shown a representation (staged or cinematic) of an event. At a later stage they are exposed to further information directly related to the observed event. This information may be consistent, or inconsistent, with what they originally observed, and it may be transmitted through a number of media (for example, as embedded in interrogation by questionnaire or in a narrative account of the original event). Still later subjects are administered a measure designed to reflect the effect of postevent information on memory. This measure is usually a forced choice recognition task, but free recall tasks have also been used. Table 3.1 represents the prototypical experimental design utilized in paradigm #3.

Representing the design in this way should make it clear that the experimental intention is to show that any difference in test performance across control and experimental subjects is due to the differential

TABLE 3.1 REPRESENTATION OF THE METHODOLOGICAL INNOVATION UNDERLYING PARADIGM #3.

	OEI	PEI	TEST
CONTROL GROUP	REPRESENTATION OF EVENT.	(I) NO PEI OR (II) CPEI	PEI VS OEI
EXPERIMENTAL GROUP	REPRESENTATION OF EVENT.	(I) IPEI OR (II) CPEI	PEI VS OEI

OEI = ORIGINAL EVENT INFORMATION
PEI = POSTEVENT INFORMATION
CPEI = CONSISTENT POSTEVENT INFORMATION
IPEI = INCONSISTENT POSTEVENT INFORMATION

manipulation of postevent information across the two groups. Later in the dissertation I shall present several arguments that dispute the ability of the experimental design to answer the question it poses.

#3.1 The Classic Slide Pairing Studies.

Loftus, Miller & Burns (1978) is, to borrow one of Kühn's (1970) valuable phrases, the *exemplar* of the studies in #3. In Loftus, Miller & Burns (1978), experiment 1, a large number of subjects were shown a photographic slide sequence depicting an autopedestrian accident, in which a red Triumph motorcar crosses the road from a stop intersection and knocks down an elderly pedestrian. Some time later half of the subjects were required to complete a questionnaire, one question of which had embedded in it the false suggestion that the intersection was a yield intersection. The remaining half of the subjects were kept as a control group against which possible differences could be assessed: they were asked to complete an unrelated filler task. Still later all subjects completed a forced choice recognition task: subjects saw 8 pairs of slides, each pair consisting of one slide from the original slide series, and one not seen during the original series, and were required to indicate which one of the two slides they had seen in the original sequence. One of the slide pairs (the critical pair) depicted (i) a red Triumph motorcar at a stop sign (originally seen), and (ii) the same red Triumph motorcar at a yield sign (misleading information). Subjects who had been exposed to misleading postevent information chose correctly less frequently than subjects who had not been exposed to the misinformation: the misled group identified the yield sign as the sign they remembered seeing in the original series more often than the control group.

The rest of the subparadigms in #3 are (variously) elaborations or tests of the fundamental design embodied in Loftus et al. (1978), experiment 1.

#3.2 Delaying the Presentation of Postevent Information.

In the prototypical #3 experiment, inconsistent information is introduced almost immediately after the presentation of the original information (usually after a short filler task). In real life situations, however, as Loftus et al. (1978), rightly point out, witnesses are more likely to be exposed to false postevent information much longer after the original event than in the typical postevent information experiment.

There are two obvious ways in which the role that time plays in the transmission of the postevent information effect can be investigated. Both of these involve a simple modification to the experimental design that underpins #3. The first modification involves delaying the presentation of the postevent information until just before the final test, while the second modification involves delaying the test till well after the presentation of the postevent information. The rationale for the first modification is that with a delay between the presentation of original event information and postevent information we expect witness memory to be less complete than immediately following the presentation of original event information, and therefore to be more susceptible to misinformation. In the case of the second modification, subjects should still have fairly accurate and complete knowledge about the original event and should consequently be less susceptible to misinformation (Loftus et al. 1978).

Loftus, Miller & Burns (1978), experiment 3, utilized the modifications outlined above in an experiment that addressed the question of temporal intervals in the transmission of the misinformation effect. Five retention intervals (immediate, 20 minutes, 2 days, 1 week) were used in the experiment for both modifications of the design (ie. (i) postevent information delayed; (ii) test delayed). In both modifications, longer retention intervals coupled with the presentation of misleading postevent information led to worse performance, approximating, in both cases, the level of chance performance, after 2 days. But, in particular, when postevent information was delayed until just before the final test (the second of the modifications), subjects were more likely to incorrectly identify the misleading information as that which they had seen in the original slide series.

#3.3 The Effect of Consistent Postevent Information on Memory for an Event.

The experiments reviewed in paradigm #3 thusfar all exposed subjects to postevent information inconsistent with the event originally witnessed. The logical corollary of this manipulation is to expose subjects to postevent information that is consistent with the original event.

A number of experiments examine the effect of consistent postevent information on witness memory. The modifications to the prototypical experimental design of #3 are slight: usually this is simply a matter of either (1) exposing the experimental group to consistent postevent information, or (2) extending the design to include an additional experimental group, which is exposed to *consistent* postevent information, in contrast to the other experimental group which is exposed to *inconsistent* postevent information.

An early experiment that utilized consistent postevent information is reported in Loftus (1975). In this experiment, subjects were shown a film depicting a car crossing a stop intersection and causing a five - car pileup. Later, the experimental group was administered a 10 item questionnaire in which was embedded the (correct) suggestion that a stop sign was seen in the film. Further on, in the same questionnaire, both experimental and control subjects were asked to indicate whether they remembered seeing a *stop* sign in the original film. Experimental subjects were found to be more likely to answer this question in the affirmative than the control group. Exposing subjects to consistent postevent information thus improved performance on a subsequent memory task.

Several other studies use *consistent* postevent information (Bekerian & Bowers, 1983, 1984), and these show a similar, but reversed, experimental effect to that obtained in studies showing the paradigmatic postevent information effect using inconsistent postevent information.

#3.4 Integrating Information in Memory Across Different Modalities.

Experiments in #3 often present original event information and postevent information in different sensory modalities. In the experiment that serves as exemplar for the paradigm (Loftus et al. 1978), original event information is embodied in a photographic slide, while postevent information is presented as part of a questionnaire. Still more clearly, in Power, Andriks & Loftus (1979), original event information is presented visually (slide) and postevent information is presented verbally (spoken narrative). Loftus' contention that postevent information effects typically show *integration* of original event information and postevent information into a single memorial representation (see Chapter 1) is thus also the claim that memories acquired through different sensory modalities are integrated into a single code in memory. This claim has attracted a substantial amount of attention from experimental cognitive psychologists (not surprisingly, as the nature of memorial representation has long been one of the central areas of dispute in cognitive psychology - see, for example, Paivio & Begg (1981)). A number of experiments in cognitive psychology outside of the eyewitness paradigm directly explore the phenomenon of memorial integration *apropos* of the issue of the nature of memorial representation.

Thus, Pezdek (1977) presented subjects with photographic slides of line drawings and then at a later stage presented them with verbal descriptions, some of which were semantically *relevant* to the slides previously seen, and likewise, some that were semantically *irrelevant* to the slides seen. Descriptions that were semantically relevant always changed at least one element of the original line drawing, thus technically constituting *inconsistent* postevent information.

For example, one of the drawings in Pezdek's experiment showed a motorcar (without ski-racks) parked next to a tree. One of the verbal descriptions in the second phase of the experiment read:

The car by the tree had ski-racks on it.

Subjects were then given a recognition test of their memory for the original event in which they were required to indicate which of two slides presented to them was in the original slide set. One slide corresponded to the original line drawing, the other to the incorrect description of the line drawing provided in the postevent information phase of the experiment. In the example above, subjects were shown the *original* slide of the car next to a tree, alongside a slide showing the same car, *with ski racks*, next to the same tree.

Pezdek's results showed that intervening questions led to lower test performance ie. subjects were less likely to correctly identify material originally seen when they had subsequently been exposed to material inconsistent with that originally seen. Pezdek's (1977) study is thus a *replication* of the classic finding of #3 (Loftus et al. 1978).

Several other experiments in #3.4 utilize a procedure very similar to Pezdek's[7], and likewise report replications of the paradigmatic postevent information finding (Rosenberg & Simon 1977, Gentner & Loftus 1979).

Results of these non-simulatory postevent information experiments are typically interpreted as evidence that new information may be integrated into existing memories. That this happens across sensory modalities viz. that information obtained through a different sensory modality than that through which original information was obtained may be integrated into memories of the original event is said to show that the code in which memories are represented is a unitary, semantic code. This conclusion is quite clearly tangential to what we are concerned with here, but it is, nevertheless, perfectly consistent with Loftus' claim that postevent information may be integrated into the memorial representation of an event. Whether this integration is destructive ie. alters the memory of the original information, though, is an issue not addressed in #3.4.

#3.5 The Effect of Hypnotic Induction on the Classic Postevent Information Finding.

Two questions have been of especial interest to investigators here:

(i) whether people in hypnotic states are unusually susceptible to the postevent information effect.

(ii) whether *suggestibility* (as indexed by scores of proneness to hypnotic induction) affects susceptibility to postevent information.

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7. I mean that they look specifically at integration across sensory modalities.

Investigators here have stuck fairly closely to the paradigmatic methods considered earlier. Indeed, virtually all the studies in this paradigm have modelled their procedures directly on Loftus et al's (1978) study, to the extent that the materials for all but a couple of the hypnosis studies were borrowed from Loftus for the execution of the experiments.

The answer to question (ii) seems to be that subject suggestibility does not affect subject susceptibility to misleading postevent information (Sheehan & Tilden 1983). Subjects who scored highly on hypnotic measures of suggestibility were no more likely to be misled than subjects who scored lowly.

The answer to question (i) is not nearly as clear. While Sheehan, Grigg & McCann (1984) and Sheehan & Tilden (1984) report that subjects in hypnotic trances are more likely to be susceptible to misleading postevent information, Yuille & McEwan (1985) found no such effect.

With respect to the enduring finding of #3 viz. that misleading postevent information may affect performance on tests of memory for the original event, #3.5 corroborates this. In all the studies that constitute the paradigm, subjects exposed to misleading postevent information performed at levels significantly lower than subjects who were not exposed to the misleading information.

#3.6 The Effect of Gender and Personality Variables on Susceptibility to Postevent Information.

In a 1979 study (Power, Andriks & Loftus (1979)), Loftus and associates makes the observation that the postevent information effect is usually not very large (roughly speaking, it is in the order of a 20 - 25% difference between control and experimental groups; but see chapter 6 for a more formal measure of the effect). This she takes to show that only certain people are susceptible to influence by postevent suggestion, and further draws the conclusion that knowledge of the personality variables that affect susceptibility to postevent information would be of considerable interest to criminal practice ie. it would allow officials to identify suggestible witnesses.

Four personality variables have been addressed directly in reported postevent information research. These are (A) gender of the witness; (B) age of the witness; (C) 'neuroticism' of the witness; and (D) the witness' personality type (using Jungian classifications).

(A) GENDER OF THE WITNESS

Several early studies claimed that men were better eyewitnesses than women - specifically, that men were less prone to suggestion than women:

> ...suggestive questions ...[of this sort]... operate with especial force in the case of young and uneducated persons; more with women than with men. (Stern 1910. p 273)

Power, Andriks & Loftus (1979) addressed this claim directly in their 1979 study. They showed that gender related differences found in typical postevent information scenarios were *item specific.* In particular, male subjects tended to be more easily misled about female items (eg. dress of women actors in a staged crime scenario) than about male items; and similarly, female subjects were more easily misled about male items than female items.

The effect, in any case is only marginal and the issue of gender has attracted no further attention in the study of postevent information.

(B) AGE OF THE WITNESS

Ever since Münsterberg's pioneering work in the early twentieth century, researchers have concerned themselves generally with the question of the effect of age on witness reliability. As far as the study of postevent information is concerned, the question of interest has been whether children are more prone to suggestion by postevent information than adults.

A substantial literature exists here, which is not relevant enough to warrant extensive attention. Loftus & Davies' (1984) review of the literature notes that although some studies show that children *are* more prone to suggestion than adults, others show just the reverse viz. that children are less susceptible to suggestion than adults.

The literature simply isn't consistent enough in any case to allow us to draw conclusions about the effect of age on witness suggestibility.

(C) NEUROTICISM

Very few studies are reported in the postevent information literature that directly examine the contribution of personality variables to suggestibility to postevent information. Of those that exist, one examines the effect that neuroticism has on suggestibility to misinformation. Zanni & Offerman (1978: experiment 2), examined the relationship of neuroticism to susceptibility to postevent information, using the procedure developed by Loftus & Palmer (1974). Subjects were shown films depicting a number of incidents after which they were interrogated with suggestive questions (using, as in Loftus & Palmer (1974), the indefinite article). Subjects also completed the Eysenck Personality Inventory. It was shown that subjects who scored higher on measures of neuroticism were more susceptible to postevent information. This was explained by positing that neurotics have higher waking arousal levels (an idea ascribed by the authors to H. Eysenck), which would interfere with their ability to concentrate, thus rendering them more susceptible to postevent information (Zanni & Offerman, 1978.).

(D) PERSONALITY TYPE

In the second of the personality studies, Loftus & Ward (1985) examined the relationship between Jungian personality type and suggestibility to postevent information. Using the same procedure and materials as Loftus et al. (1978), experiment 1, they found that Introverts and Intuitives (and Introvert-Intuitives)[8] are more prone to accept both misleading and consistent postevent information.

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8. Personality types were classified using the Myers-Briggs inventory.

For all three of the research areas subsumed under #3.6, experimental effects, when observed, were marginal. Little attention has been paid to the issues in the literature, and certainly even less theoretical attention has been directed at the findings made in #3.6. They are included here for the sake of completeness.

#3.7 Presenting Misleading Information About Colour: the Notion of Memory Blends.

Postevent information experiments typically measure the effect of postevent information by the ability of subjects to correctly choose one of two items in a forced choice test. Changes in memory using this methodology can only be measured *discretely*. For example, in Loftus et al. (1978) the postevent information effect was shown by the fact that subjects incorrectly recalled having seen a *yield* sign where the original information was a *stop* sign. That this change is discrete is certainly to be expected: either the thing to be recalled is a stop sign *or* it is a yield sign, no intermediate is possible.

It is not necessarily the case that changes in memory, if they do occur, should be discrete. In cases where it is possible to *integrate* original and postevent information, it may be that memories are formed that correspond to integration of the two pieces of information. Should we expect memory blends in cases where original event information and postevent information are not discrete units of information? Loftus (1977) reports a number of experiments that examine this issue.

Loftus showed subjects 30 colour slides depicting an autopedestrian accident. She then administered a questionnaire to the experimental group, which included the false suggestion that one of the cars seen in the slide sequence was blue (which, in fact, was green). The questionnaire given to the control group included no such suggestion. After a filler activity all subjects were administered a colour recognition task in which they had to name the colour of 10 objects seen in the slide sequence, one of which was the green car misled for in the questionnaire. Subjects named colours by indicating their choice on a colour wheel constituted by a collection of 32 distinct colour strips. This procedure allowed Loftus to determine changes in recollection far more sensitively than in previous recognition tasks.

Performance of the experimental group in this experiment was extremely interesting: instead of uniform, discrete changes in recollection on the part of subjects in the experimental group, Loftus found a significant overall shift in subjects' memory for the colour of the car from green to blue ie. subjects tended to recall the colour of the car as that colour on the colour wheel which we might call green-blue.

For Loftus this constitutes sure evidence that postevent information may truly be integrated into the memorial representation of an event[9]. Original event information (a green car) and postevent information (a blue car) seem to have blended in the final memorial recollection (a green - blue car).

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9. Indeed, in the dispute with McCloskey & Zaragoza, which I cover in chapter 5, she uses the findings from this experiment to assert that postevent information experiment findings are not artefactual.

In a second experiment reported in the 1977 study, Loftus repeated the procedure for the first experiment, with a few modifications: half of the subjects completed the colour recognition task before being given the misleading postevent information, and later again completed the colour recognition task. Subjects not given a preliminary colour recognition task proved to be far more difficult to influence and these subjects performed better than experimental subjects on the final task. This is said to show that commitment to a proposition about a state of affairs facilitates resistance to suggestion about that state of affairs.

This examination of the effects of postevent information on memory for non-discrete pieces of information has (unfortunately) not been repeated.

#4 STUDIES THAT ELIMINATE THE POSTEVENT INFORMATION EFFECT

In this paradigm of postevent information research we are concerned with studies that fail to produce the effect that characterizes the typical postevent information experiment. Some of these studies severely restrict the generality of the misinformation effect across materials and scenarios, and others eliminate it entirely. This latter class of study has important implications for both the practical and theoretical implications of the misinformation effect, which are discussed in detail in two later chapters.

#4.1 Warning Studies.

In their 1982 paper, Greene, Flynn & Loftus (1982) directed themselves to the question of whether marking the misleading postevent information by explicitly warning subjects of the misleading nature of the information would eliminate the postevent information effect observed in earlier experiments.

There are two clear ways in which the warnings can be introduced into the paradigmatic postevent information experiment. The first way is by warning subjects about the nature of the postevent information *before* the postevent information is presented. This allows subjects to identify and thus ignore misleading information. The second manipulation is to warn subjects about the nature of the postevent information after the postevent information has been presented. (If the assertion that postevent information alters memory is correct, then memory should have been altered by the time subjects receive the warning, and the warning should therefore be ineffectual).

In Greene et al's study, subjects were shown a slide sequence depicting a wallet snatching incident, and were then exposed to misleading postevent information (in the form of a narrative ostensibly written by a police cadet) either *before* or *after* having been warned that the postevent information was misleading. They were later tested with the paradigmatic final forced choice recognition test. Subjects warned *before* the presentation of misleading information were more accurate on the final recognition test than subjects warned *after* the presentation of the postevent information, but were still considerably less accurate than subjects not exposed to postevent information at all.

In another experiment reported in the same study, Greene et al. attempted to identify the mechanism responsible for the modulation of the postevent information effect by the warnings. This experiment incorporated measures of the time subjects took to read the postevent information. Subjects warned about the nature of the postevent information prior to the presentation of the misinformation took significantly longer to read the narrative embodying the postevent information than either control subjects or subjects not warned prior to the presentation of postevent information. Greene et al. speculate that the increased time that subjects took to read the narrative enabled a closer scrutiny of the postevent information on the part of those subjects, which explains why these subjects outperformed experimental subjects not given the benefit of this manipulation.

The authors draw the conclusion that warnings do not constitute very effective resistance to suggestion by misleading postevent information. This contention, however, is one not borne out by subsequent research, as we shall now see.

One of the conspicuous weaknesses of Greene et al's study is the vagueness of the warning given to subjects. Indeed, this is something that the authors of the study acknowledge:

> In the current research the warnings were broad and general. More specific warnings may have been more successful in changing the subject's approach to the task. (p 218)

Consequently, the study leaves the question of whether a warning to subjects will eliminate the postevent information effect, unanswered. Christiaansen & Ochalek (1983), realizing this, conducted a series of experiments in which subjects were quite explicitly warned that some of the information given to them was false.

As in previous experiments, subjects were shown a slide sequence (this time depicting a shoplifting incident); misled in a subsequent narrative about certain details in the slide sequence; and later given a forced choice questionnaire to assess recognition accuracy. The modifications in their experiment included:

(1) inserting an initial accuracy test into the design immediately after the original slide sequence (so that estimates of memorial accuracy could be based on information positively encoded in memory - an important methodological innovation[10]).

(2) warning subjects explicitly after presentation of postevent information that some of the information given them was false. The exact wording of the warning read:

> ...a few of the details in the description of the slide sequence, which you read at the beginning of the hour, were inaccurate - some of the details are correct and a few are incorrect. (p 469).

Subjects who *were* warned about the misleading nature of the postevent information they had been exposed to *after* the presentation of the postevent information performed no worse on the final recognition test than control subjects. That is, warning subjects explicitly about the untrustworthiness of the postevent information they had been exposed to,

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10. See Chapter 6, where I report empirical work of my own, utilizing this innovation.

eliminated their tendency to incorporate the misleading information in subsequent memorial reports.

Christiaansen & Ochalek's study, then, presents an example of an experimental manipulation that eliminates the postevent information effect observed in other experiments.

#4.2 The Pragmatic Isolation of Postevent Information in Postevent Information Studies

One of the strongest criticisms of postevent information research is that the experiments typically used to demonstrate the effects of postevent information suffer from 'pragmatic isolation'. This is a point Dodd & Bradshaw (1980) make:

> In experiments of the kind described above, it is not clear what intention the speaker has in asking a leading question since it is not clear who the speaker is nor what purposes that speaker might have. (p 696)

Using a simulatory scenario then, far from being the 'ingenious manipulation' that Neisser (1983) takes it to be, presents serious problems to the ecological realism of experiments conducted in the postevent information paradigms. In particular, the type of simulatory scenario used in postevent information studies renders subjects unable to bring a type of independent evidence to bear on the misleading messages which they would under ecologically real conditions. This is precisely because the scenarios used in postevent information experiments inadvertently eliminate this evidence by not including the pragmatic context of the postevent information.

Dodd & Bradshaw (1980) note that real eyewitness scenarios *always* involve pragmatic content and that witnesses are likely to use their attributions about the source of any information they might acquire after an event, especially if it is likely that the source may have vested interests in the event to be remembered.

To demonstrate that the pragmatic content of misleading information influences whether subjects will replace original event information in memory, Dodd & Bradshaw (1980) modified the typical #3 experiment so that the postevent information was pragmatically embedded.

Subjects were shown a slide sequence, similar to that often used by Loftus, portraying an autopedestrian accident. They subsequently answered a series of questions about the event. These questions varied according to the group subjects had been assigned to. In the *control* group, questions contained no false information; in the first experimental group (*Presupposed - Unspecified Source*), some of the questions contained false assertions, and finally, in the second experimental group (*Presupposed Specified Source*), some of the questions again contained false assertions, but the questionnaire instructions warned subjects that the questions were asked by a lawyer representing the driver of the car causing the accident. Later, all subjects answered a final recognition questionnaire which asked direct questions about the film originally seen.

Embodying the postevent information pragmatically in this way eliminated the misinformation effect: subjects given misleading postevent information, but also given details of the linguistic origin of the misleading postevent information, were no more likely to include misleading suggestions in subsequent memory recall than subjects not given any misleading suggestions in the first place. However, subjects who were exposed to misleading postevent information, but not to the pragmatic origin of the misleading postevent, showed the same tendency observed in previous experiments, namely to incorporate the incorrect information in subsequent recall.

This experiment (and very similar results from a second experiment reported in the same study) at the same time replicates the findings made by Loftus and others in earlier research *and* demonstrates serious problems with the simulatory method usually incorporated in typical postevent information experiments. In particular, the process of acquiring information in real life scenarios seems clearly different from the way information is acquired in Loftus' laboratory.

#4.3 Blatantly Contradictory Postevent Information.

#4.1, as I have shown, varies the sociolinguistic context of the postevent information presented to subjects. #4.2, on the other hand, shows how the effect may be eliminated by varying the *content* of the misinformation that subjects are exposed to.

To what extent is the misinformation effect restricted to 'plausible contradictions'? Is there a restriction on the kinds of subsequent information that may be incorporated into existing memories? Loftus (1979c) attempted to answer this question by using postevent information that blatantly contradicted original event information. Subjects were first shown a slide sequence depicting a wallet snatching incident, then given an accuracy recognition test, after which they were required to read a narrative ostensibly written by a Psychology professor. This narrative contained one piece of blatantly contradictory postevent information (the suggestion that a *red* wallet seen in the slide sequence was *brown*), and three other subtler pieces of misinformation. After reading the narrative, subjects completed a final recognition test.

Loftus found that subjects given blatantly contradictory information uniformly rejected it. Not only this, but subjects who successfully rejected the blatant contradiction were also less susceptible to subtle misinformation than subjects not exposed to blatantly contradictory postevent information. Furthermore, subjects who were accurate on the initial accuracy test (given before presentation of postevent information in this particular experiment) were also more able to resist suggestion. This tendency was more pronounced for subjects also exposed to blatant contradictions.

In a second experiment reported in this study, Loftus found that the "carry over" effect of blatantly contradictory postevent information (ie. the tendency for subjects exposed to blatant contradictions to resist subtle contradictions) is eliminated by delaying the presentation of blatant contradictions until after the other, subtler, misinformation has been processed.

Loftus' explanation for the set of findings from #4.2 is that blatantly contradictory postevent information makes subjects discount and more closely scrutinize the body of information in which the postevent information is embedded, resulting in the rejection of false information by subjects.

#4.2 then, shows that there is a limit to the type of misinformation that may be incorporated in witness recollections. In particular, misinformation that is an implausible candidate for substitution is uniformly rejected by subjects.

#4.4 Retrieving Information by Enhancing Retrieval Cues.

One of the conspicuous features of postevent information experiments is the rigid way in which measures of memory are taken. Typically, the postevent information effect is taken as the difference between an experimental and control group on an item in a forced choice recognition test. The recognition test usually takes the form of a multiple choice questionnaire or a two option forced choice slide recognition test. The items that constitute the questionnaire and the slide test are almost always a random combination of items from the original materials. As a random combination of items, the forced choice tests constitute a rather poor retrieval environment, and certainly a most contrived simulation of ordinary retrieval conditions. The misinformation effect may thus be consequent upon the impoverished retrieval conditions introduced by forced choice test structures. Differences between experimental and control groups may well disappear with an enriched retrieval environment.

Bekerian & Bowers (1983, 1984), following this line of reasoning, devised an ingenious and very simple modification to the prototypical #3 experiment. In the paradigmatic #3 experiments, the slide sequence presented to subjects is always a series of slides that forms a 'story': in Loftus et al. (1978), for example, the story concerns an autopedestrian accident. The position of individual slides within the sequence is consequently of thematic importance (as a 'story' is constituted by a *sequence* of events.) However, in the forced choice recognition task of the typical postevent information experiment, the test slides are a random subset of the original set. As the 'story' behind the slide show is constituted by a sequentially presented set of slides, important thematic cues will be lost by the practice of creating a forced choice test with a random selection of slides. To test the hypothesis that the postevent information effect is consequent upon an 'impoverished' retrieval environment, Bekerian & Bowers (1983, 1984) restructured the recognition test so that the slide sequence presented in the test matched the set presented originally in terms of the sequence of the slides. Retrieval conditions thus thematically resembled encoding conditions. They then repeated Loftus et al's (1978) experiment, using exactly the same materials as used in the original experiment. Two experimental groups were used: the first group was tested using Loftus et al's original random slide sequence test, and the second using the modification outlined above.

Results were precisely as predicted: restructuring the recognition test so that the test slide set matched the original slide set sequentially, eliminated the postevent information effect - subjects given inconsistent information performed no worse on the recognition test than subjects given consistent postevent information. (In addition, the paradigmatic postevent information effect was replicated in the procedure utilizing Loftus' original test structure, showing that results obtained with the revised test structure were not due to other critical changes to the methodology).

Following this ingenious piece of experimental work up in a subsequent study, Bekerian & Bowers (1984) extended the design of the experiment so that the questionnaire in which the postevent information was embedded also fell under the random/sequential manipulation ie. the questionnaire which introduced the postevent information either matched the sequence of the slides originally shown to subjects, or was a random combination of questions relevant to the original slide set. Thus, a subject could be exposed to consistent, or inconsistent postevent information in a random, or sequential questionnaire, and later be tested with a recognition test which constituted a sequential, or random, match of the slides originally presented.

Bekerian & Bowers replicated the finding made in the earlier experiment viz. that a sequentially ordered test eliminates the postevent information effect hitherto observed, *but* found that when the questionnaire presenting the postevent information matched the original slide set sequence, the postevent information effect reappeared regardless of the test conditions subjects were assigned to.

Bekerian & Bowers show forcefully that ostensiby irretrievable information may be retrieved with relatively minor methodological changes. Their demonstrations are not alone: a number of experiments show retrieval of information ostensibly destroyed by postevent information. One such experiment, akin to Bekerian & Bowers' work, is reported by Kroll & Timourian (1986).

Kroll & Timourian eliminated the postevent information effect by extending the paradigmatic design so that two (out of three) subject groups were given enhanced retrieval cues by reinstating some of the original encoding conditions.[11]

Subjects first saw a slide set depicting a purse snatching incident, and were then given a questionnaire, three items of which had embedded in them false postevent information. Subjects completed a forced choice recognition test, after which they were shown a slide sequence depicting the physical location of the original slide sequence (ie. the same location as that used for the original slide sequence, without the actors). Subjects were then warned that the questionnaire they had been exposed to had contained false information, and were given a further recognition test. This recognition test, as in the paradigmatic test, paired original event information and postevent information.

Kroll & Timourian found that returning subjects to the scene of the crime (ie. enhancing retrieval cues) substantially reduced the extent of the incorporation of misinformation into memory, although this manipulation was not entirely effective.

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11. That reinstating the original encoding conditions enhances retrieval is, of course, a well documented finding in experimental psychology (Abernathy, 1941).

#4.5 Reconstructing the Recognition Test: Pairing Original Event Information and Information not Previously Presented

Weinberg, Wadsworth & Baron, in their 1983 study, introduced an innovation into the postevent research paradigm that is a central part of the methodological dispute to be addressed later. This innovation resides in the restructuring of the paradigmatic forced choice test so that original information is paired with information not seen before. For the meanwhile I will leave the methodological ramifications of this innovation undiscussed, but will return to them later.

Weinberg et al. repeated Loftus et al.'s 1978 study closely, adding to it a factorial manipulation of test structure. One level of this additional factor was constituted by the paradigmatic test structure (ie. OEI vs PEI [Stop vs Red Yield]), and the other by a revised test structure in which original event information was paired with information not seen in the course of the experiment [Stop vs Yellow Yield]. Weinberg et al. justify this extension by reasoning that if memory really is altered by the presentation of postevent information (as Loftus claims), then subjects should perform as poorly on the new recognition test as the old, precisely because the memorial representation of the original event information has been destroyed. However, if subjects perform differentially on these tasks, then this would seem to indicate that demand characteristics are at operation in the experiment: in particular, it may be that the *yellow yield* sign seen by subjects in the postevent information phase of the paradigmatic postevent information experiment acts as a subtle cue to subjects, informing them that the experimentally desired response on the forced choice task is for them to choose the sign.

Experimental subjects in this experiment performed at lower levels on a subsequent recognition test than control subjects, even when the task was modified so that original event information and postevent information were not paired. However, performance of subjects across the two types of recognition task (ie. *original event information vs postevent information,* and *original event information vs information not seen before*) was *not* equivalent - subjects tested with the second of these tests were *more likely* to remember the original information than subjects tested with the original test. Weinberg et al. concluded that this seems to demonstrate the operation of some demand characteristics in the experiment, but at the same time confirms the existence of the postevent information effect.

As we shall presently see, the results of Weinberg et al.'s study are somewhat surprising when considered alongside McCloskey & Zaragoza's study, which proceeded in a virtually identical manner. Indeed, McCloskey & Zaragoza argue in their analysis of the Weinberg et al. study that the *yellow yield* sign used by Weinberg et al. is so much like the original information in the slide set (*red yield sign*) that it introduces unnecessary confounding factors into the experiment (see McCloskey & Zaragoza (1985a)).

#4.6 Extending the Generality of the Postevent Information Effect

We have seen in this section of the literature review that several studies challenge findings reported by Loftus and her coworkers. Some of these studies eliminate the misinformation effect entirely, and others restrict the claims originally made by Loftus about the pervasiveness and extent of the effect. What do these studies tell us about the experimental validity of postevent information studies? Do the studies categorized here under #4 explore the important, but necessary limits of a theory? Or do they demonstrate that the misinformation effect is largely dependent on a specific experimental procedure and set of materials? Read & Bruce (1984) report four experiments that directly examine these issues. In all four of the experiments prototypical #1 and #2 methodology is followed.

In experiment 1, Read & Bruce tested for generality of the postevent information effect across materials by questioning groups of subjects about a total of 24 different types of objects. They found that the misinformation effect, present at the usual effect level (15 - 20 % of the subject population), showed virtually no variation across the different stimulus items. They concluded that the experiment shows "..the broad generality of the effectiveness of leading questions across ... objects of different characteristics" (Read & Bruce, 1984; p 37).

In experiment 2, Read & Bruce tested the effects of familiarity with the physical locale of a witnessed event on the effects of leading questions, as produced by #1 methodology. Groups of students *familiar* and *unfamiliar*, respectively, with an area on a university campus, were shown film clips of a car accident on the campus, and were later asked to estimate the speeds of the vehicles involved in the accident. Subjects were questioned, as in the Loftus & Palmer study, with verbs that suggested different speeds.

The results of the experiment showed that subjects who were familiar with the physical location of the witnessed event were virtually unaffected by the suggestions embedded in the questions put to them, whereas subjects unfamiliar with the location reproduced the same 'leading question' effect originally obtained by Loftus & Palmer (1974).

In experiment 3, Read & Bruce examined the effect of degree of attention paid to the witnessed event on susceptibility to false presuppositions (ie. #2 methodology), using Craik & Lockhart's (1972) 'levels of processing' framework. Subjects were (again) shown a film clip and then asked (i) to count the number of camera perspective changes in the film (shallow processing), or (ii) to evaluate the 'dramatic/eventful' nature of the film clip on a Likert type scale (deep processing). Later, subjects were required to complete a questionnaire, in which several questions presented misleading presuppositions. Subjects in the 'deep processing' condition failed to incorporate the misleading details in their subsequent memory reports, but subjects in the 'shallow processing' condition replicated the effects reported in the paradigmatic #1 studies. Degree of attention, they conclude, is a crucial variable in the transmission of the misinformation effect.

In experiment 4, Read & Bruce set out to determine whether knowledge of postevent information research itself would regulate the misinformation effect. College students were given a lecture on postevent information research one month prior to a direct repetition of experiment 3, above. Those subjects who had specific knowledge about postevent information research were less susceptible to the 'leading question' effect than subjects who didn't have this knowledge.

Read & Bruce's study thus makes an important finding. Although the misinformation effect is reliable across different experimental materials, several ecological manipulations modulate the transmission of the effect. Laboratory investigations of the effect thus seem indefensible.

Although several investigators examine the generality of the misinformation effect, these examinations usually concern inanimate materials. One of the most important 'materials' in real life eyewitness scenarios however, is quite obviously the human face. Two studies of note exist that make an attempt to consider this important variable.

In the first of these, Loftus & Greene (1980) found that subjects exposed to misleading suggestions about the face of a person they had previously seen briefly were far more likely to incorporate misleading information in subsequent memory descriptions of the suspect, and subsequent composite photograph reconstruction of the face. In addition, subjects were also more likely to select the wrong face from a photographic identification array. (The experiments reported in this study (4 in all) are sufficiently close in procedure to other postevent information experiments that I shan't discuss them here in terms of the procedures used).

In the second of these studies, Jenkins & Davies (1985), the procedures used in Loftus & Greene(1980) were repeated more rigorously with much the same sort of results. An interesting additional manipulation here, though, was the presentation of postevent information. Here postevent information was presented (in one phase of the study) in the form of a misleading identikit[12]. This manipulation produced the expected effect: misled subjects incorporated the misleading suggestion in both *recall* and *recognition* tests (written descriptions of the suspect; and identification of the suspect from photographic arrays).

3. INTEGRATION

The published literature exploring the postevent information effect is large, as this review shows, and the integration of it is not a straightforward matter. Nevertheless, an integration is useful here, and in what remains of the chapter I attempt to draw together the results of research discussed in the review. (Note that I limit myself strictly to matters *internal* to the research: in the following chapter I evaluate postevent information research in terms of its applied orientation, and in Chapter 5 I turn to theoretical and methodological issues raised by postevent information research).

Paradigms 1 and 2, which examine the effect of question wording on eyewitness reports, appear to indicate that suggestions embedded in questions may bias answers in the direction of the suggestions. In addition, one experiment (Loftus & Palmer, 1974) seems to show that suggestive question wording may change a subject's *conception* of an

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12. 'Identikit' is the tradename for a composite facial reproduction system often used by criminal branches in police forces. According to Jenkins & Davies, they are a particularly powerful source of misinformation, as it is rarely the case that such composite reconstructions adequately resemble the offender in question.

event (in the direction of the suggestion). Nevertheless, there is some dispute about the validity of these results, as two studies fail to reproduce the findings (Zanni & Offerman, 1978; Read et al. 1978).

Experiments from paradigm 3 (which extends the question pursued in paradigms 1 and 2 to that of 'memorial alteration') report fairly consistently and reliably that postevent information presented to subjects may be incorporated in subsequent memory reports. Several things are shown to modulate this effect. Delaying the presentation of misinformation until well after the original observations is particularly effective in producing the misinformation effect (Loftus et al. 1978). Presenting subjects with postevent information consistent with what they originally observed reverses the misinformation effect (ie. *increases* the chances of remembering original information) (Loftus 1975). Hypnotic indexes of suggestibility are *not* related to the tendency to incorporate misinformation in memorial reports (Sheehan et al. 1983, 1984), but people in hypnotic trances may be unusually suggestible (Sheehan et al. 1983, 1984, although this is disputed by some (Yuille & McEwan, 1985)). Gender of the subject is unrelated to the general tendency to be misled by postevent information, but is related to the tendency to be misled for *specific* items ('sex - characteristic items') (Power et al. 1979). Certain personality types seem to be more easily misled, especially 'introverts' and 'intuitives' (following Jungian classifications) (Ward & Loftus, 1985). Finally, some evidence exists that suggests that subjects may 'blend' information from different sources into a composite recollection (where the information to be blended can be represented as a composite) (Loftus, 1977).

Paradigm 4, which concerns itself by and large to show that Loftus' theoretical interpretation of postevent information study findings are incorrect, establishes important (and troubling) limits to the misinformation effect. Thus, warnings about the misleading nature of postevent information given to subjects after presentation of the postevent information serve to eliminate the misinformation effect (Christiaansen et al. 1983). Embedding postevent information sociolinguistically (ie. in terms of the intentions of speakers) also eliminates the misinformation effect (Dodd & Bradshaw, 1980). In addition, only certain types of postevent information are effective in producing the misinformation effect (ie. information that doesn't blatantly contradict original information) (Loftus, 1979). The effect is eliminated in cases where subjects are familiar with the physical location of the represented event, where the degree of attention paid to the misinformation is greater, and where subjects possess specific knowledge about postevent information research (Read & Bruce, 1978). Finally, restructuring the final recognition test so that original and postevent information are not paired, is reported to attenuate, but not eliminate, the misinformation effect (Weinberg et al. 1983) (but see McCloskey & Zaragoza's argument on this point in a later chapter).

It is clear from this that the misinformation effect is a well replicated phenomenon. However, the studies in paradigm 4 draw attention to several unsatisfactory features: the fact that the phenomenon is eliminated by a number of manipulations that attempt to situate postevent information in more ecologically real circumstances is particularly troubling.

CHAPTER 4: EVALUATING POSTEVENT INFORMATION RESEARCH

As I pointed out in the introduction to the dissertation, there are two central issues in the dissertation: one concerns what the study of postevent information tells us about applied research, and the other concerns the validity of results produced by postevent information experiments. In this chapter I provide an evaluation of postevent information research in terms of its applied orientation; in later chapters I will turn to the question of the validity of results produced by postevent information experiments.

1. INTRODUCTION

Postevent information research is first and foremost applied research: it derives, as I pointed out in Chapter one, from a contemporary concern with the reliability of testimony delivered by people who claim to have witnessed certain events. The theoretical implications of the research became an issue only after particular methods had been devised to answer the legal question informing the research. Despite the theoretical nature of the implications that Loftus (& others) have taken postevent information research to have, the question which postevent information experiments were designed to answer is an applied one, and the research is consequently best assessed in terms of its applied orientation.

The best place to begin the evaluation is with the formulation of the research problem.

2. FORMULATION OF THE PROBLEM

Elizabeth Loftus has always been adamant that psychological research into the reliability of eyewitness research has an important role to play in legal procedure. Specifically, she thinks that such research may serve the ends of justice by minimizing the possibility that an innocent defendant in a criminal trial will be convicted of crimes that he/she did not commit:

> What shall be done to protect against the dangers of a mistaken identification? ...[A] solution would be to allow the judge and especially the jury to hear an expert witness present psychological testimony about the factors that affect the reliability of eyewitness accounts... In this way the jurors would have information with which to evaluate the identification evidence fully and properly. (Loftus, 1979: p 191)

Loftus' argument assumes that innocent defendants are sometimes convicted for crimes that they did not commit on the basis of mistaken identifications. This is not a problematic assumption: indeed, she cites many cases in which defendants have been acquitted of crimes some time *after* conviction and prosecution. In her widely read rèsumè of eyewitness research, Eyewitness Testimony, for example, Loftus begins chapters 2, 4, 7, 9, 10, and 11 with accounts of cases in which people were mistakenly convicted on the basis of eyewitness identifications.

A second assumption Loftus makes is that this type of legal mishap occurs frequently enough to warrant legal concern and action.

> The number of mistaken identifications leading to wrongful convictions, combined with the fact that eyewitness testimony is accepted too unquestioningly by juries, presents a problem for the legal community. (Loftus, 1979; p 201)

The motivation of the research then seems to be a concern about false convictions that may rest on mistaken eyewitness identifications, and the contributions that psychology can make to this legal problem. The primary concern of the research is thus not whether 'memories' are indelible, but the conviction of innocent people that may occur as a consequence of the legal system's reliance on eyewitness identifications. The question of postevent information is relevant to the concern with the court's undue reliance on identification evidence insofar as it contributes to the possibility that someone will be convicted on the basis of a mistaken identification:

> Being told, either directly or in a more subtle way, that a particular culprit has a mustache or that a given car ran through a red light can lead a witness to believe that he knew this fact all along. (Loftus, 1979. p 104).

The questions to be answered by the research here are statedly of a practical orientation: the issue is the miscarriage of justice that arises from mistaken eyewitness identifications, particularly those mistaken identifications that may be due either to questions that lead the witnesses' answers, or to misinformation acquired after the event.

This is the stated problem for postevent information research. There is no more said by Loftus about the aims of her research than that it addresses the possibility that information acquired after an event might adversely affect the witnesses' recall of an event, and that it might as a consequence lead to a miscarriage of justice.

Expressed in this manner, there is no need for an answer to Loftus' question. The courts have often taken cognizance of the fact that information given to witnesses after an event may 'colour' their subsequent recollections. In South Africa, for example, Justice Boshoff made the following observation:

> ...perhaps most crucial of all [the contributors to unreliable testimony] is the extent to which the witness' original impression has been overlaid by subsequent suggestion and imagination. (R. v. Mputing 1960 (1) S.A. 785 (T); quoted in Hoffman & Zeffertt, 1983).

The problem of postevent information in legal procedure is a real one, but the formulation of the problem in postevent information research is inadequate. It adds nothing to the concern which the legal system already shows. Loftus' statement of the question to be addressed by the research seems to me an acute example of the problematic notion of 'applied science' that guides much applied research: the notion that it is sufficient to motivate applied research by identifying a practical problem, that all that is needed is a conceptual nexus with a real world problem.

This point is suitably demonstrated by the way the research problem evolves in the postevent information research tradition. Loftus extends the question asked about postevent information in ways that would suit a traditional cognitive psychological paradigm. Thus, we saw in the literature review that the questions which were addressed in the paradigmatic research program concerned (among other things) (i) the effect of the time interval between presentation of original event information and postevent information on experimental - control differences; (ii) whether blatantly inconsistent postevent information would attenuate the paradigmatic misinformation effect; and (iii) whether consistent postevent information would reverse the effect obtained when postevent information is inconsistent. In short, the problem of postevent information is treated in an abstract, theoretical way; 'postevent information' is treated as a theoretical construct and not as a practical problem faced by legal personnel.

If we are going to take seriously the dangers that postevent information presents to the just conduct of criminal trials (which is Loftus' stated concern), then we need to specify the problem(s) to be addressed far more rigorously. We need an analysis of the process that leads up to the testimony of an eyewitness in court; the identification of stages in this process at which witnesses may be exposed to postevent information; an evaluation of the methods courts presently use to guard against the possibilities of postevent suggestion - and this is certainly only a beginning. I made this point in different circumstances in chapter 2, when I indicated that for applied research to be successful, researchers need to set up an *infrastructure* with the people or organizations that the research is aimed at. What is needed is a system that *enables* application, and an adequate problem specification is the first step towards this goal. Adequate problem specification in a serious applied program must proceed with a thorough analysis of the problem in *material* terms, not in the very general manner that it does here.

I drew attention a little earlier in this section of the chapter to the lack of a clear analysis of the legal problem of postevent information. The exclusive reliance on experimental methods seems to me to be one of the reasons for this state of affairs. Experimental methods have the disadvantage of 'cloaking' research with a degree of scientific respectability: even though a piece of research may be sadly wanting conceptually and theoretically, if it is experimentally sound then it may pass as sound research.[1] It is tempting to think that postevent information research has passed into cognitive textbooks as some of the best contemporary psychological research in just this way: because the methods are experimentally elegant, and because what looks like a substantial research paradigm has grown up around the original work, the virtual conceptual ignorance of the research has passed by unnoticed.

This is not to say that postevent information research has proceeded without questions in mind, because such an activity is unthinkable, but that postevent information research has proceeded *without an application in mind*, which is the readier measure of the value of research conducted in an applied programme.

3. METHODS UTILIZED.

The feature of postevent information research that singles it out from many cognitive psychological paradigms is its extensive use of the method of simulation. Ulric Neisser, for one, describes the method as ingenious (Neisser, 1983: p

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1. This is not my insight: J.G. Taylor made the point thirty years ago (Taylor, 1958).

15), and several cognitive psychology textbooks draw attention to this feature of the research. I want to suggest in this section of the chapter that 'simulation' is far from being a satisfactory experimental method. Later in the section I shall have something to say in general about the methods that researchers have used to address the problem of postevent information.

3.1 The Problem of Simulation

In the typical postevent information experiment subjects are usually shown a film or a slide show depicting a series of events which is a *simulation* of an event that could occur as a real life scenario, and on which people could be called to testify as witnesses. In Loftus, Miller & Burns (1978), subjects were shown a slide show depicting an autopedestrian accident; in Loftus (1975), subjects were shown a motor vehicle accident which occurred on a freeway; and in Loftus (1979c), subjects were shown a slide show in which a petty thief steals a woman's purse. In each of these examples, eyewitnesses are 'created' from the simulation of a 'likely' physical scenario.

Although Loftus never directly discusses the reasons for using 'simulation', it seems clear that the method is used to ensure that experiments conducted bear a sufficient resemblance to real life witness scenarios. It secures a measure of 'external validity', or generalizability, for the research.

Useful as it may seem, simulation brings with it several problems. The crucial problem concerns what is to be simulated. Is there a paradigmatic 'witness scenario'?

This is an important question, because the generalizability of experimental research is dependent on such a state of affairs.

There may be good reason to doubt whether 'real life' witness scenarios are paradigmatically related. An eyewitness, in legal terms, is simply one who claims to "have perceived [the event in question] with his own senses." (Hoffman & Zeffertt, 1982, p 463). Is there any justification for treating 'witness' as a category concept? There need be nothing in common between acts of witnessing that can be usefully addressed at the level of categories of events. The acts of witnessing a rape, an armed robbery, or a petty theft need have nothing more in common than that somebody claims to have observed them, and yet they may differ in countless other ways.

In short, there's no guarantee that the eyewitness scenarios created in Loftus' experiments are representative of the scenarios that courts have to deal with, simply because the courts may not be dealing with paradigmatic states of affairs. The method of simulation is thus no guarantee that the results of postevent information experiments can be generalized to the problems that courts (and other legal bodies) may be faced with.

It's an exasperating problem. That the traditionally favoured methods of empirical psychology may be entirely inappropriate when it comes to the question of witness reliability is particularly antagonistic to the claim that psychology can make a special contribution to legal matters by virtue of its utilization of empirical methods.

3.2 Other Problems with Research Methods

Although postevent information research never confronts the problem of what is meant by a 'witness scenario', and only makes half hearted analyses of the problem of generalizability, it does at several places address the question of the 'generality' of the postevent information effect. (In the literature review I drew attention to several studies that validated findings across different sets of materials (Read & Bruce, 1984; Loftus, Miller & Burns, 1978)). Although postevent information researchers address the problem of generality, postevent information experiments have used an unusually small range of materials. Some 80% of the experiments use, between them, three sets of materials: a set of slides depicting an autopedestrian accident (Loftus, Miller & Burns, 1978; Greene et al. 1982; Bekerian & Bowers, 1982, 1984); a set of slides depicting a wallet snatching incident (Loftus, 1977; Sheehan et al., 1983, 1984); and a film depicting a motor vehicle accident (Loftus, 1975; Loftus & Palmer, 1974). Nevertheless, several experiments that specifically address the question of the generality of the misinformation effect across experimental materials seem to demonstrate that there is no problem as far as this aspect of the researh is concerned.

What does seem limiting, though, is the exclusive dependence by researchers on traditional experimental methods. This is in spite of the contemporary concern in psycholegal circles about the appropriateness of experimental methods in applied legal studies (Konecni & Ebbesen, 1979; Konecni & Ebbesen, 1986). Konecni & Ebbesen (1979), for instance, suggest that archival research is far more promising than traditional methods - principally because archival research avoids the enormous problems of generalizability that face experimental investigations.

To conclude this section, then, the methods used in postevent information research are not unduly problematic in terms of the usual criteria on which research is evaluated. That is, the generality and reliability of the research seems satisfactory. However, the simulatory method adopted by postevent information research to ensure the representativeness of the research setting seems to me a cause for some concern, as does the exclusive reliance on experimental methods. The problems introduced by simulation seem especially troubling.

4. INTERPRETATION

I turn in this section to the question of what has been made of postevent information research. That is, what implications Loftus and her associates have taken postevent information research to have for legal practice.

In spite of the fact that Loftus has testified in a number of American courts on postevent information research, there is no major review of the applications that postevent information research sustains. However, Loftus (and other researchers) make remarks *apropos* of reported research on numerous occasions that suggest that postevent information research is highly relevant to legal practice. In what follows I attempt to piece together a number of these remarks into a *statement*.

Loftus, on page 104 of her 1979 rèsumè, says the following of postevent information research:

> These findings have implications for the legal process. Being told, either directly or in a more subtle way, that a particular culprit has a mustache or that a given car ran through a red light can lead a witness to believe that he knew this fact all along. Thus, a fact that is reported sometime after a critical incident along with the remark "I knew it at the time, but I just forgot to mention it" should be treated with some caution. Hindsight does not equal foresight. (p 104)

This is not a particularly insightful observation. That witnesses may introduce details into testimony that they failed to include at earlier stages is something judges have often commented upon. Indeed, in South African Law, for this very reason, if it can be shown that the witness saw the accused, or a photograph of the accused, then descriptions of the assailant given in court by the witness are treated with caution.[2] In the case of *Poopedi en ander v. S.*, for example, a conviction was set aside on these grounds.

In a similar vein, Loftus, Altman & Geballe (1976) comment

> The present data strongly suggest that the first investigator to interrogate a witness can substantially color the way a witness sees and reports the incident. One practical implication of the result is that the court should give due consideration, for each and every witness, to the issue of which side had the opportunity of the initial interrogation of that witness. (p 165).

Exactly what the application is here is particularly unclear. If the court were to give *any* weight to which side had the initial opportunity to interrogate the witness, then it would only be empowered to do so on the submission that an irregularity had occurred (such as an intolerably suggestive question). If it were standard practice to take

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2. Poopedi en ander v. S. 1966 (2) P.H. H407 (T)

cognizance of which side had the opportunity for initial interrogation, then cross examination, the court's sole method of evaluating evidence, would fall away. The only way in which the court could take into consideration which side had the initial opportunity for interrogation would be to take cross examination with a pinch of salt!

Published postevent information literature is replete with this vague and unhelpful type of speculation. At virtually no point in any of the published studies do researchers indicate precisely *which* aspect of legal procedure they have in mind. On page 78 of Loftus' 1979 major résumé of the research there is a particularly good example of this:

> Anytime after a witness experiences a complex event, he may be exposed to new information about that event. The new information may come in the form of questions — a powerful way to introduce it — or in the form of a conversation, a newspaper story, and so on. The implication of these results for courtroom examinations, police interrogations, and accident investigations is fairly obvious: interrogators should do whatever possible to avoid the introduction of "external" information into the witness memory. (p 78)

But exactly *where* in the sequence of events is this likely to occur and *what* can be done to counter it? Almost as baneful is the following paragraph, on p 73:

> ...most people have some ability to be swayed by the nonverbal communication that comes their way... A police officer may tell a witness that a suspect has been caught and the witness should look at some photographs or come to view a lineup and make an identification... It is here that nonverbal as well as verbal suggestions can easily be communicated. If the officer should unintentionally stare a bit longer at the suspect, or change his tone of voice when he says, "Tell us whether you think it is number one, two, THREE, four, five, or six," the witness' opinion might be swayed. (p 74, the emphasis is Loftus').

Here Loftus overlooks the fact that the court is well aware of the dangers of a police officer communicating the identity of the suspect to the witness. Indeed, in most countries (South Africa included) the court insists that the identification parade be conducted by a police officer not involved in the case, and who doesn't know the identity of the accused (Hoffmann & Zeffertt, 1983). In R. v. Y & Ano.[3], a conviction was set aside on the grounds that the officer who conducted the parade knew the identity of the suspect. There's little point then, in telling the legal community that an officer conducting an identification parade might communicate the identity of the suspect to the witness - this is already well known.

There is thus no systematic attempt in the postevent information literature to spell out the applications to legal procedure that the research sustains, and those attempts that are made are almost without exception vague and unhelpful. This seriously compromises the value of postevent information research. In his seminal 1978 paper, Wells puts the point briskly:

> ...in undertaking an applied project, it is incumbent on a researcher to demonstrate the applied utility of an eyewitness study. (Wells, 1978: p 1555)

Postevent information researchers have certainly failed to demonstrate the utility of postevent information research. In addition, they seem to have overlooked some elementary

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3.. R. v. Y & Ano. 1959 (2) S.A. 116 (W.L.D) legal principles in the few (and baneful) attempts made at 'applying' the research.

5. CONCLUDING REMARKS

I have had little complimentary to say thusfar in this evaluation of postevent information research as an applied psychological research paradigm. At this stage it may seem appropriate to urge research away from the problem altogether. This would be a considerable mistake (perhaps greater even than any identified in the dissertation), for the problem of postevent information is a very real one, and one readily within the reach of serious applied research. Nevertheless, if we wish to address the problem with a serious applied research program, we need to think clearly about the issues that 'application' raises. One useful beginning in this regard has already been published (Wells, 1978). In the final chapter of the thesis I will discuss Wells' contribution in some detail, and show how it may profitably change the approach to the problem of postevent information. (To continue the present chapter along these lines will pre-empt several important issues raised by the theoretical and methodological disputes addressed in the following two chapters).

CHAPTER FIVE: THEORETICAL AND METHODOLOGICAL ISSUES

In previous chapters I reviewed and evaluated postevent information research in terms of its applied orientation. In this chapter I turn to theoretical issues *internal* to the research, and then to the question of the validity of the paradigmatic experimental design.

1. THEORETICAL ISSUES: COEXISTENCE VS ALTERATION

1.1 Introduction

Paradigmatic postevent information experiments show that information acquired after an event may influence witness reports of the event. Where the information is *consistent* with what originally happened, witness reports are usually more accurate than they would have been in the absence of this information, and in cases where the information is *inconsistent* with what originally happened, the reports are usually less accurate. Originally these results were taken by Loftus to show that memory for an event may be *altered* by information acquired after the event. More recently, however, this contention has been made untenable by research that shows that ostensibly "irretrievable" memories can be made retrievable with relatively minor methodological changes. It will be as well to trace the theoretical development of this state of affairs here: this will serve as immediate introduction to the methodological dispute to be introduced later in the chapter.

1.2 Loftus' Position

Loftus' theoretical treatment of postevent information research is conceptually dependent on a collection of influential ideas known loosely as *schema theory*. These ideas are usually traced to Bartlett's (1932) work at Cambridge, although the ideas are certainly not uniquely or originally Bartlett's.

The starting point in this treatment of human memory is the rudimentary observation that memory is characteristically prone to incompleteness and distortions. That is, memory is not a storehouse of veridical traces of information, a repository for a 'devouring Cartesian eye'. What resides in memory is the result of a process of selection, and what is retrieved from memory is the result of a process of reconstruction. Both of these processes are dependent on preexisting mental structures or 'schemata'. Just how to define a 'schema' though, is a problem that has defied theoretical resolution and that draws the sharpest criticism from critics of schema theory (Brewer & Treyens, 1983; Alba & Hasher, 1983). Alba & Hasher suggest that in the broadest sense in which memory theorists use the term, a schema is the "...general knowledge that a person possesses about a particular domain." (1983: p 203).

Bartlett's original demonstrations of how schemata, defined in this way, seem to preside over memory processes remain some of the most convincing in the literature. Subjects that he gave a short narrative folktale to read (War of the Ghosts) tended to recall only an outline of the story, frequently distorting details in the direction of their pre-existing knowledge. Existing knowledge seemed to be strategically deployed in the encoding of new information.

Despite the intriguing results reported by Bartlett, and the long tradition of work after him, the idea of a schema remains theoretically rudimentary. Articulations of the idea abound in the literature, but there is little agreement. Alba & Hasher (1983), in their extensive review of schema theories, treat the problem of lack of theoretical clarity by outlining a 'modal theory', which is fairly close to Bartlett's position in Remembering. This modal theory postulates that the encoding of complex information is schema-driven, being characterized by four basic processes: *selection*, *abstraction*, *interpretation*, and *integration*. The theory is processual. Thus, from any environmental event, only the information relevant to a currently activated schema will be *selected* for encoding. Of this information the semantic content will be *abstracted* and the surface form will be lost. The semantic content abstracted in this way will be *interpreted* in a manner that is consistent with the schema, and the information that remains will be *integrated* with previously acquired, related information that was activated during the current encoding episode. The operation of any one of these processes will usually produce a memorial representation that is (strictly speaking) unfaithful in detail to the information embodied in the original event.

It should be clear from this brief outline that Loftus' theoretical position on the effects of postevent information is closely tied to the fourth of the processes described above. Information acquired after an event will be integrated with information already possessed and activated during the encoding process.[1] It is not necessary to demonstrate that Loftus takes bearings on schema theory, as Alba & Hasher (1983) argue this very convincingly.[2] It is enough to take note of the line that ties Loftus' theoretical claims to those laid down by schema theory, which is that the integration of new information into old knowledge structures makes accurate retrieval of original information highly unlikely (Alba & Hasher, 1983; p 212). Schema theory predicts that inaccurate retrieval will occur because individual traces of a to-be-remembered event do not exist (Alba & Hasher, 1983; p 212).

1.3 The Evolution of Postevent Information Theory

Loftus was sensitive to the implications that postevent information research findings may have for theoretical deliberations from the time of her 1974 and 1975 publications with Palmer and Zanni respectively. In the report of her work with Zanni, for instance, she says

> ...the definite article leads a subject to infer that the object was in fact present, causing for some a reconstruction in their original memory for the event. (1975, p 88).

By the time of her 1975 study on the effects of leading questions, Loftus was postulating the existence of a 'constructive mechanism' to explain the results of her

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1. Loftus, in several of her later publications, emphasizes the requirement that memory be 'activated'.

2. Indeed, they argue that Loftus' work is exemplary of research guided by schema theory.

studies. She embedded the mechanism in theoretical form in the familiar tripartite 'stage' model often adhered to by cognitive theorists. Her visual representation of the model is reproduced as Figure 5.1.

The model makes the point that memorial representations may be quite malleable. Representations of events may be altered simply by the addition of new information into existing memorial structures. Subsequent recollections may be based on either of the sources of information, and if postevent information differs from memory of original information, then these recollections may be inaccurate.

The model is solely descriptive: it restates the observation that exposure to postevent information may affect subsequent attempts to recollect original information. There is nothing in the model to explain why only approximately 20% of subjects are typically influenced in the direction of the postevent suggestion, nor is there any attention given to the fact that subjects might not encode information from the original event in the first place. (The model is easily revised for the latter possibility by uncoupling the processes of acquisition of original and postevent information).

At this stage in the development of the theory, the issue of 'integration' was the centre (and the extent) of the explanation of the experimental findings. The question of whether the integration involved the destruction ('updating' or 'overwriting') of original information, or the coexistence of the two bodies of information, was referred to obliquely, if at all.

Fig 5.1. **Loftus' 1975 model for changes in subject recollections after exposure to postevent information**

ACQUISITION PROCESSES			RETRIEVAL PROCESSES	
Acquisition of original experience	→	Acquisition of subsequent experience		
Decision about what to look at	Integration of information into some representation →	Integration of new information into the representation	Regeneration of the altered representation	Response

Reproduced from Loftus, 1975; p 587

By 1978, though, the issue was very much in mind. Loftus declared the problem insoluble in the following terms:

> ...either the subsequent information alters the original memory or both the original and the new information reside in memory, and the new competes with the old. Unfortunately, this extremely important issue cannot be resolved with the present data.[3] Those who wish to maintain that the new information produces an alteration cannot prove that the earlier information will not one day spontaneously reappear. Those who wish to hold that new and old information both exist in memory will argue that a person who responds on the basis of new information alone does so because the proper retrieval cue or the right technique has not been used. (Loftus et al. 1978, p 30)

Loftus argues that although it is difficult to settle the issue, the positions have very different and very real consequences. She spells these out in her 1979 publication in American Scientist (Loftus, 1979b):

1. If original and postevent information coexist intact in memory, then simply removing the interfering information may cause original event information to become available.

2. On the other hand, if postevent information alters memory for original information (ie. destructively updates it), then a witness can only be returned to his (her) original memory by realtering the version currently in memory.[4]

Although Loftus declared the question insoluble, she herself pursued the question relentlessly. Indeed, after 1979, she restricted herself almost solely in her postevent

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3. In her influential 1980 article (Loftus & Loftus, 1980), Loftus drops the qualification lodged here. It is never finally possible to resolve the dispute.

4. It looks like Loftus has in mind here the consequences each of the theoretical positions has for the practical matter of 'getting the truth out' of someone who has been exposed to postevent information. If her intention is practical, then it is in a particularly abstruse manner that she pursues application.

information research to the question of coexistence. In a publication she coauthored with W. Cole, for example (Cole & Loftus, 1979), she reports a series of reaction - time experiments designed to bring evidence to bear on the problem. And again, in a study with Greene & Flynn (Greene, Flynn & Loftus, 1982), she reports experiments that attempt to retrieve original event information by warning subjects of the misleading nature of the postevent material. (These are certainly not all of the studies either: see her 1979 book for a review).

All these attempts failed to eliminate the postevent information effect: a certain proportion of subjects in each of the experiments seemed unable to recall the original information. From this, she drew the inference that the weight of the evidence supported the hypothesis of memorial alteration:

> ...some memories may undergo destructive transformation due to postevent inputs, and the original memories may no longer be retrievable (Loftus & Loftus, 1980). This position has been strengthened (although by no means proved) by numerous empirical attempts to recover original information that have failed. Even the 'mysterious' technique of hypnosis has failed to lead to the original memories once they have been altered... .(Loftus, 1983, p 417).

By the early 1980's then, Loftus had settled on the claim that postevent information *alters* existing memories.

However, at about the same time, the first disconfirmatory studies had started appearing in the literature. (These are reported in the literature review at some length). Thus, Dodd & Bradshaw (1980) reported that embedding misleading information in realistic sociolinguistic contexts eliminated the misinformation effect; and likewise, Bekerian & Bowers

(1983, 1984) reported that enriching the retrieval environment eliminated the effect.

The theoretical response to these developments takes the route identified by Kühn in The Structure of Scientific Revolutions.

At first Loftus modified her claims so that the disconfirmatory evidence simply looked like the specification of natural boundaries. In her 1981 chapter in Long & Baddeley's handbook Attention and Performance (vol. IX), for example, she sets out a few (previously unexpounded) conditions for the manifestation of the misinformation effect. Thus she states that postevent information is stored in memory with existing related information, and becomes 'bonded' to existing information automatically, provided that postevent information is not tagged as being 'inappropriate'. To avoid being tagged as 'inappropriate', postevent information must attract an 'optimal minimal amount of attention'[5]: if the postevent information arouses the interest of subjects, then its misleading nature is more likely to be identified and it is consequently less likely to alter existing representations.

Loftus cites evidence from three sources in favour of this claim.

1. She argues that the early 'leading question' studies (see #1 in the literature review) demonstrated

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5. Loftus draws attention here to the analogy between this position and the well known 'levels of processing' phenomenon (Craik & Lockhart, 1972). The notion is that sufficient processing will attenuate postevent information effects.

misinformation effects because postevent information presented to subjects was usually embedded in a *relative clause*. A relative clause receives less attention than a main clause (by virtue of its place in the sentence structure), and thus aids the effectiveness of suggestions.

2. Misleading information is much more likely to produce a misinformation effect when embedded in complex sentences than when embedded in simple sentences. Loftus (1981) cites Johnson's (1979) unpublished Ph.D. dissertation, and some unpublished work of her own as support for this proposition.

3. Warnings given to subjects prior to exposure to misleading information are demonstrably effective guards against misinformation, whereas warnings given immediately after acquisition of misinformation do little to attenuate effects due to misinformation (Greene, Flynn & Loftus, 1982).[6] Subjects who are forewarned about the presence of misinformation are likely to focus in much more detail on what follows.

Loftus extends this line of thought in an address delivered in 1983 and reported in the <u>Philosophical Transactions of the Royal Society of London</u>. Here she outlines factors known to critically affect recollection changes produced by postevent information (I referred to some of these in passing in the previous passage, and repeat them now for the

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6. At this stage Christiaansen et al's (1983) disconfirmation had not been published. See section #4.1 in the literature review.

sake of completeness). These are: (i) intervals between observation and presentation of misinformation (and between presentation of misinformation and final testing); (ii) the syntactical complexity of the linguistic material that misinformation is embedded in; (iii) the violence of the observed event[7]; and (iv) the forewarning of subjects about the presence of postevent information.

From these Loftus identifies an enabling process: *the elimination (and modulation) of the postevent information effect depends on the detection of discrepancies between original and postevent information.*

This process, according to Loftus, underlies the operation of all other variables. As explication, Loftus refers to Ausubel's (1963) notion of a 'subsumption process'. According to the 'subsumption process', new information entering the cognitive field naturally interacts with, and is appropriately subsumed within, relevant portions of the cognitive system.[8] The subsumption of traces provides 'anchorage' for new material, and thus constitutes an effective way for retaining information for future availability. Loftus puts this another way, saying that the 'integration mechanism' permits memory to behave in an orderly, efficient, and stable manner. Just as 'natural selection' is the mechanism by which only useful genetic information is retained, so we may be tempted to think of 'integration' (of the type identified by Loftus) as another

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7. Loftus reports this, but fails to cite supporting studies. See p 180 of the transactions.

8. The account I present here is dependent on that rendered by Loftus (1983). To dwell on Ausubel's work here will serve no useful purpose: I mention it simply to show what Loftus makes of the misinformation effect.

sort of natural selection mechanism, by which 'only the fittest memories survive' (Loftus, 1983, p 421).

Although Loftus' treatment of this contention hardly takes the form of a major theoretical statement, she gives it extended treatment in a later publication, published in a volume of eyewitness research co-edited with Wells. In this publication she reviews the evidence that suggests that memory is *not altered* by postevent information, which by this stage included Bekerian & Bowers' convincing demonstrations. Her response to Bekerian & Bower's work looks much like a case of fitting the paradigm to the problem:

> ...Bekerian & Bower's data are consistent with the notion that the process of change in recollections occurs gradually (Hall, Loftus & Tousignant 1985; p 134. My emphasis).

Loftus and her associates interpret Bekerian & Bowers' findings as being in line with the claim of memorial alteration: they explain the failure to show the misinformation effect by positing that Bekerian & Bowers' experimental manipulation failed to reactivate memory for original information, thus allowing inconsistent recollections to coexist.

This interpretation is substantially removed from earlier theoretical positions, and Loftus certainly couldn't let it through without discussion. She consequently presents a revision of previously adopted positions, which is at the same time the most complete theoretical treatment of the experimental results.

Loftus (and her co-authors) "offer two general principles that ... constitute a framework for discussing when and how changes in recollection occur":

> 1. Recollections can change only if the subject does not immediately detect discrepancies between postevent information and memory for the original event. (p 135)

Implicit in this principle is the possibility that discrepant pieces of information can be stored separately: postevent information will not necessarily change memory for original information, which means that there must (in such cases) be two representations. The claim, rather, is that to be stored independently, the incongruency between postevent information and existing representations must not become apparent to the subject.

In the second principle, Hall et al. add a rider about 'active' and 'inactive' memory:

> 2. Change in recollections for an original natural event occurs only if a postevent experience restores memory for the original event to an active status. (p 137)

Hall et al. use 'active' here in the sense imputed to it by Lewis (1979): active memories are those that have been recently retrieved from storage for active problem solving. Hall et al. cite some work from animal memory research in support of this principle, which purports to show that 'active' memories are highly susceptible to interference, but they fail to provide any *direct* support for the proposition from postevent information research. This does not stop them from drawing the conclusion:

> In short, the second principle of change is consistent with a large body of experimental evidence (p 138).

The only direct experimental evidence cited though, is, as I have indicated, drawn from the animal research literature. The 'large body' that Loftus et al. have in mind isn't 'experimental evidence' at all:

> ...it is a commonplace finding that irrelevant filler tasks assigned during the interval between training and testing are adequate to prevent changes in recollection. Such filler tasks are effective in preserving in recollections, presumably because they do not evoke memory for the event. (p 137).

Apart from the fact that this is not in the least like 'experimental evidence', the reasoning here begs several questions. Filler tasks are not presented to prevent changes in recollection, they are designed to counteract the unequal tendency of subjects to 'rehearse' originally presented information, and thus serve to maintain the randomization of the experiment. Secondly, it can hardly be assumed that such changes do occur if it is precisely such a proposition that Loftus is arguing for. The leap from here to the further claim that filler tasks prevent changes to recollections by virtue of the fact that they fail to 'activate' original memories is thus simply the crossing of a fictional bridge.[9]

Bad logic needn't cripple a good theory, though, and what really renders the position espoused here particularly precarious is that the introduced revisions are not the upshot of paradigmatic research (Kühnian problem solving),

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9. There may be another motive behind the addition of this rider to Loftus' position. Recall from the earlier account of schema theory that new information is interpreted in terms of currently activated schemata. The addition of the rider may stem from the recognition of this principle.

but attempts to stretch the theoretical canvas over anomalies that threaten to collapse the paradigm.

That this is indeed the case will be made even clearer when I look at Loftus' response to the claim that postevent information effects are artefactual.

1.4 Opposition

By 1984 several studies had appeared that reported results incongruent with the theoretical position adopted by Loftus. These studies[10] showed that misinformation effects reported by Loftus and her associates could be eliminated with relatively minor methodological changes.

Alongside the disconfirmatory empirical work reported in the literature, several theoretical notions about the fate of memory for originally acquired information sprung up. Among these, the most thoroughly developed account seems that adhered to by Bekerian & Bowers, and expounded at some length in Morton, Hammersley & Bekerian (1985). I will consequently focus my attention on this account, which goes by the name of **Headed Records Theory.**

1.4.1 HEADED RECORDS THEORY

In the previous section I argued that Loftus' 1985 theoretical exposė is an attempt to protect a theoretical position from anomalous empirical findings. Headed Records Theory, on the other hand, is not an 'impromptu' theory, and I will have to introduce it here by discussing it in rather

10. I have referred to them at length in the literature review, but see especially Bekerian & Bowers (1983, 1984), and Pirolli & Mitterer (1984).

general terms before its relevance to postevent information research becomes clear.

According to Headed Records Theory (HRT), there are two basic structures of memory: the **memory unit** (a 'Record'), and the **access key** (a 'Heading'). Records are the unit of storage for recallable information in memory, and Headings form the means through which Records are accessed. Certain features characterize Records:

1. Records are discrete. No connections link Records that happen to be related in terms of their content.[11]

2. Access to a record is all-or-none: either the record is retrieved or it isn't.

3. Records have no restriction on the amount of information they contain, nor on the format for information.

4. There is duplication of information in Records. The same event or any of its constituents can be represented in multiple Records, and the format for the event's representation may or may not vary.

5. Once information is represented in a Record, it is not subject to alteration either by modification or by the addition of new information.

Headings, on the other hand, are characterized by the following:

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11. This feature, Hammersley et al. observe, distinguishes HRT from both schema and associational theories of memory.

3. If a match has been made, then the associated Record is retrieved and made available for further processing.

4. The retrieved Record is evaluated. Records will usually be evaluated in the light of the goals represented in the Task Specification.

It should be clear from these propositions that HRT is structurally antagonistic to Loftus' hypothesis of memorial alteration. In the light of this, Morton et al. use the HRT model in their 1985 publication to explain the misleading effects obtained by Loftus under an hypothesis of memorial coexistence.

The misleading effects reported by Loftus are explained in HRT as being due to the omission of critical information at the time of the test. In the paradigmatic postevent information experiment, the Heading of the Record of the original slide sequence would contain thematic information related to the *sequence* of events portrayed. As the slide pairs used in the test were arranged in an order that was random with respect to the original slide sequence[13], important thematic information would be missing from the Description used to search Headings. Thus, the retrieval cycle would not have enough information to discriminate the Heading for the original slide set from the Heading for the postevent information. In accordance with the principles outlined above, the Heading for the more recent of these Records would be matched, which is the Record containing the

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13. See the literature review for an explanation. See especially the studies conducted by Bekerian & Bowers, 1983, 1984.

postevent information, and the misleading effect would thus be produced.

Bekerian & Bowers, as I pointed out in the literature review, introduced a modification to the paradigmatic design, which involved rearranging the structure of the final test so that slides shown to subjects matched the original slide set sequentially. This modification effectively eliminated control - experimental differences, which is taken by Morton et al. as firm support for the interpretation of the misinformation effect offered by Headed Records Theory.

Under Headed Records Theory the misinformation effect is 'real', but does not constitute support for the claim that misinformation may alter memory for an event. Original event information and postevent information coexist as Records; all that is affected is the retrievability of these Records, and this depends not on the Records themselves, nor even the Headings, but on the *Description* that is used to search memory.

1.5 Conclusion

Alongside Bekerian & Bowers' forceful demonstrations, several other studies have shown that the misleading effect reported by Loftus is inconsistent with a position that postulates memorial alteration. Kroll & Timourian (1986), in particular, present a most convincing demonstration. Subjects in their study were exposed to the paradigmatic postevent information experimental procedure, replicating the usual experimental - control difference. After producing the paradigmatic effect, Kroll & Timourian provided subjects with contextual cues ('returning' subjects to the scene of the crime), which had the consequence of eliminating the recently observed misinformation effect.

This, and a few other demonstrations, seem to have settled the internal theoretical dispute raised by postevent information research in favour of the contention that misinformation affects *retrieval* processes.

Loftus in recent years has sided with the opinion that postevent information does not *alter* memory for an event, but for entirely different reasons. It is to these reasons, which concern the validity of postevent information research findings, that I now turn.

2. THE METHODOLOGICAL DISPUTE

2.1 Introduction

Loftus' work, as I have pointed out, has met with concerted opposition on a theoretical level. More disconcerting than this theoretical dispute, though, is the recent claim that the postevent information effect reported by Loftus and her associates is artefactual. This claim, outlined by McCloskey & Zaragoza (1985), and Zaragoza et al. (1987), posits that the experimental - control differences observed in typical postevent information experiments are a product of the experimental design utilized, and not of differences between experimental and control groups. The question of what to attribute the postevent information effect to (memorial alteration or retrieval difficulties) thus becomes insignificant, because the effect is spurious.

This powerful and controversial claim virtually sounds the death knell for postevent information research. Laying the paradigm to rest without further ado, though, is a lamentable business. Apart from the sheer waste (which as the length of the literature review shows, is quite substantial), the problem of postevent information remains a very real and pressing legal problem. A little later in this chapter, and in the next, I will bring evidence to bear that indicates limits to the claim of artefactuality. In the meanwhile, the best place to begin is by reviewing McCloskey et al.'s claim in some detail.

2.2 McCloskey & Zaragoza's Argument.

As the claim of artefactuality rests on a consideration of the experimental design typically used in postevent information research, I will reintroduce some of the salient aspects of the experimental design.

The paradigmatic way of examining the postevent information effect is by comparing groups of subjects that are exposed to misleading information after having observed an event, with groups that are not exposed to the misleading information. At an initial stage of the experiment both groups are shown the same stimulus material. Later, the experimental group is given information inconsistent with the original stimulus, and the control group information either unrelated to, or consistent with, the original stimulus. Finally, both groups are given a forced choice recognition test in which original and postevent information are paired, enabling a measure of the effect of the false information. (Tables 1.1 and 3.1 depict the design).

The measure of interest is the proportion of control subjects choosing the incorrect slide in the test in relation to the proportion of experimental subjects choosing the incorrect slide. This measure is said to show the effect that the inconsistent information has on memory for the event, and is typically in the range of a 20 - 25% difference between experimental and control groups.

McCloskey & Zaragoza argue that the methodology employed in the paradigm is severely flawed, and indeed, that the paradigm, contrary to what Loftus and others claim, tells us nothing about the effect of postevent information on eyewitness memory. This is because the experimental - control differences observed in typical experiments in the paradigm are to be expected as a result of the methodology employed, even if equal numbers of control and experimental subjects remember the original information at the time of the recognition test.

McCloskey & Zaragoza demonstrate this point by comparing expected control - experimental differences on the recognition test.

Taking the case of the control group first, expected performance is constituted by (i) the proportion of subjects who remember the original information at the time of the test and choose correctly at the time of the test, in addition to (ii), the proportion of remaining subjects who do not remember the original information, but still perform accurately on the test (ie. at the level of chance). To use McCloskey & Zaragoza's hypothetical simulation, if 0.40 of the subjects here remember the original information and perform accurately on the test, then 0.60 of the subjects (not remembering the original information) will perform at the level of chance on the recognition test. Thus, total expected performance will be

> Subjects who remember OEI + subjects who don't remember OEI & choose correctly (ie. at the level of chance)
>
> 0.40 + 0.60*(0.5) = 0.70.

Expected performance on the final test in the case of the experimental group is somewhat different, however. Even if the proportion of subjects in this group remembering the original information is equivalent to the corresponding proportion in the control group, performance on the final test is likely to be lower than control group performance. This is because subjects in the experimental group, unlike subjects in the control group, are exposed to postevent information and a proportion of those subjects not remembering original information *will* remember the (incorrect) postevent information, and thus systematically choose the incorrect information on the recognition test, lowering experimental group performance below control group performance. We also expect a proportion of subjects from this remainder (ie. who don't remember original information) to remember both the original and postevent information at the time of the test, and to choose incorrectly in the final test. Using McCloskey & Zaragoza's hypothetical simulation once again, if 0.40 of the subjects in the experimental group remember the original information in the experimental group, only a certain proportion of the remaining 0.60 subjects will perform accurately at the level of chance. Assuming that 0.5*(0.60) of the subjects who don't remember the original information do remember the postevent information (and thus choose incorrectly on the test), and that 0.5 of the remaining subjects (0.30) remember both the original and postevent information, but choose the postevent information on the forced choice test, expected performance of the experimental group is only 0.475.

> Subjects who remember OEI + subjects who don't remember OEI, but choose correctly (ie. at the level of chance).
>
> 0.4 + 0.075 = 0.475

It is consequently quite possible for a large control - experimental difference to exist even if equal proportions of control and experimental subjects remember the original information at the time of the test.

This argument demonstrates that differences observed between experimental and control groups in experiments utilizing the methodology under scrutiny need not tell us anything about the effect of postevent information on the memory of eyewitnesses: such differences are just as easily interpreted as experimental artefact. As the methodology employed in the paradigm doesn't allow us to decide whether observed differences between experimental and control groups is due to experimental artefact or real effects on memory processes, McCloskey & Zaragoza argue that it may be a good thing to jettison the methodology. They suggest that a more appropriate way to test the hypothesis that postevent information affects eyewitness memory is to reconstruct the forced choice recognition test so that information originally seen is paired with information not seen in either the original slide series or in the postevent information condition. (Thus, if a stop sign was originally seen, and a yield sign seen in the postevent information condition, then an appropriate manipulation would be to pair the stop sign with say, a no entry sign, in the forced choice test). Here, they reason, if memory is affected by postevent information, then the effects will still yield experimental - control differences, and will now not be due to artefact (because postevent information is no longer one of the options on the final test). Table 5.1 depicts the revised experimental procedure.

McCloskey & Zaragoza (1985) conducted six experiments utilizing this revised procedure, and in all of these no significant differences were found between experimental and control groups. They conclude that this strongly suggests that previous findings of memory impairment were spurious. It may be a good thing, they suggest, to give up on the question that has informed the paradigm (viz. the possibility that postevent information impairs eyewitness memory).

2.3 Conclusions

McCloskey & Zaragoza's argument is very convincing. Loftus, in fact, has conceded to many of their claims: she accepts that the methodology is flawed and also seems to accept the conjecture that this renders the claims made by her and others about the permanence of human memory doubtful (Loftus, Wagenaar & Schooler, 1985). She retracts her earlier postulates that the experiments show an alteration of memory in favour of the claim that postevent information may affect memorial *reports*. This amounts to an abandoning of the research question pursued in #3 (see the literature review), and a return to the (unexceptional) question that Muscio addressed in 1915.

Table 5.1 McCloskey & Zaragoza's revision of the paradigmatic experimental procedure.

	SLIDE SHOW	QUESTIONNAIRE	TEST
CONTROL GROUP	ORIGINAL INFORMATION	—	OEI VS NEI
MISLED GROUP	ORIGINAL INFORMATION	POSTEVENT INFORMATION	OEI VS NEI

OEI = ORIGINAL EVENT INFORMATION
NEI = INFORMATION NOT SEEN BEFORE.

What implications does McCloskey & Zaragoza's argument have for postevent information research? Convincing as the argument may seem, it conceals an *a priori* flaw which curtails the generalizability of the argument. In the next section of the chapter I will take up this question.

2.4 An Evaluation of McCloskey & Zaragoza

For the hypothetical data that McCloskey & Zaragoza use to establish their major point, I think the argument is unassailable. In certain conditions, if we use the methodology typically utilized in the paradigm, we can indeed never be sure whether our results are due to artefact or real effects on memory for an observed event. It may consequently be a good idea to give up the methodology.

Nevertheless, I want to suggest that McCloskey & Zaragoza's argument should not entice us into rejecting the entirety of the substantial amount of research that the paradigm has generated. Later in the dissertation I will suggest that McCloskey & Zaragoza's argument cannot explain the results of a number of experiments, in which experimental control differences have been observed. That is, in a number of relevant experiments, differences observed between control and experimental subjects on forced choice recognition tasks cannot be due to the specific artefact that McCloskey & Zaragoza outline. These experiments may show real effects of postevent information on eyewitness memory.

2.4.1 *A PRIORI* CONSIDERATIONS: A PROPORTIONAL MODEL;

McCloskey & Zaragoza's argument is centred on the premise that the total performance of subjects in the typical experimental scenario on the forced choice task can be broken down into proportions of subjects performing correctly or incorrectly for a number of expected reasons. It is a proportional model, and as such, McCloskey & Zaragoza fail to clarify the conditions under which this model must be false.

There are at least three conditions under which McCloskey & Zaragoza's hypothesis must obviously be false (and many more under which it will be unlikely). To consider these conditions, it will be useful to use a simple algebraic notation representing McCloskey & Zaragoza's hypothesis. Expected performance in the control and experimental groups can be represented as equations 1a and 1b respectively:

1. a. $A + X*q = Z$
   b. $A + S*q = W$

Where Z and W are the proportions of subjects, in the control and experimental groups, respectively, choosing the correct option in the forced choice task; A is the proportion of subjects remembering the original information and thus performing correctly in the test (assumed to be equal for control and experimental groups by the hypothesis); X is the remaining proportion of subjects in the control group (ie. 1 - A), who are expected to choose the correct slide by chance in the test; q is the level of chance in the forced choice task (ie. 1/number of alternatives). A and X must sum to N (1.00), the total proportion of subjects in the experiment.

Using this simple model it is easy to calculate the unknowns from Z, W and q, which are always known.

The model above reflects McCloskey & Zaragoza's hypothesis for experimental scenarios in which control subjects are either not given postevent information, or are given *irrelevant* postevent information. It can easily be expanded for scenarios in which control subjects are given postevent information that is *consistent* with original information. (In these situations we can expect control performance to be boosted to show even greater control - experimental differences: indeed, we expect the difference between a control (or experimental) group given consistent postevent information and a control group given no postevent information to be the same as the difference between a control group not given postevent information and an experimental group given misleading postevent information). Thus, in situations where postevent information is consistent for the control group expected performance for control and experimental groups can be represented by equations 2a and 2b.

2 a. $A + J + S*q = Z$

b. $A + S*q = W$

Where J = the proportion of subjects choosing the correct slide neither because they remember the original information nor because they choose at chance levels, but because they either remember the postevent information and correctly choose the original slide on the basis of this memory, or because they remember both the original and postevent information and thus choose the correct slide.

There are at least three conditions in which this model, taken as an hypothesis about expected differences between control and experimental groups, is obviously false.

2.4.1.1 *WHERE Z < Q*

Consider the case, where in the control group, the proportion (Z) of subjects performing correctly is less than q (the level of chance). For convenience sake, assume a two choice forced choice task is used ie. $q = 0.5$. If 0.40 of the total number of control subjects perform correctly on the forced choice task, then, from 1a, $X = (N-Z/q-1) = 1.20$.

This is patently impossible, as the total proportion of subjects in the experiment is always 1.0. Wherever $Z < N*q$, $A + X$ (the total proportion of subjects in the experiment) must be greater than 1, but as $A + X$ is necessarily 1.0, the hypothesis is necessarily false. This is merely another way of saying that whenever less than $1/q$ of the subjects perform correctly on the forced choice task, it cannot be the case that this proportion of subjects is constituted by a proportion of subjects correctly remembering and thus correctly performing, and a proportion choosing at the level of chance. All the same, it would be an odd thing to find performance on the recognition task at less than would be expected by chance, for this would mean that some subjects were choosing the incorrect slide systematically. While this seems a feasible happening in the experimental group (indeed, it forms the basis of McCloskey & Zaragoza's model), there is little reason to expect it in the control group.

2.4.1.2 WHERE W < A

The second and more interesting case in which McCloskey & Zaragoza's hypothesis is necessarily false is the case in which the observed performance of the experimental group is less than the proportion of subjects remembering the original information in the experimental (and, by the hypothesis, control) group (ie. $W < A$). This is because, by the hypothesis, the proportion of subjects remembering the original information in the control group is the same as the proportion of subjects in the experimental group remembering the original information. That is, the total proportion of subjects performing correctly on the test would be less than the proportion remembering the original information and performing accurately on the basis of this memory. This is impossible, as the total proportion of total correct responses in the experimental group is constituted by the proportion remembering the original information and the proportion performing at chance levels. So, for example, in the case where the proportion of correct responses in the control group is .75 and in the experimental group is .30, McCloskey & Zaragoza's hypothesis spelt out in equation form will be

control:	$0.50 + 0.5*(0.5) = 0.75$
experimental:	$0.50 - 0.4*(0.5) = 0.30$

This - needless to say - is a necessarily false state of affairs - the proportion W is less than the proportion A. In this case, therefore, the proportion of subjects in the experimental group remembering the original information must be less than that in the control group, which is directly contrary to the hypothesis of artefact. This case is not solely a demonstrative point either - the data are taken directly from the fifth experiment reported in McCloskey & Zaragoza (1985), "original procedure".

2.4.1.3 WHERE A + X (OR A + S) > 1.0

The third case in which the model must be false is the case in which the proportions A and X, or A and S sum to greater than N (1.0). The model is false here because it implies an impossible state of affairs - N is the total proportion of subjects in an experimental group (ie. N necessarily equals 1.0).

There is an empirical way to test McCloskey & Zaragoza's hypothesis. As Z, W, Q and N are nearly always reported in published studies it is a simple matter to test whether McCloskey & Zaragoza's hypothesis can be considered to account for observed differences between control and experimental groups. In cases where any of the three conditions outlined earlier (ie. if $Z < N*Q$, or $W < A$, or $(A + S) > N$ [or $(A + X > N]$) is met, the hypothesis cannot possibly account for the findings of the experiment. To do this, though, requires revising the model above so that it applies to samples. In its present form it is applicable only at the level of populations. In the next chapter I will outline the revisions required to transform the model for use with samples.

In the next chapter I will use the model specified above to test the hypothesis of artefact in two ways. In the first, an existing body of published research will be tested against the model, and in the second, hypotheses will be specified for an experimental test of McCloskey & Zaragoza's claim.

In the meantime, I turn to a consideration of some recent research that reproduces the postevent information effect reported by Loftus in spite of correction for the source of artefact identified by McCloskey & Zaragoza. This research was not discussed in the literature review, as it is part of the methodological dispute, and can only be understood in terms of the dispute.

2.4.2 *A POSTERIORI* CONSIDERATIONS: DISCONFIRMATION OF MCCLOSKEY & ZARAGOZA

In the literature review I drew attention to several studies that eliminate experimental - control differences in pursuit of the hypothesis that the postevent information effect shows the coexistence of original and postevent information. These studies present problems to the hypothesis of artefact insofar as they retain the (problematic) recognition test structure of postevent information experiments, yet fail to show the paradigmatic experimental - control difference. McCloskey and Zaragoza, acknowledging that the studies present problems for their argument, provide an analysis of these experiments and dismiss them (five in all) on methodological grounds[14]. It is not worth pursuing an analysis of these studies here, as the 'methodological grounds' that McCloskey & Zaragoza base their dismissals on are technically accurate, but nevertheless, the unidirectionality of the results produced by the studies is a little baffling.

Apart from the studies that precede McCloskey & Zaragoza's paper, and which come under the methodological scalpel, two studies that postdate their paper show positive effects of misleading information after correction for experimental bias. In the first of these, (Ceci, Ross & Taglia, 1987), misleading information given to children was found to be effective both when children were tested with the original test procedure and when tested with McCloskey & Zaragoza 's (1985a) modified test procedure. (McCloskey & Zaragoza's

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14. Variously, the failure to adequately counterbalance a design (Christiaansen et al., 1983); the use of confounding materials (Weinberg et al., 1983); and nonreplication (McCloskey & Zaragoza, 1985 - of Bekerian & Bowers (1983, 1984).

(1985a) test procedure nevertheless, substantially attenuated the effect found with the original test procedure). Ceci et al. take their experiment to show that misinformation may, contrary to McCloskey & Zaragoza's (1985a) claims, adversely affect memory for an event. However, they qualify their conclusions by suggesting that the misinformation effect demonstrated in their experiment may be specific to children, and need not generalize to adults.

Similarly, Kroll & Timourian (1986) report an experiment in which, again, misinformation effects persist in spite of correction for McCloskey & Zaragoza's (1985a) hypothesis of experimental bias. Further corrections for experimental bias, however - on the basis of information given via personal communication by McCloskey & Zaragoza - remove the effect. Kroll & Timourian, unlike Ceci et al. (1987) made post hoc corrections for experimental bias on the basis of estimates of guessing effects, and due to the post hoc nature of these call for an independent estimate of the biasing factor in postevent information experiments. While Ceci et al. (1987) attempt to provide this estimate in their use of a control group that is not exposed to misinformation and that completes the modified test procedure, their findings (viz. that experimental subjects exposed to misinformation show real memory impairment) does not, as previously indicated, replicate McCloskey & Zaragoza's (1985a) finding of no differences with an equivalent experimental procedure.

There is thus at least one clear experimental refutation of McCloskey & Zaragoza's hypothesis of artefact. In addition, there are further, empirical, grounds on which to dispute the sufficiency of the hypothesis.

In the next chapter I will bring evidence from three investigations to bear on these matters.

CHAPTER 6: EMPIRICAL CONTRIBUTIONS.

1. INTRODUCTION

In this chapter I present evidence of an empirical nature that has bearing on the methodological dispute discussed at some length in the previous chapter. As the previous chapter showed, evidence of this nature is important: the arguments lodged by McCloskey & Zaragoza place the entire body of postevent information research in jeopardy.

Three forms of evidence are provided. Two of these derive from *post hoc* analyses of experimental results reported in the literature, and one from an experiment conducted with the intention of providing independent estimates of artefact in postevent information studies.

In the first of the *post hoc* analyses I attempt to test the algebraic formulation of the hypothesis of artefact against reported results of postevent information experiments. In the second I report a small scale meta-analysis of the postevent information literature, conducted with the methodological issues directly in mind.

The experiment, which is the only direct empirical work reported in the thesis, is a carefully designed factorial manipulation of recognition test structure and information type. It aims to exhaust knowledge about the relationship(s) between (i) consistent, neutral, and inconsistent postevent information, and (ii) several types of forced choice recognition tasks. One of these recognition tasks was devised here to independently test the claim of experimental artefact.

I begin with the *post hoc* analyses.

2. *POST HOC* ANALYSES

2.1 Proportional Model

In the previous chapter I developed a simple algebraic description of McCloskey & Zaragoza's model of artefact, and suggested that the model could be evaluated against reported results of postevent information experiments. The evaluation is possible, I suggested, because the algebraic description of the model (implicitly) specifies a number of conditions in which the model must fail. In this section of the present chapter, I evaluate the hypothesis of artefact by modelling the reported results of a number of postevent information experiments against the algebraic description of the model.

As it stands, the algebraic description of the model is applicable at the level of populations. It needs to be modified for use at the level of samples. This can be achieved with little effort. Recall from the previous chapter that the model hypothesizes a state of affairs that is described as:

Control group:	$A + X*q = Z$
Experimental group:	$A + S*q = W$

From this description, we can calculate the unknowns at the level of populations simply by solving the set of equations. Thus, $X = (1-Z)/(1-q)$, and $A = Z - Xq$. Once the equation for the Control group is solved, the solution for that describing performance of the Experimental group follows. The solution is made possible by the fact that Z, the total proportion of subjects in the Control group choosing the correct alternative on the forced choice test, is always reported, as is q, which is the level of chance on the forced choice task (1/no. of alternatives).

However, at the level of samples, the solution of the equations is made a little more complex by the introduction of sampling error by the forced choice task. Thus, although the probability of choosing the correct option is 1/(no. of alternatives) (usually 0.5), observed performance will be binomially distributed around this figure. The appropriate correction to the procedure outlined above is to express Xq as a confidence interval, using a conventional level of statistical significance, and to compute two sets of equations for the experiment, one around each tail of the confidence interval. No correction need be made to the estimate A (apart from its recalculation), as the model hypothesizes that A and Xq sum to unity in any particular experiment.

Estimating the parameters of the model thus presents little problem. Evaluating McCloskey & Zaragoza's model for particular solutions of the equations is a little more problematic. There seem to me to be two methods though, that hold some promise.

In the first one takes advantage of the knowledge that the algebraic description identifies states of affairs that are impossible. (I identified these in the previous chapter as being (i) where $Z > Q$; (ii) where $W > A$; and (iii) where $A + X$ (or $A + S$) > 1.0.) In cases where solutions of the equations for particular experiments or sets of experiments fail to satisfy these restrictions, the model is necessarily false, and the claim of artefact must also be false. That is, differences between experimental and control subjects cannot be due to the source of artefact that McCloskey & Zaragoza identify.

This method is an interesting and powerful test of the claim of artefact, but may not be strict enough because it only tests for conditions of possibility. It accepts the null hypothesis even though the state of affairs outlined by the null hypothesis may be extremely unlikely. This comes about because the method increases the likelihood of falsely accepting that experimental results which satisfy the test show the correctness of the claim of artefact (ie. the probability of making a type II error is made too great).

A method of testing the hypothesis of artefact that doesn't raise the probability of making a type II error to unsatisfactorily high levels involves solving the equation describing performance of the experimental group in a slightly different way. To do this, I introduce a further assumption into the model. The proportion S, which is the proportion of experimental subjects choosing at random in the final recognition test (ie. those subjects who remember neither original event information nor postevent information at the time of the test), is assumed to be equivalent to the proportion of subjects in the control group not remembering original event information at the time of the test. That is, X is set equal to (1 - A)(1 - A): the proportion of subjects who remember neither postevent information nor original information at the time of the test is the proportion of subjects who don't remember original information multiplied by itself (ie. a proportion of subjects, A, will remember original information, leaving a proportion of subjects who fail to remember original information (1 - A); of this remaining proportion (1 - A) a further proportion of subjects will remember postevent information, leaving a further remaining proportion that fail to remember postevent information (X), and the best estimate of this remaining proportion is the proportion who fail to remember original information (1 - A). This is not a foolproof assumption by any means - I indicate one of the problems it raises later - but it seems to be a fairly reasonable assumption, insofar as it reproduces the assumption made in McCloskey & Zaragoza's model, namely that only a certain proportion of subjects will remember particular information contained in a representation of a complex event, and insofar as the further assumption that it makes viz. that the best estimate of this proportion in the experimental group is the proportion in the control group in an analogous position is plausible.

2.1.1 METHOD.

The algebraic description developed in Chapter 5 was utilized here to test the hypothesis that results of postevent information experiments are due to experimental artefact. Data reported in studies from experiments that employed paradigmatic methodology (see Chapter 3) were collected for analysis. (Studies were identified from several literature searches).

To make matters simple, I will describe the results of the analysis for the experiments reported by McCloskey & Zaragoza in the 1985 paper in which they spell out the claim of artefact. After describing the results of the modelling for these experiments I will extend the analysis to a number of other experiments.

2.1.1.1 METHOD 1.

Equations describing performance of control and experimental groups were solved for reported values of Z & W. Table 6.1 depicts the reported values of Z, W, and N, and Table 6.2 the solution of the equations.

Solving the equation with the best single estimate of q (0.5) suggests that the data reported in McCloskey & Zaragoza (1985) are inconsistent with the claim of artefact. However, as I indicated above, q must be treated as a confidence interval and not as a single estimate. Accordingly, Table 6.3 presents two sets of equations, constructed around the tails of the confidence interval for $q = 0.5$, $p < 0.05$.

Table 6.1 Reported values of Z, W & N, McCloskey & Zaragoza (1985).

EXPERIMENT	Z	W	N
Experiment 1	0.75	0.36	18
Experiment 2	0.70	0.35	24
Experiment 3	0.67	0.45	30
Experiment 4	0.75	0.42	36
Experiment 5	0.72	0.40	42
Experiment 6	0.70	0.30	24
Combined	0.71	0.38	174

Table 6.2 Equations for combined results of experiments reported in McCloskey & Zaragoza (1985).

Control group:	$0.43 + 0.28 = 0.71$ $(75) + (50) = (125)$
Experimental group:	$0.43 - 0.05 = 0.38$ $(75) - (9) = (66)$

* Numbers in brackets refer to actual subject numbers

Table 6.3 Solutions of equations using 95% confidence intervals: McCloskey & Zaragoza (1985).

(A) Lower tail:	
Control group:	$0.48 + 0.23 = 0.71$
Experimental group:	$0.48 - 0.10 = 0.38$
(B) Upper tail:	
Control group:	$0.37 + 0.34 = 0.71$
Experimental group:	$0.37 + 0.01 = 0.38$

Table 6.4 Results of the test of McCloskey & Zaragoza's hypothesis: Method 2, single best estimate of X.

Control group:	$0.43 + 0.28 = 0.71$
Experimental group:	$0.22 + 0.16 = 0.38$
Results of the test:	$0.43 > 0.22$; $Z = 4.18$; $p < 0.01$

These sets of equations show that the model is only barely possible at the 5% significance level. In particular, the model is only tenable if it can be accepted that a mere 0.01 of the subjects[1] in the experimental group constitute the proportion that choose at the level of chance.

The correct statistical conclusion is that the hypothesis of artefact is possible for the experimental results reported by McCloskey & Zaragoza (1985), under the conditions of possibility implied by the algebraic description of the hypothesis. This, as I indicated earlier, does not mean that the hypothesis is acceptable: in particular, the estimate of the proportion of subjects in the experimental group choosing at the level of chance seems unreasonable.

2.1.1.2 METHOD 2.

A minor revision to the algebraic model is introduced here for purposes of exposing the hypothesis of artefact to a severer test. The solution of the equations is revised by setting S equal to (1 - A)(1 - A), and calculating the proportion A in the Experimental group independently of the estimate A in the Control group. In this way the proportion A in the Experimental group can be tested for equality with the proportion A in the Control group, which is the assumption underlying the hypothesis of artefact.

Table 6.4 reports the results of this method, using the best single estimate of X, and Table 6.5 reports results using the 95% confidence interval around X.

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1. In real terms, two subjects.

Table 6.5 Results of the test of McCloskey & Zaragoza's hypothesis: Method 2, using 95% confidence interval.

(A) UPPER TAIL	
CONTROL GROUP:	$0.48 + 0.43 = 0.71$
EXPERIMENTAL GROUP:	$0.24 + 0.14 = 0.38$
RESULTS OF THE TEST:	$0.48 > 0.24$; $Z = 4.66$; $P < 0.01$.
(B) LOWER TAIL	
CONTROL GROUP:	$0.37 + 0.34 = 0.71$
EXPERIMENTAL GROUP:	$0.18 + 0.20 = 0.38$
RESULTS OF THE TEST:	$0.37 > 0.18$; $Z = 3.96$; $P < 0.01$.

As both of these tables show, the hypothesis of artefact fails to satisfy the test embodied in Method 2. Both tails of the confidence interval yield a difference between estimates of A in Experimental and Control groups that is statistically significant at the 5% level.

These results, I suggest, raise suspicions about the sufficiency of the hypothesis of artefact as an explanation of the misinformation effect. The results are not particular to the series of experiments reported by McCloskey & Zaragoza (it would be a strange thing if they were, for the experiments employ the paradigmatic design): in Table 6.6 I report equations solved for a number of experiments from an array of postevent information studies. (The equations are calculated on the basis of the best estimate of q (ie. 0.5); I do not bother to correct the estimate for sampling error, as the point is simply to replicate the results produced for McCloskey & Zaragoza's series of experiments, and several of the equations clearly defy the algebraic model).

It is tempting to conclude from the results of this modelling that the hypothesis of artefact is *mistaken*. Such a conclusion may be a little hasty, though, and it may be challenged on several grounds. For example, is the additional assumption introduced in Method 2 legitimate? It may well be the case that the proportion S is not equivalent to the proportion (1 - A)(1 - A): the proportion of subjects who fail to remember original information but who succeed in remembering postevent information need not be equivalent to the proportion remembering original information, because they are already a distinct group of subjects (ie. they failed to remember original information). The point really

TABLE 6.6 EQUATIONS FOR STUDIES THAT USE PARADIGMATIC METHODS.

Study: Christiaansen et al.(1983a)

Control group: $0.93 + 0.06(0.25) = 0.95$
Experimental group: $0.93 - 2.34(0.25) = 0.41$

Study: Christiaansen et al.(1983b)

Control group: $0.93 + 0.06(0.25) = 0.95$
Experimental group: $0.93 - 2.49(0.25) = 0.62$

Study: Christiaansen et al.(1983d)

Control group: $0.14 + 0.85(0.25) = 0.36$
Experimental group: $0.14 + 0.22(0.25) = 0.21$

Study: Christiaansen et al.(1983e)

Control group: $0.14 + 0.8(0.25) = 0.36$
Experimental group: $0.14 + 1.3(0.25) = 0.49$

Study: Christiaansen et al.(1983f)

Control group: $0.14 + 0.85(0.25) = 0.36$
Experimental group: $0.14 + 1.00(0.25) = 0.43$

Study: Christiaansen et al.(1983g)

Control group: $0.14 + 0.85(0.25) = 0.36$
Experimental group: $0.14 + 2.78(0.25) = 0.85$

Study: Jenkins & Davies (1985a)

Control group: $0.47 + 0.52(0.5) = 0.73$
Experimental group: $0.47 + 0.08(0.5) = 0.41$

Study: Jenkins & Davies (1985b)

Control group: 0.91 + 0.08(0.5) = 0.95
Experimental group: 0.91 -0.75(0.5) = 0.50

Study: Jenkins & Davies (1985e)

Control group: -0.026393 + 1.02(0.07) = 0.04
Experimental group: -0.026393 + 6.36(0.07) = 0.42

Study: Jenkins & Davies (1985f)

Control group: 0.02 + 0.97(0.07) = 0.09
Experimental group: 0.02 + 5.43(0.07) = 0.40

Study: Jenkins & Davies (1985g)

Control group: 0.16 + 0.83(0.07) = 0.22
Experimental group: 0.16 + 3.32(0.07) = 0.37

Study: Loftus (1975a)

Control group: 0.95 + 0.05(0.5) = 0.97
Experimental group: 0.95 - 0.25(0.5) = 0.82

Study: Loftus (1975b)

Control group: 0.88 + 0.12(0.5) = 0.94
Experimental group: 0.88 + 0(0.5) = 0.88

Study: Loftus (1975c)

Control group: 0.88 + 0.12(0.5) = 0.94
Experimental group: 0.88 - 0.28(0.5) = 0.74

Study: Loftus 1975c1)

Control group: 0.96 + 0.04(0.5) = 0.98

Experimental group: 0.96 - 0.08(0.5) = 0.92

Study: Loftus (1975c2)

Control group:	0.96 + 0.04(0.5) = 0.98
Experimental group:	0.96 - 0.28(0.5) = 0.82

Study: Loftus et al. (1976b)

Control group:	0.75 + 0.25(0.2) = 0.8
Experimental group:	0.75 - 0.85(0.2) = 0.58

Study: Loftus et al. (1976a)

Control group:	0.04 + 0.96(0.5) = 0.52
Experimental group:	0.04 + 0.54(0.5) = 0.31

Note: letters behind dates refer to the order of experimental - control comparisons reported in the study.

is that multiple *post hoc* revisions may be introduced into the algebraic model: the tests reported as Methods 1 and 2 here cannot finally establish the validity of the hypothesis of artefact because they do not proceed by causal examination.

3. META-ANALYSIS

In this section of the chapter I report the results of a small scale meta-analysis, conducted with the methodological issues discussed in the previous chapter firmly in mind.

3.1 Introduction

In the literature review I made mention of the vastness of the postevent information literature and the difficulties that this vastness produces for the review task.

These difficulties are not unique to postevent information research. Most psychological research areas are constituted by a substantial number of studies, which usually differ in important methodological ways, making comparisons a tricky business. In addition, studies that use very similar experimental procedures often produce contradictory results, making the task of integration a taxing and baffling business.

The response from researchers to the problems posed by integration of research findings across studies usually takes one of two forms. In the first place, the researcher may provide a narrative review of all the studies that constitute an entire research area. When the research area is large, which may mean that more than 100 studies are involved, the review is often as intimidating as the research area itself. The review is a pedestrian exercise, "[with]...verbal synopses of studies ... strung out in dizzying lists" (Glass, 1976, p. 4). In the second instance, which is frequently seen in psychological journals, researchers limit their review to a subset of studies in the area, the rest being rejected as methodologically flawed. A manageable amount of information is selected from the body of published research and presented in the review, and the rest is simply swept aside.

Both of these methods are scientifically unacceptable. The first provides little more than a redescription of the research to be reviewed, and the second wastes most of the available information, drawing *a priori* conclusions about matters that are really open questions[2].

The inadequacy of traditional methods of review is currently much under discussion in statistical circles. Present opinion is that where a research area is constituted by a large number of studies, quantitative review methods seem more adequate than the traditionally used methods. To complete the literature review, which was begun much earlier in the thesis, I will subject the studies reviewed in a previous chapter to quantitative review.

Although quantitative review methods have been used since the 1950's (Hunter et al. 1982), it is only in recent years

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2. The claim that a study suffers from a methodological problem is only of interest insofar as it shows that the results produced by the study may be artefactual: the observation that the study fails to eliminate confounding variables is not enough on its own to invalidate the study's findings - it is, as I have said, a thoroughly open question.

that a fully fledged statistical theory of quantitative review has emerged. Glass (1976), one of the architects of the theory, dubs quantitative review 'meta-analysis'. 'Meta-analysis' is not strictly used in the generic sense imputed to it by Glass, and practitioners often disagree about statistically acceptable techniques. The most advisable route to take in this dissertation seems to adopt the more conservative of the approaches in the literature. Consequently, the meta-analysis reported here is modelled on that outlined in Hunter et. al. (1982), which advises against the use of inferential methods.

3.2 The Approach to be Adopted Here: Hunter et al. (1982)

Meta-analysis is the quantitative cumulation and analysis of descriptive statistics across studies (Hunter et al. 1982; p. 137). In the case of experimental studies, the primary focus is on effect sizes. To the extent that effect sizes vary across studies, they exhibit the influence of (i) artefactual error, which includes that introduced by sampling error; measurement error; and error due to restriction or enhancement of range, which are contaminating sources of variation; and (ii) moderator variables, which are substantive sources of variation. Meta-analysis derives its usefulness from its ability to statistically correct effect sizes for these types of error, and its ability to identify theoretically substantive sources of variation.

Hunter et al.'s quantitative review method proceeds in the following manner:

i. The collection of the set of all studies that provide empirical evidence that bears on the relationship of interest (in this case, an experimental-control difference).[3]

ii. The expression of the relationship of interest as a common statistic, for each study.

iii. The computation of the mean value of the statistic across studies. This may be a good estimate of the mean population value across studies, but the variance across studies is greatly inflated by sampling error.

iv. Thus, the observed variance across studies is corrected to eliminate the effect of sampling error.

v. Then the mean and variance of population values are corrected for the effect of error of measurement and range variation.

vi. If a large variation in the computed statistic still exists across studies, then the review proceeds by searching for moderator variables.

vii. Studies are coded (on the basis of theoretical, logical, statistical and psychometric considerations) for properties that vary across studies (p. 140).

viii. These properties (or moderator variables) are used to split the studies into subsets. Meta-analysis is

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3. The set of all studies is obviously preferable to a 'convenience sample', but is virtually impossible to obtain. Even the set of published studies is a difficult sample to acquire, and Hunter et al. make provision for the meta-analysis of a sample of studies that is convenient to acquire.

> applied to each subset separately. If moderator variables are operative in the studies reviewed, then large differences between subset means should appear, with corresponding reduction in within-subset variation across studies. Meta-analysis shows how much of the residual within-subsets variation is due to artifacts. (Hunter et al. p 140)

3.3 Method

3.3.1 IDENTIFICATION OF STUDIES

A literature search of journals that report studies in English and that are abstracted in Psychological Abstracts and Social Science Citation Index, was conducted. The Dialog computerized literature search service conducted part of the literature search. Studies that reported experiments in which postevent information was a stated concern were selected for analysis.

Of these studies, those that reported differences between experimental and control groups in terms of proportions were selected for the meta-analysis. This is in accord with the methodological dispute, which is the central concern of this dissertation, but unfortunately overlooks a substantial number of studies that take a different measure as the dependent variable.

Altogether 70 'experiments' were selected for meta-analysis, from some 25 studies. (An experiment is defined here as an experimental - control comparison.)

3.3.2 CONSTRUCTION OF A COMPARISON STATISTIC

An estimate of the effect of the experimental manipulation on the proportions of experimental and control groups correctly completing the required task was calculated using probit transformations of the difference between the two groups ie. by differencing the standard normal deviates corresponding to the proportions observed in the experimental and control groups (Glass et al. 1981, p 138).

3.3.3 PROCEDURE

The meta-analysis was conducted in a stepwise manner, breaking down sets of studies according to residual variance remaining after the meta-analysis at the previous level. Unfortunately only correction for sampling error could be made, as the psychometric properties of the measures used in postevent information studies are not well enough documented to permit correction for measurement error, or for restriction of range.

Each meta-analysis proceeded in the following manner:[4]

i. The calculation of the mean effect size. Each effect is weighted by the number of subjects used in the experiment. The formula for mean effect size is:

$$\underline{dm} = \frac{\Sigma[Nidi]}{\Sigma Ni}$$

[4] Meta-analytic procedures, as I noted before, differ widely in practice, and I report the procedure adopted here in considerable detail. I have taken the formulae from Hunter et al.

ii. The calculation of the variance of effect size. Again, each effect is weighted by the number of subjects in the experiment. The formula follows:

$$\sigma^2_d = \frac{\Sigma[Ni(di - \underline{dm})^2]}{\Sigma Ni}$$

iii. The calculation of sampling error in the observed effect sizes. The formula follows:

$$\sigma^2_e = \frac{4(1 + \underline{dm}^2/8)K}{N}$$

iv. The correction of the variance in effect sizes for sampling error:

$$\sigma^2(\delta) = \sigma^2_d - \sigma^2_e$$

v. Thus, if the observed distribution of effect sizes is characterized by dm and σ_d, then the actual distribution of effect sizes is characterized by δ and σ, where

$$\delta = \underline{dm}$$

$$\sigma(\delta) = \surd(\sigma^2_d - \sigma^2_e)$$

If effect size is really the same across studies, then $\sigma(\delta)$ will be approximately 0. If the variation is large, relative to the mean effect size, then it is permissible to look for moderator variables.

3.3.4 STEPWISE ANALYSIS

3.3.4.1 ALL STUDIES INCLUDED

A total of 2041 subjects constitute the sample[5]. The mean effect (d) is 0.581, and the variance of the observed effects is 0.299. After correction for sampling error (which is 0.116), the variance is 0.183, which means that the differences in effect sizes between studies are due to real differences in the properties of the studies. (Table 6.8).

Accordingly, studies were coded to permit inspection of differences between studies. Studies were coded on 13 properties. Table 6.7 depicts the coding used. Of the properties that were coded for, the most important are (i) the structure of the final recognition test; and (ii) the 'paradigmatic' status of the experiment[6].

Although meta-analysis often purports to be a strictly empirical pursuit, in the model followed here, theoretical expectations are an important guide for the analysis. In terms of the methodological dispute introduced earlier, these may be stated as hypotheses:

Hypothesis 1: By the claim of artefact, the mean effect size will vary according to the structure of the final recognition test. A linear difference between consistent, neutral and inconsistent postevent information will be observed. In addition, there will be differences

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5. The sample, in a meta-analysis, is not the set of studies that constitute the body of research, but the total number of subjects, pooled across the identified studies.

6. Although certain experiments use the same recognition test structure as Loftus et al. (1978), the paradigm case, they may differ in the experimental procedure used (as in Bekerian & Bowers, 1983, 1984).

Table 6.7. Coding used in meta-analysis of postevent information studies.

1. AUTHORS (of study)

2. SCENARIO (ie. Codes = Representational event methodology; Live event methodology)

3. PARADIGMATIC STATUS OF EXPERIMENT: (ie. Paradigmatic = Test structure used by Loftus et al. 1978: Codes = Paradigmatic; Revised; Question wording):

4. MATERIALS: (Codes = Film; Video; Staged)

5. OPTIONS: (No. of options on final test. Codes = No. of alternatives)

6. INFO PAIRING: (Codes = Consistent PEI vs Neutral PEI; Consistent PEI vs Inconsistent PEI; Neutral PEI vs Inconsistent PEI)

7. PROPORTION: (of control subjects choosing correct option on final test)

8. PROPORTION: (of experimental subjects choosing correct option on final test)

9. EFFECT: (Probit transformation of differences between proportions of control and experimental subjects.

10. N [control]
    N [experimental]

11. STATISTIC: Where reported.

12. PROBABILTIY: Reported significance level of experimental - control differences.

13. DELAY: ie. Between presentation of PEI and final test.

TABLE 6.8. **Results of the meta-analysis: data for the entire body of postevent information research.**

STATISTIC	**VALUE**
dm	0.581
ΣN	2041
σ^2_d	0.299
σ^2_e	0.116
$\sigma^2(\delta)$	0.183

TABLE 6.9. **Results of the meta-analysis: consistent postevent information vs inconsistent postevent information.**

STATISTIC	**VALUE**
dm	0.756
ΣN	275
σ^2_d	0.110
σ^2_e	0.124
$\sigma^2(\delta)$	-0.01

between studies that use the modified test structure and those that don't that cannot be explained at the level of random sampling variation.

Hypothesis 2: By the claim that postevent information studies show more than just the operation of experimental artefact, we may find the linear difference hypothesized in Hypothesis 1, but the differences between studies that use the modified test structure and those that don't will be explicable at the level of random sampling variation.

3.3.4.1.1 Consistent postevent information vs Inconsistent postevent information.

The average effect size produced by this manipulation is 0.756, but the variance around this figure is substantial - 0.11. Corrections for sampling error reduce the variance to a negligible amount (-0.014)[7], suggesting that differences between studies are entirely attributable to sampling error. (Table 6.9).

In practical terms, this means that the transformation of the paradigmatic experimental design introduced by Bekerian & Bowers (1983, 1984) has no real effect on experimental - control differences, but this conclusion may be a bit hasty, as only two experiments employing their manipulation are included in the analysis. The substantial variation around the mean effect size may be real, and the correction for sampling error, which weights effect sizes by the number of subjects used in experiments, may overcorrect - in the sense that the differences in mean effect size produced by Bekerian & Bowers' experiments may not be based on enough subjects to eliminate suspicions of sampling error.

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7. Negative variances (here and in other places in the meta-analysis) should be taken to mean 'close to zero variance'.

Excluding the Bekerian & Bowers studies, we find that the mean effect size is 0.92, with a variance of 0.025, which shows that most of the variation around the mean effect size is introduced by the Bekerian & Bowers studies. (Table 6.10).

The conclusion then, is that the best estimate of the mean effect size for studies in which the critical comparison is between consistent and inconsistent postevent information is 0.75, and although there are substantial differences between different studies using this manipulation, there is insufficient statistical evidence to suggest that these differences are not due to sampling error.

3.3.4.1.2 Consistent postevent information vs Neutral postevent information.

The mean effect size for experiments in which Consistent postevent information was paired with Neutral postevent information is 0.39, with a variance around the mean of 0.016. The variance here, relative to the mean effect size, is negligible, which means that exposing subjects to postevent information reliably improves their performance on the final recognition test relative to subjects given neutral postevent information. (Table 6.11).

3.3.4.1.3 Neutral postevent information vs Inconsistent postevent information.

The mean effect size for the prototypical experimental manipulation (ie. pairing inconsistent and neutral postevent information) is 0.631. Variance around the mean is 0.283, which corrected for sampling error, becomes 0.164. This

TABLE 6.10. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information; data from the Bekerian & Bowers (1983, 1984) revisions removed

STATISTIC	VALUE
<u>dm</u>	0.924
ΣN	167
$\sigma^2{}_d$	0.022
$\sigma^2{}_e$	0.13
$\sigma^2(\delta)$	-0.109

TABLE 6.11. Results of the meta-analysis: consistent postevent information vs neutral postevent information

STATISTIC	VALUE
<u>dm</u>	-0.39
ΣN	105
$\sigma^2{}_d$	0.016
$\sigma^2{}_e$	0.077639
$\sigma^2(\delta)$	-0.061639

large remaining variance suggests that the differences between studies that use this manipulation are non-trivial, and are not explicable as sampling artefact. (Table 6.12).

Accordingly, the studies that use this manipulation need to be broken into subgroups for analysis on the basis of further moderating variables.

In the previous chapter I introduced the central internal theoretical dispute, namely the question of whether postevent information *alters* memorial representations, or merely renders the task of remembering certain details more difficult. This theoretical dispute provides an interesting way to break the remaining studies into subgroups: studies that use the paradigmatic experimental procedure may be expected to produce a consistently large experimental-control difference, and studies that introduce innovations into the typical design may be expected to produce no differences of note between experimental and control groups.

3.3.4.1.3.1 Studies using the paradigmatic design.

The mean effect size for this set of experiments is 0.774, with a variance about this figure of 0.134. Correction for sampling error reduces the variance to 0.024, but when this is transformed to a standard deviation, the resulting estimate of variability is 0.154. The 95% confidence interval for the mean effect size under these circumstances would thus be 0.466 - 1.08, which is rather uninformative about the true value of the mean effect size. (Table 6.13).

TABLE 6.12. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information

STATISTIC	VALUE
<u>dm</u>	0.613444
ΣN	1661
$\sigma^2{}_d$	0.283263
$\sigma^2{}_e$	0.118509
$\sigma^2(\delta)$	0.164754
$\sigma(\delta)$	0.405899

TABLE 6.13 Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: paradigmatic results

STATISTIC	VALUE
<u>dm</u>	0.836
ΣN	795
$\sigma^2{}_d$	0.075
$\sigma^2{}_e$	0.098
$\sigma^2(\delta)$	-0.022

Consequently, this body of studies was broken down into three subgroups. These subgroups were identified on the basis of methodological deviations from the paradigmatic (Loftus et al. 1978) study, with the proviso that they retained the crucial paradigmatic pairing on the final recognition test (ie. neutral postevent information vs inconsistent postevent information). The identified sub-groups were (i) studies that used hypnotic induction; (ii) studies that conflated presentation of misinformation and the recognition test - all these studies explored so-called questioning effects, and were covered in some detail in the literature review (as #1); and (iii) the remaining studies, all of which stuck strictly to the paradigmatic experimental design.

3.3.4.1.3.1.1 Studies that use hypnotic induction.

In this set of studies, hypnotic induction forms a crucial part of the experimental design. As I indicated in the literature review, the hypotheses explored in these studies usually concern the mediating effects of hypnotic induction on suggestibility of subjects exposed to misinformation.

The mean effect size estimate for this set of studies is 0.678, with a variance around this figure of 0.174. Correction for sampling error reduces the variance to 0.045, but when this figure is transformed to a standard deviation, the estimate of variability is 0.20 (95% confidence interval = 0.278 - 1.078), which suggests that observed differences in effect size across studies are not attributable to sampling error, and should be accorded theoretical weight. (Table 6.14).

The search for moderator variables in this particular subgroup of studies was terminated here, as the studies under investigation were not coded on a sufficient number of properties to admit further analysis.

3.3.4.1.3.1.2 Studies that explore questioning effects.

The mean effect size estimate here is 0.729, with a variance around this figure of 0.223. Correction for sampling error reduces the variance to 0.11, which means that the standard deviation is 0.338, which, for reasons given earlier, probably means that the effect size estimate is unreliable, and that variation of effect sizes across studies is real. (Table 6.15).

The search for moderator variables in this particular set of studies was abandoned at this stage. There seems little theoretical purpose in pursuing the matter further here, simply because there is too little information to take the search for moderator variables any further.

3.3.4.1.3.1.3 Strictly paradigmatic designs.

The mean effect size for this set of experiments (N = 795) is 0.83, with a variance about this figure of 0.075. Correction for sampling error reduces the variance to -0.02, which suggests that the mean effect estimate is fairly reliable, and that differences between studies are probably due to sampling error. (Table 6.16).

TABLE 6.14. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: hypnosis studies

STATISTIC	VALUE
<u>dm</u>	0.678
ΣN	221
σ^2_d	0.174
σ^2_e	0.133
$\sigma^2(\delta)$	0.042
$\sigma(\delta)$	0.200

TABLE 6.15. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: question wording studies.

STATISTIC	VALUE
<u>dm</u>	0.729416
ΣN	274
σ^2_d	0.223797
σ^2_e	0.108986
$\sigma^2(\delta)$	0.114811
$\sigma(\delta)$	0.338837

From a theoretical point of view, this analysis means that experiments that pair inconsistent and neutral postevent information, under paradigmatic experimental conditions, produce a statistically real experimental - control difference.

3.3.4.1.3.2 Studies that revise the paradigmatic experimental design.

The mean effect size across experiments that revise the paradigmatic methodology is 0.010. The dispersion around this figure is high (variance = 0.374), and correction for sampling error does little to remedy the situation (corrected variance = 0.22), which means that the mean effect estimate is unreliable, and that differences between studies are probably real (ie. not due to attenuation or enhancement by statistical artefact). (Table 6.17).

Accordingly, these studies were broken down further in a search for moderator variables. Only two reported studies constitute the body of studies under scrutiny (Christiaansen et al., 1983; McCloskey & Zaragoza, 1985a), so two subgroups were readily identifiable.[7] Of these, only the group constituted by McCloskey & Zaragoza's study was subjected to further meta-analysis. (Christiaansen et al's study contained too many conditions to make a meta-analysis useful.)

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7. The lack of a theoretical or methodological similarity between the two sets of studies made it necessary to treat the two subgroups as unique.

TABLE 6.16. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: strictly paradigmatic results

STATISTIC	VALUE
<u>dm</u>	0.836
ΣN	795
$\sigma^2{}_d$	0.075
$\sigma^2{}_e$	0.984
$\sigma^2(\delta)$	-0.022

TABLE 6.17 Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: studies using revised methodologies.

STATISTIC	VALUE
<u>dm</u>	0.017
ΣN	351
$\sigma^2{}_d$	0.37457
$\sigma^2{}_e$	0.14815
$\sigma^2(\delta)$	0.22642
$\sigma(\delta)$	0.475836

The series of experiments reported by McCloskey & Zaragoza have an estimated mean effect size of -0.055, with a variance of 0.023, which corrected for sampling error, becomes -0.084. This suggests that there is a negligible difference between experimental and control groups on the final recognition test in experiments in which neutral and inconsistent postevent information are paired (in the final test), and that observed differences are due to sampling error. (Table 6.18).

3.4 Conclusions

Fig 6.01 summarizes the discriminations that the meta-analysis is able to make. Several things that have a bearing on the postevent information literature reviewed earlier and on the methodological dispute surrounding postevent information research are identifiable from the figure and the preceding analysis.

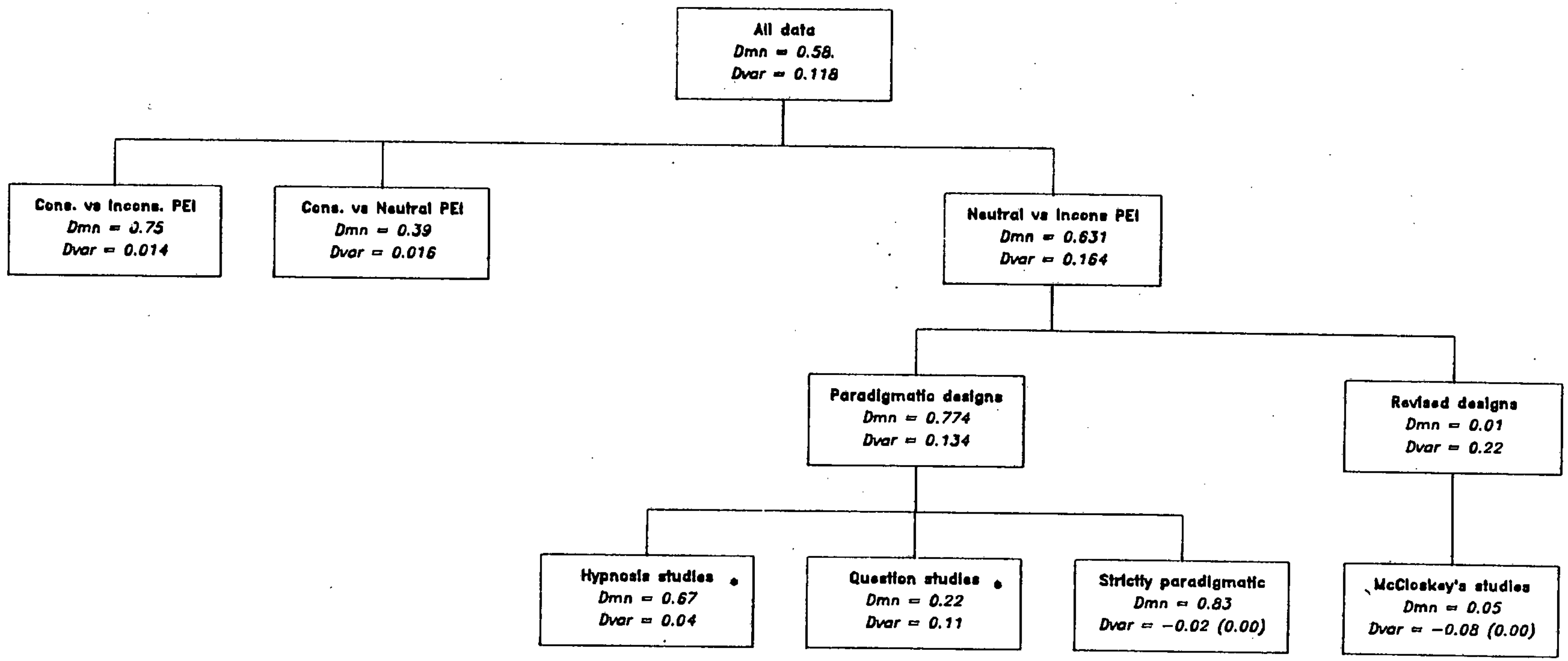
In the first place, both the question wording and hypnosis studies produce experimental outcomes that vary substantively intra-paradigmatically: reported disconfirmations and non-replications are not due to random sampling variation, they constitute real disagreements. Unfortunately, the meta-analysis does not identify the source(s) of the variation within these paradigms, but as the studies reporting disconfirmations and non-replications differ unremarkably from paradigmatic research, it bodes poorly for the external validity of the research.

Superficially, results seem to support the claim of artefact. In terms of hypotheses 1 and 2, outlined in section 3.3.4.1, studies which pair Consistent and Neutral

TABLE 6.18. Results of the meta-analysis: consistent postevent information vs inconsistent postevent information: McCloskey & Zaragoza's studies.

STATISTIC	VALUE
<u>dm</u>	-0.055
ΣN	222
σ^2_d	0.023
σ^2_e	0.108
$\sigma^2(\delta)$	-0.084

Fig 6.01. Discriminable sets of studies identified by meta-analysis



+ Dmn = Mean effect size;
  Dvar = Variation in effect size, corrected for sampling error

* Analysis terminated

postevent information show a consistently large effect size, as do studies which pair Neutral and Inconsistent postevent information, producing the expected linear effect (ie. Consistent postevent information > Neutral postevent information > Inconsistent postevent information); and, in addition, differences between effect sizes produced by studies using the paradigmatic test structure, and studies using the revised test structure, are substantive, and cannot be dismissed as random sampling variation. In addition, the argument advanced by Headed Records Theory as explanation of the postevent information effect (ie. that the effect is due to retrieval difficulties) is disconfirmed by the linear effect - HRT does not predict that consistent postevent information will provide experimental groups with a statistically real advantage over control groups[8].

Nevertheless, this cannot be said to settle issues in favour of the claim of artefact. Although the expected linear effect is identified, the mean effect sizes do not reflect this directly: strictly speaking, under the algebraic model embodied in the claim of artefact, the effect size observed in studies pairing Consistent and Neutral postevent information should be *equivalent* to that observed in studies pairing Neutral and Inconsistent postevent information. It clearly isn't: referring to Figure 6.01, the first of these effect sizes is 0.39, and the second is 0.83. In addition, studies that pair Consistent and Inconsistent postevent information show an effect size of only 0.75, which is also out of keeping with the algebraic model - it should be larger than either of the other pairings. (However, this

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8. I argue this in greater detail later in the chapter, when I discuss the results of my experiment.

might be due to an attenuation of effect size by the Bekerian & Bowers studies - a more reliable estimate of the effect size may be 0.92, which is more in keeping with the algebraic model; see section 3.3.4.1.1).

The meta-analysis reported here thus indicates *equivocal* support for the claim of artefact lodged by McCloskey & Zaragoza, in addition to clarifying some inconsistencies in the postevent information literature.

4. EXPERIMENTAL STUDY

4.1 Rationale

It's clear from the evaluation of McCloskey & Zaragoza's claim presented in the previous chapter that there is a need for additional empirical estimates of experimental artefact in postevent information studies. McCloskey & Zaragoza's findings using the revised recognition test procedure need to be replicated, and in particular, the claim of artefact needs to be corroborated by a procedure that is independent of that adopted by McCloskey & Zaragoza.

In the experiments to be reported here, two methods of assessing experimental bias in a paradigmatic postevent information experiment were identified and utilized.

In the first of these methods, scores on the forced choice recognition test are corrected for 'initial accuracy'. McCloskey & Zaragoza's (1985a) argument is that differences between control and experimental groups in postevent information experiments may be attributed largely (but not exclusively) to incorrect performance on the final recognition test by subjects who don't encode - or subsequently forget - original information, but do remember postevent information, thus choosing it in the final test, and artificially lowering experimental group performance. If all subjects are first scored for memory of original information immediately after exposure to original information, and only those who accurately remember the information at this stage considered in the analysis of the forced choice test, then this large source of bias should be eliminated from the experiment.

This method of correcting test scores removes all those subjects who don't get information into memory in the first place (or have subsequently forgotten it), and thus all subjects who would be expected to perform correctly or incorrectly either by chance or by remembering the (misleading) postevent information, but not the original information. Unfortunately this procedure also allows the possibility that differences may still be manifested, as it leaves those subjects who do remember the original information but nevertheless choose incorrectly on the final test (because they, say, remember both original and misleading information, but choose the misleading information on the final test - see McCloskey & Zaragoza, 1985a.) Nonetheless, should the correction attenuate the misleading effect significantly (such that, for example, differences between experimental and misled groups fail to reach statistical significance), then this second source of experimental bias is probably not worth bothering about.

In the experiments to be reported here, estimates of initial accuracy were obtained by giving subjects a free recall test of their memories for original information immediately after the observed event. Responses of subjects on the final recognition test were corrected for initial accuracy on this recall test. Problems with this method include the possibility of overcorrecting scores: as recall tests are reportedly worse measures of memory than recognition tests[9] (ie. the same subject might 'remember' the correct information on the forced choice test, but not on the recall test), subjects who would have remembered the original information given a forced choice test will be eliminated from the analysis. As the point of the analysis is to look for misinformation effects, it may be precisely these subjects with 'weak' memories that are most prone to misinformation effects, and thus, eliminating these scores from the analysis may serve to attenuate a real effect. The initial accuracy method was pursued here despite this possibility: as Loftus' claim is that memorial representations are altered by postevent information, it is important to demonstrate that original information is encoded in the first place. In addition, Loftus makes the claim in her 1979 book that several of her experiments show memory change in subjects in which encoding of original information was experimentally verified.

The second method of obtaining an independent estimate of the bias operating in postevent information studies in this experiment consisted in a restructuring of the final recognition test so that *misleading information* was paired with *new information* (ie. information not seen previously in the experiment), thus further revising the test used in postevent information studies to assess memory impairment. Table 6.19 schematizes three forced choice test structures: the first is the paradigmatic structure, the original test, used by Loftus and her colleagues, the second is McCloskey &

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9. Zaragoza et al. (1987) cover this point in some detail.

Table 6.19 Test item structures of recognition tests used in postevent information experiments.

TEST PROCEDURE	TEST ITEM STRUCTURE
ORIGINAL (LOFTUS ET AL. 1978)	ORIGINAL INFORMATION VS POSTEVENT INFORMATION
MODIFIED (MCCLOSKEY & ZARAGOZA 1985A)	ORIGINAL INFORMATION VS NEW INFORMATION
NEW (THIS STUDY)	POSTEVENT INFORMATION VS NEW INFORMATION

the first is the paradigmatic structure, the original test, used by Loftus and her colleagues, the second is McCloskey & Zaragoza's (1985a) modified test, and the third is the new, restructured version of the test used in this study.

Subjects who choose the incorrect sign in the *new* forced choice test should be those subjects who do not remember the original information, but who do remember the misleading postevent information and think that it is the original information, thus choosing it in the final test and consequently lowering the performance of the experimental group. The new test procedure thus allows an *independent* estimate of the biasing factor in postevent information experiments.

In the experiment to be reported here, the 3 test procedures outlined above constitute levels of a between subjects factor that is crossed with a 3 level within subjects factor. The within subjects factor is constituted by information given to subjects in the postevent stage of the experiment: subjects were given *consistent*, *neutral*, and *inconsistent* postevent information.

The use of these two methods to estimate the extent of experimental bias in postevent information experiments allows us to choose not only between Loftus' (and her colleagues') explanation of the results of postevent information experiments, and McCloskey & Zaragoza's (1985a) hypothesis, but also between theories of memory inaccessibility and McCloskey & Zaragoza's (1985a) hypothesis.

By Loftus' hypothesis of memorial alteration (Loftus et al. 1978) (in the case of exposure to inconsistent postevent information), and memorial strengthening (Loftus 1975)(in the case of consistent postevent information), we expect all three test procedures to boost consistent scores above neutral scores, and to lower inconsistent scores below neutral scores. However, by McCloskey & Zaragoza's (1985a) hypothesis of experimental artefact, we expect the original test procedure to (i) boost consistent scores and lower inconsistent scores only in the original test procedure; (ii) to collapse differences between the three types of information on the modified test, and to (iii) produce only lowering of inconsistent scores on the new test procedure. The last of these tests will produce the most decisive evidence w.r.t. the dispute between Loftus and McCloskey & Zaragoza (1985a): as the test pairs misleading and new information, those subjects who have forgotten the original information (or who might not have encoded it in the first place), and who are exposed to consistent postevent information will not be able to choose consistent information on the final test, for it is deliberately excluded. Thus, if Loftus is correct, we expect a linear effect in the new test procedure (consistent > neutral > inconsistent), and if McCloskey & Zaragoza are correct then we expect scores to be lowered in the case of inconsistent information, but neutral and consistent scores to be equal.

It's useful to represent these hypotheses formally here. Figures 6.1 and 6.2 represent hypotheses diagrammatically, and the following section states them formally.

4.2 Hypotheses

The dependent variable for each of the comparisons outlined below is accuracy on the final recognition test.

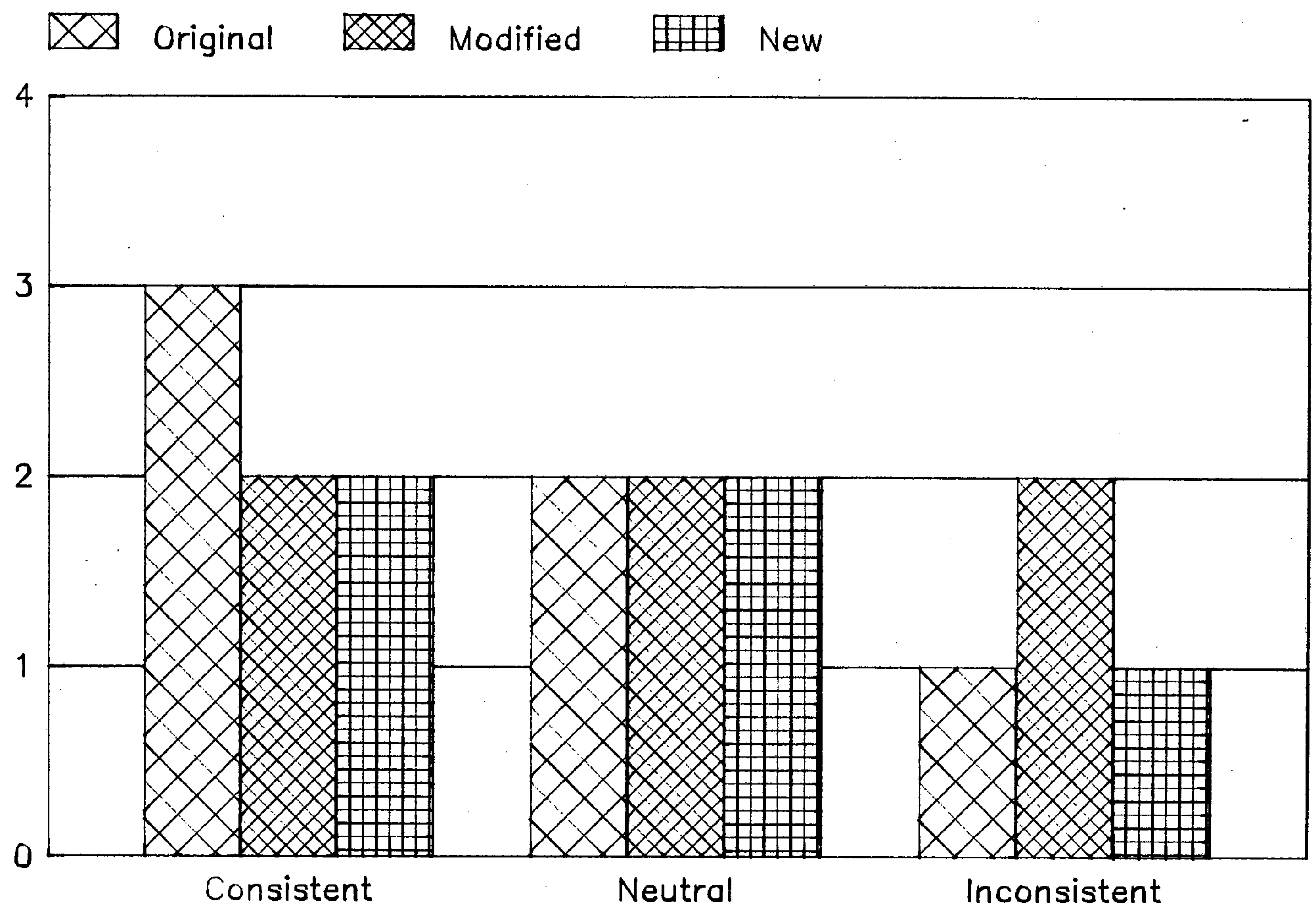
4.2.1 THE TEST STRUCTURES

Differences between types of recognition test are expected under the hypothesis of experimental artefact. When the postevent information is consistent with original event information, the original test procedure should inflate performance above both the modified and new test procedures. When the postevent information is inconsistent the original test procedure should deflate performance below the modified test procedure, but remain equal to performance measured with the new test procedure. When the postevent information is neutral there should be no differences between the test procedures. This complex hypothesis is represented diagrammatically in Figure 6.1.

4.2.2 THE INFORMATION TYPES

The experimental effect that is paradigmatically said to show the influence of misleading information on memory is a linear difference between types of postevent information that subjects are exposed to. Thus, for the original test procedure we expect a linear difference between information types (ie. consistent > neutral > inconsistent postevent information). However, for the modified test procedure no differences between information types are expected. Finally, in the new test procedure, differences are expected between performance after neutral and inconsistent postevent information, but not between neutral and consistent

FIG 6.1: EXPECTED PERFORMANCE ON THE FINAL TEST: INFORMATION TYPE



* The Y axis is a numerical representation of expected differences

postevent information. This complex set of hypotheses is represented in Figure 6.2.

4.3 Pilot Study

A pilot study, which used eight subjects, was conducted as a routine check on the feasibility of the experimental procedure and materials. The experimental procedure used matched that reported for the main experiment as closely as possible.

On the basis of subject response in the pilot experiment, the structure of the 'new' recognition test was altered to include a fourth option, which allowed the subjects to indicate the absence of original information. This modification is outlined in a footnote to the METHOD section below.

4.4 METHOD

4.4.1 SUBJECTS

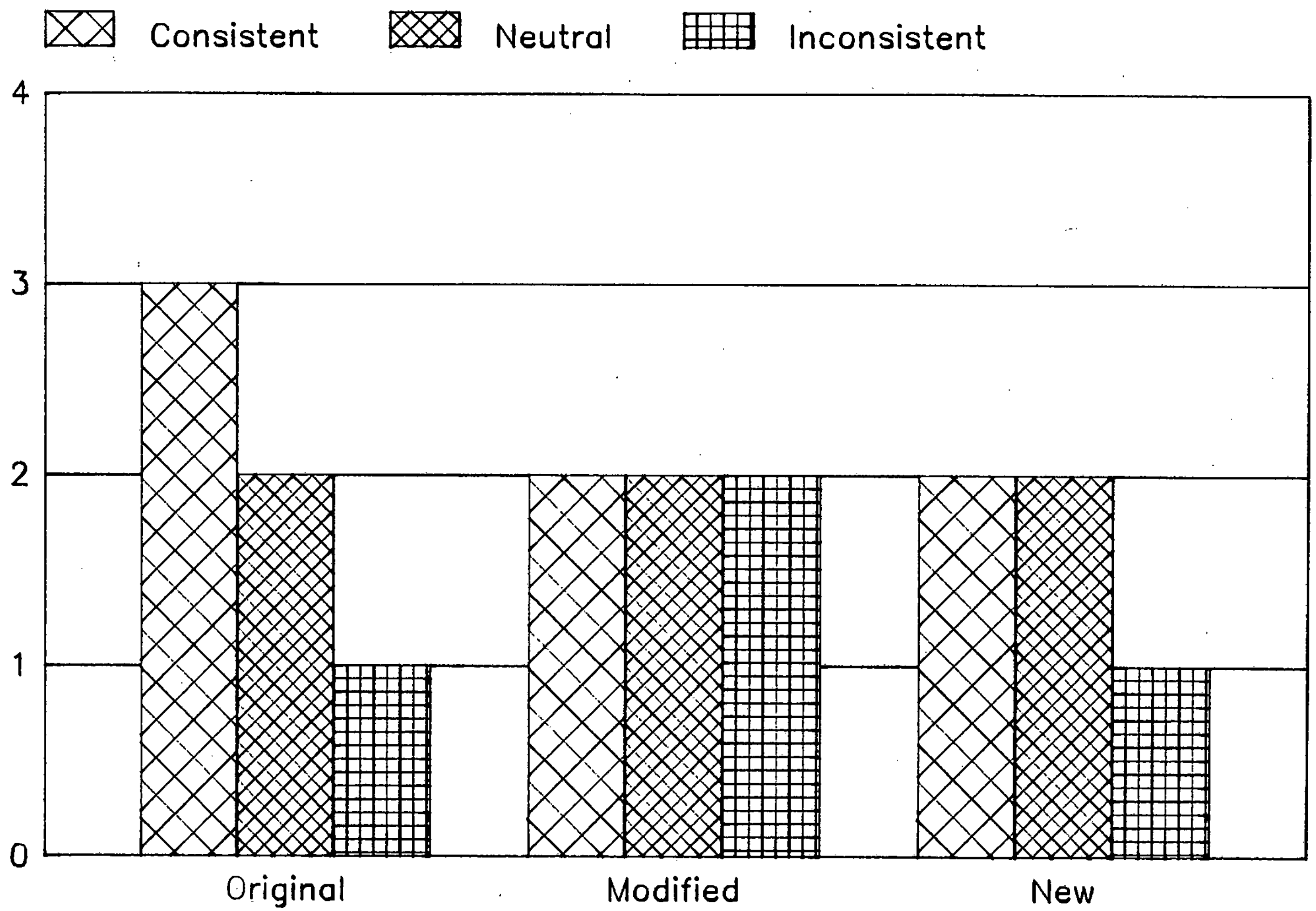
78 students from the University of Cape Town introductory psychology class participated in this experiment as part of a practical in cognitive psychology. Subjects were seen in groups ranging from 18 - 23.

4.4.2 MATERIALS

4.4.2.1 NARRATIVE.

Bartlett's (1932) 'War of the Ghosts' passage was used as the material for the original information that subjects would later be misled for (the passage is included as Appendix 1). Nine critical questions based on the passage

FIG 6.2: EXPECTED PERFORMANCE ON THE FINAL TEST: TEST TYPE



* The Y axis is a numerical representation of expected differences

were constructed. Each of these in turn was constructed in a consistent, neutral, and inconsistent form. In the consistent form the question repeated the critical detail; in the neutral form the question made no (or neutral) mention of the critical detail; and in the inconsistent form the question gave misleading information about the critical detail. (See Appendix 2).

The presentation of the questions was counterbalanced across subjects. Each subject was exposed to three consistent, three neutral, and three inconsistent questions. Each question was asked in a consistent, neutral and inconsistent manner, counterbalanced across subjects.

4.4.2.2 INITIAL ACCURACY TEST.

One third of subjects in each of the three experimental conditions were given a free recall test, which tested for memory for the narrative. Nine of the (12) questions in the free recall test probed memory for the nine critical details in the passage. (Appendix 3).

4.4.2.3 QUESTIONNAIRES.

A questionnaire (consisting of 18 questions addressing the narrative) was constructed, which subjects were asked to answer. Nine of the questions referred to the critical details and either gave consistent, neutral or inconsistent information about the critical details. This postevent information was always embedded in a subordinate clause in the question. (Appendix 2).

4.4.2.4 RECOGNITION TESTS.

Three recognition tests were prepared, each with 18 questions, of which 9 were critical. Each question in the test was answerable by circling one of a number of alternatives. In the original procedure version of the test, original and postevent information were paired, along with a third option which stated "can't remember". (Appendix 4). In the modified procedure version, original information was paired with information not previously seen, with again, a third option that allowed subjects who didn't remember the information to indicate this. (Appendix 5). In the new procedure version, postevent information was paired with information not seen before, with a third option allowing for the possibility of not remembering, and a fourth option which allowed the subject to indicate that neither of the paired items was correct.[10] (Appendix 6).

4.4.2.5 PROCEDURE

Subjects were randomly assigned to one of three conditions (Test type: original, modified, new). After a brief introduction to the academic requirements of the practical, subjects were asked to participate in an experiment. They were told that they would be informed of the purposes of the experiment at a later stage. Each subject was then given a document. Each of these documents contained a passage ("War of the Ghosts"), a questionnaire, and a test. One third of the documents also contained an initial accuracy test. Filler tasks separated the passage, questionnaire and test

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10. This option was included because of the danger of subjects who do remember the original information choosing the closest match (which would be an incorrect choice) at the time of the final test. This danger was identified in a pilot experiment.

(the phases of the experiment) from each other. (They appear as Appendix 7).

4.4.3 DESIGN.

The design consisted of a three level between subjects factor (Test type: original, modified, new) crossed with a three level within subjects factor (Information type: consistent, neutral, inconsistent).

A diagrammatic representation of the design appears as Table 6.20

4.5 Results.

Three scores were obtained for each subject. A *consistent* score, a *neutral* score, and an *inconsistent* score. These were obtained by summing the number of correct responses each subject made to questions that tested for memory for details that had had consistent, inconsistent or neutral postevent information presented about them in the postevent information stage of the experiment. In addition, two sets of scores were obtained for all subjects who completed an initial accuracy test: the first set of scores was uncorrected for initial accuracy, and the second set of scores was corrected for initial accuracy. Scores were corrected in the following manner: each question that a subject failed to answer correctly in the initial accuracy test was eliminated from the composite score on the final recognition test. Raw data appears as appendix 8.

As specific hypotheses had been formulated before the experiment, most of the analysis proceeded by planned

Table 6.20 Experimental Design.

FACTOR A: TEST TYPE

	ORIGINAL	MODIFIED	NEW
CONSISTENT			
NEUTRAL			
INCONSISTENT			

FACTOR B: INFORMATION TYPE

comparisons. Occasionally (where specific contrasts were unspecifiable)[12] the results reported are from analysis of variance procedures.

4.5.1 DIFFERENCES BETWEEN TEST PROCEDURES.

4.5.1.1 CONSISTENT POSTEVENT INFORMATION.

Subjects exposed to consistent postevent information who completed the original test procedure chose the correct option on the forced choice test more often than subjects who completed the other two test procedures. (No differences, in turn, were observed between these two test procedures. ie. Consistent - original (Mean = 2.54; Std dev. = 0.56) > consistent - modified (Mean = 2.22; Std dev. = 0.75) = consistent - new (Mean = 2.0; Std dev. = 0.84): $\underline{F}$ = 7.18, $\underline{df}$ = 1, 32, $\underline{p}$ < 0.009).

4.5.1.2 NEUTRAL POSTEVENT INFORMATION.

A simple main effect for neutral information type in a two way analysis of variance showed no differences between subjects exposed to neutral postevent information across any of the three test procedures. Neutral - original (Mean = 2.12; Std dev. = 0.78) = neutral - modified (Mean = 2.07; Std dev. = 0.67) = neutral - new (Mean = 2.00; Std dev. = 0.90): ($\underline{F}$ = 0.14, $\underline{df}$ = 2, 75, $\underline{p}$ < 0.86)

4.5.1.3 INCONSISTENT POSTEVENT INFORMATION.

Subjects who were exposed to inconsistent postevent information differed significantly according to the test

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12. As, for example, when the hypothesis is that of no differences. Analysis of variance tables for all analyses are reported in tabular format, and appear alongside the text, but are not referred to in the text.

Table 6.21 Analysis of variance summary table for a priori contrast - differences between test types for consistent postevent information.

EFFECT	STATISTIC	F	DF	P
HYPOTHESIS	SS = 3.52 MS = 3.52	7.18	1, 75	0.009
ERROR	SS = 36.84 MS = 0.49			

Table 6.22 Analysis of variance summary table: simple main effect; Neutral postevent information across Test type.

EFFECT	STATISTIC	F	DF	P
NEUTRAL PEI	SS = 0.171 MS = 0.085	0.14	2, 75	0.86
ERROR	SS = 45.3 MS = 0.60			

procedure they completed. Subjects who completed the original and new test procedures performed equally worse than subjects who completed the modified test procedure. (Inconsistent - modified (Mean = 2.29; Std dev. = 0.66) > Inconsistent - original (Mean = 1.66; Std dev. = 0.95) = Inconsistent - new (Mean = 1.66; Std dev. = 0.84): $\underline{F}$ = 9.60; $\underline{df}$ = 1, 17; $\underline{p} < 0.0027$)

A graphical representation of performance on the test broken down by Test type appears as Figure 6.3, and a further representation of statistically significant comparisons appears as Figure 6.4

The results from this analysis are clearly supportive of predictions derived from McCloskey & Zaragoza's (1985a) hypothesis of experimental artefact: performance of subjects exposed to consistent postevent information is boosted only on the original test procedure, and performance of subjects exposed to inconsistent postevent information is attenuated in the original *and* new test procedures.

4.5.2 DIFFERENCES WITHIN POSTEVENT INFORMATION TYPES.

4.5.2.1 ORIGINAL TEST

A linear effect was observed here: critical details that were represented consistently in the postevent information stage were more likely to be correctly recalled than when represented either neutrally or inconsistently. Similarly, details that were represented in a neutral way during the postevent stage were more likely to be recalled correctly than inconsistent postevent information. (Consistent - original (Mean = 2.54; Std dev. = .56) > Neutral - original

Table 6.23 Analysis of variance summary table for a priori contrast - differences between test types on inconsistent postevent information.

EFFECT	STATISTIC	F	DF	P
HYPOTHESIS	SS = 6.77 MS = 6.77	9.60	1, 17	0.0027
ERROR	SS = 5.29 MS = 0.70			

Table 6.24 Analysis of variance summary table for a priori contrast - differences between test types for consistent postevent information.

EFFECT	STATISTIC	F	DF	P
HYPOTHESIS	SS = 12.74 MS = 6.37	12.55	2, 31	0.0001
ERROR	SS = 30.58 MS = 0.47			

Fig 6.3 Performance on the Final Test: Information Type.

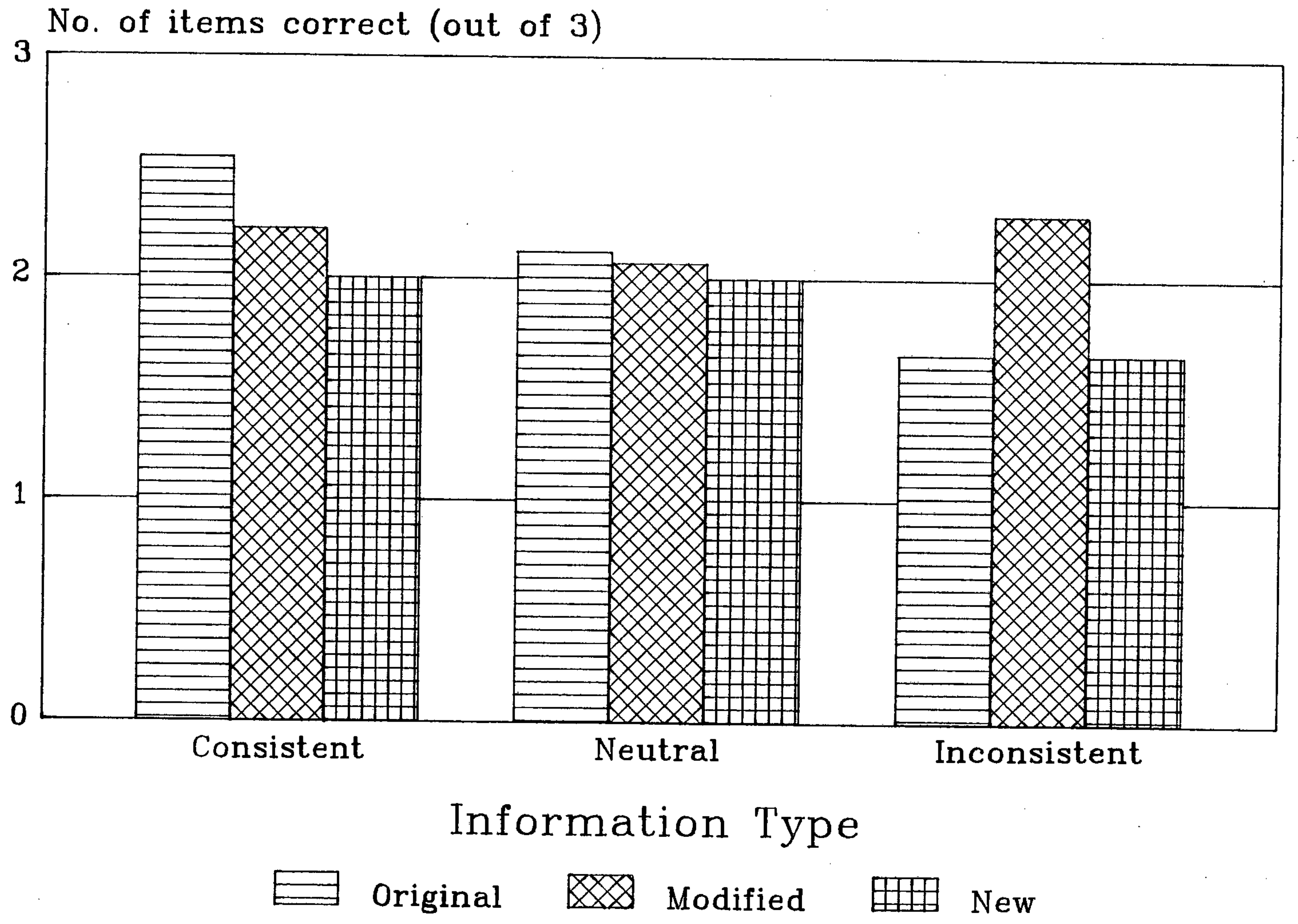
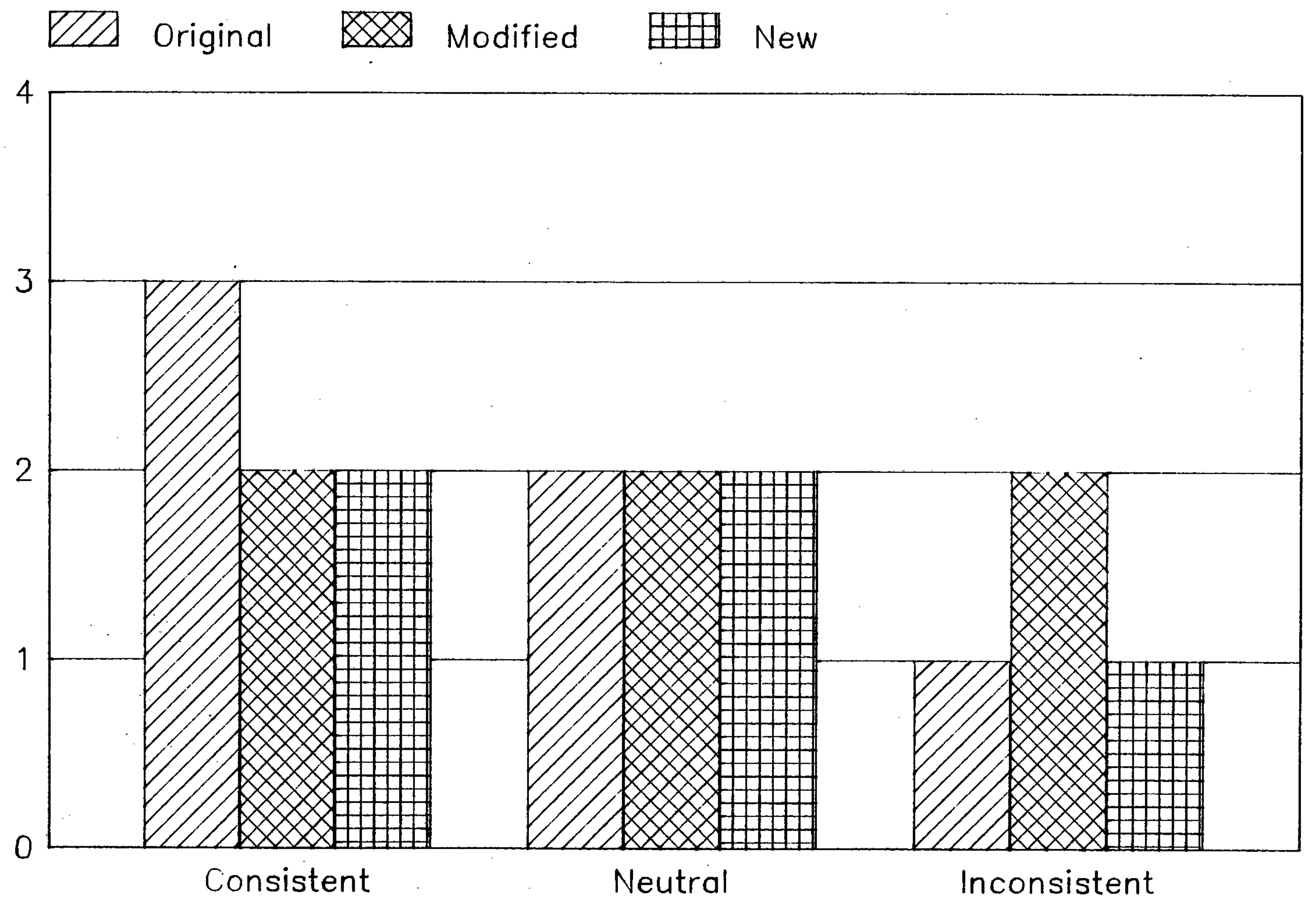


FIG 6.4. OBSERVED DIFFERENCES ON THE FINAL TEST: INFORMATION TYPE



* The Y axis is a numerical representation of observed differences

(Mean = 2.12; Std dev. = 0.78) > Inconsistent - original (Mean = 1.66; Std dev. = 0.95): F = 12.55, df = 2, 31 p < 0.0001).

4.5.2.2 MODIFIED TEST

A simple main effect for modified test type in a two way analysis of variance showed no differences within postevent information types: critical details that were presented in a consistent, inconsistent, and neutral way in the postevent information stage of the experiment showed no differences with respect to how often they were answered correctly. (Consistent - modified (Mean = 2.22; Std dev. = 0.75) = Neutral - modified (Mean = 2.07; Std dev. = 0.67) = Inconsistent - modified (Mean = 2.29; Std dev. = 0.66): F = 0.65, df = 2, 150, p < 0.526).

4.5.2.3 NEW TEST

Surprisingly, differences within information types were only marginally significant across the new test procedure: they failed to reach conventionally accepted levels of statistical significance (Consistent - new (Mean = 2.00; Std dev. = 0.84) = Neutral - new (Mean = 2.00; Std dev. = 0.90) = Inconsistent - new (Mean = 1.66; Std dev. = 0.84): F = 1.74, df = 1, 17, p < 0.20).

A graphical representation of performance on the test broken down by Information type appears as Figure 6.5, and a further representation of statistically significant comparisons appears as Figure 6.6

Table 6.25 Analysis of variance summary table: simple main effect; differences between information types on Modified test structure.

EFFECT	STATISTIC	F	DF	P
Modified Test	WCP SS = 0.691 WCP MS = 0.345	0.65	2, 150	0.52
Error	WCP SS = 77.89 WCP MS = 0.53			

Table 6.26 Analysis of variance summary table for a priori contrast - differences between information types on the new test structure

EFFECT	STATISTIC	F	DF	P
Hypothesis	SS = 1.33 MS = 1.33	1.74	1, 17	0.20
Error	SS = 13.33 MS = 0.76			

Fig 6.5: Performance on the Final Test: Test Type.

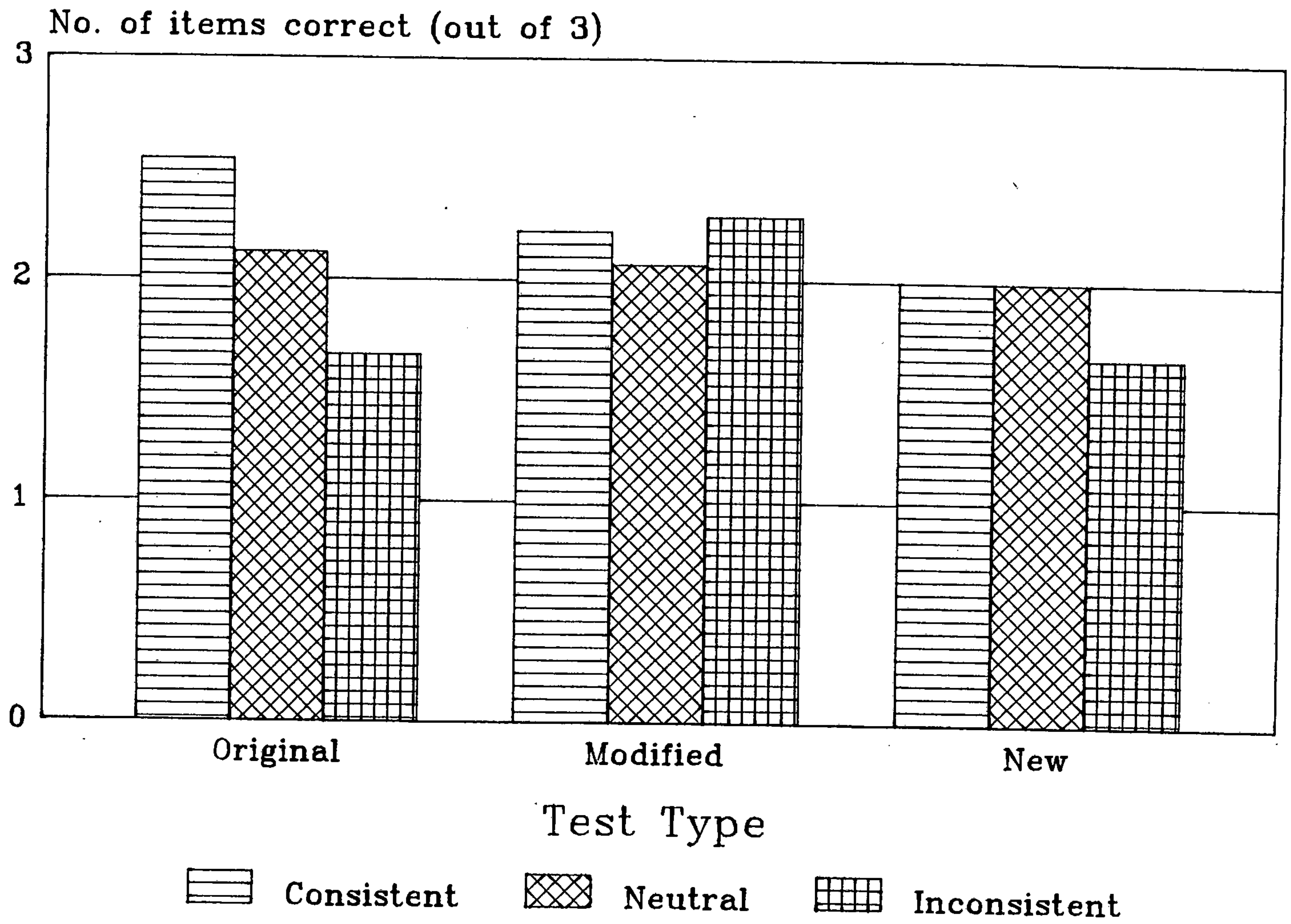
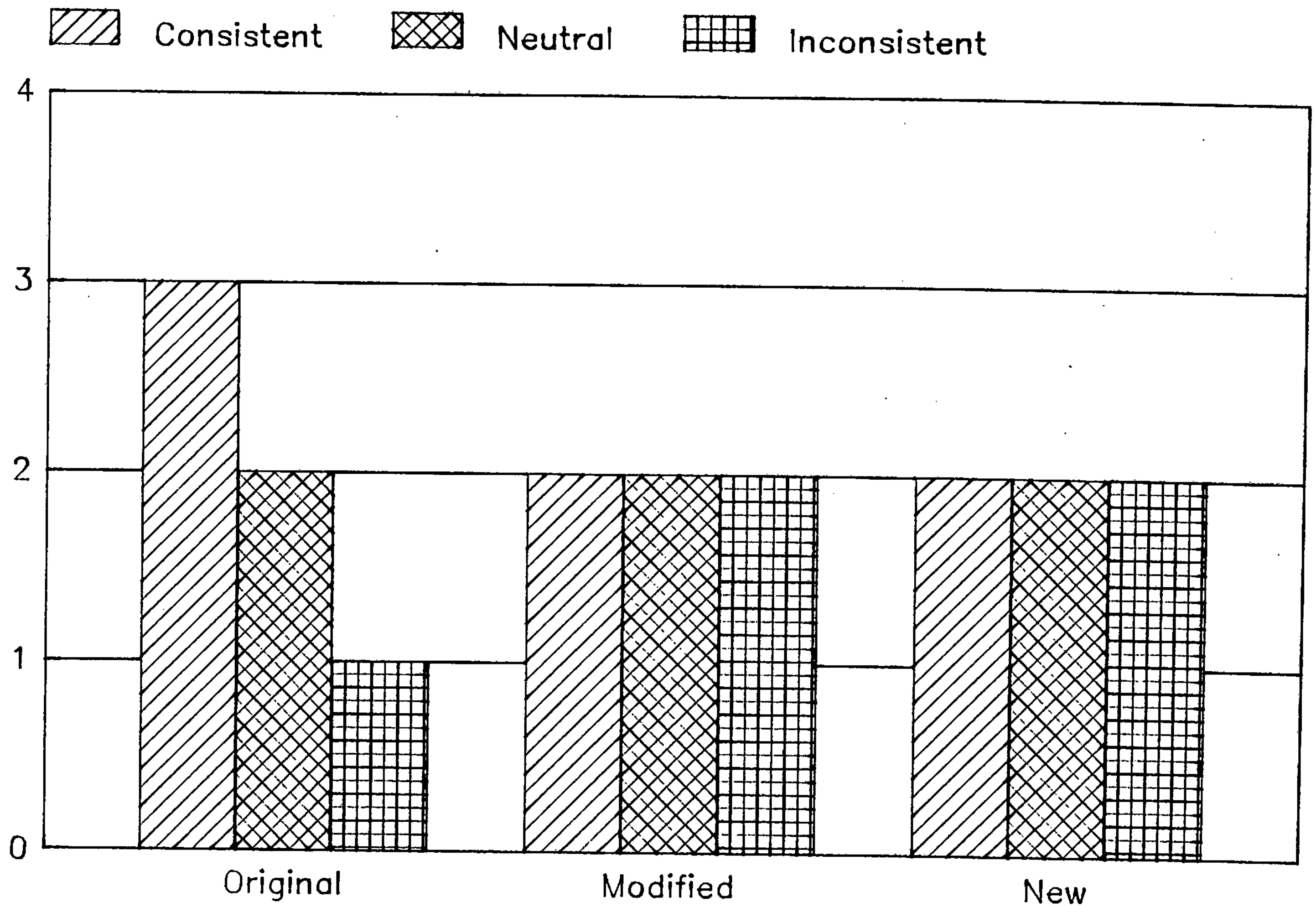


FIG 6.6: OBSERVED DIFFERENCES ON THE FINAL TEST: TEST TYPE



* The Y axis is a numerical representation of observed differences

Thus, although the results replicated findings typically made in paradigmatic postevent information studies and further replicated attenuation of these effects under the modified test procedure, predicted differences between information types on the new test procedure failed to reach statistical significance.

4.5.2.4 CORRECTIONS FOR INITIAL ACCURACY.

Scores corrected for initial accuracy were analysed only for subjects who completed the original test procedure.

Without correction for initial accuracy, differences within information types were statistically significant, replicating the misinformation effect frequently reported in journals. (One way analysis of variance - $\underline{F} = 5.50$, $\underline{df} = 2, 22$, $\underline{p} < .01$. ie. Consistent (Mean = 2.75; Std. dev. = 0.45) > Neutral (Mean = 2.25; Std. dev. = 0.96) > Inconsistent (Mean = 1.91; Std. dev. = 0.90). However, with correction for initial accuracy, these differences were attenuated such that differences between information types failed to reach statistical significance. ($\underline{F} = 1.09$, $\underline{df} = 2, 22$, $\underline{p} < .35$. ie. Consistent (Mean = 2.08; Std dev = 0.99) = Neutral (Mean = 1.75; Std dev = 0.86) = Inconsistent (Mean = 1.83; Std dev = 0.93). Figure 6.7 illustrates differences between the information types before and after the correction for initial accuracy.

Thus, removing scores from critical responses on the original test on which there was no substantial evidence that the critical details were encoded in memory in the first place attenuates misinformation effect findings typically made in postevent information studies.

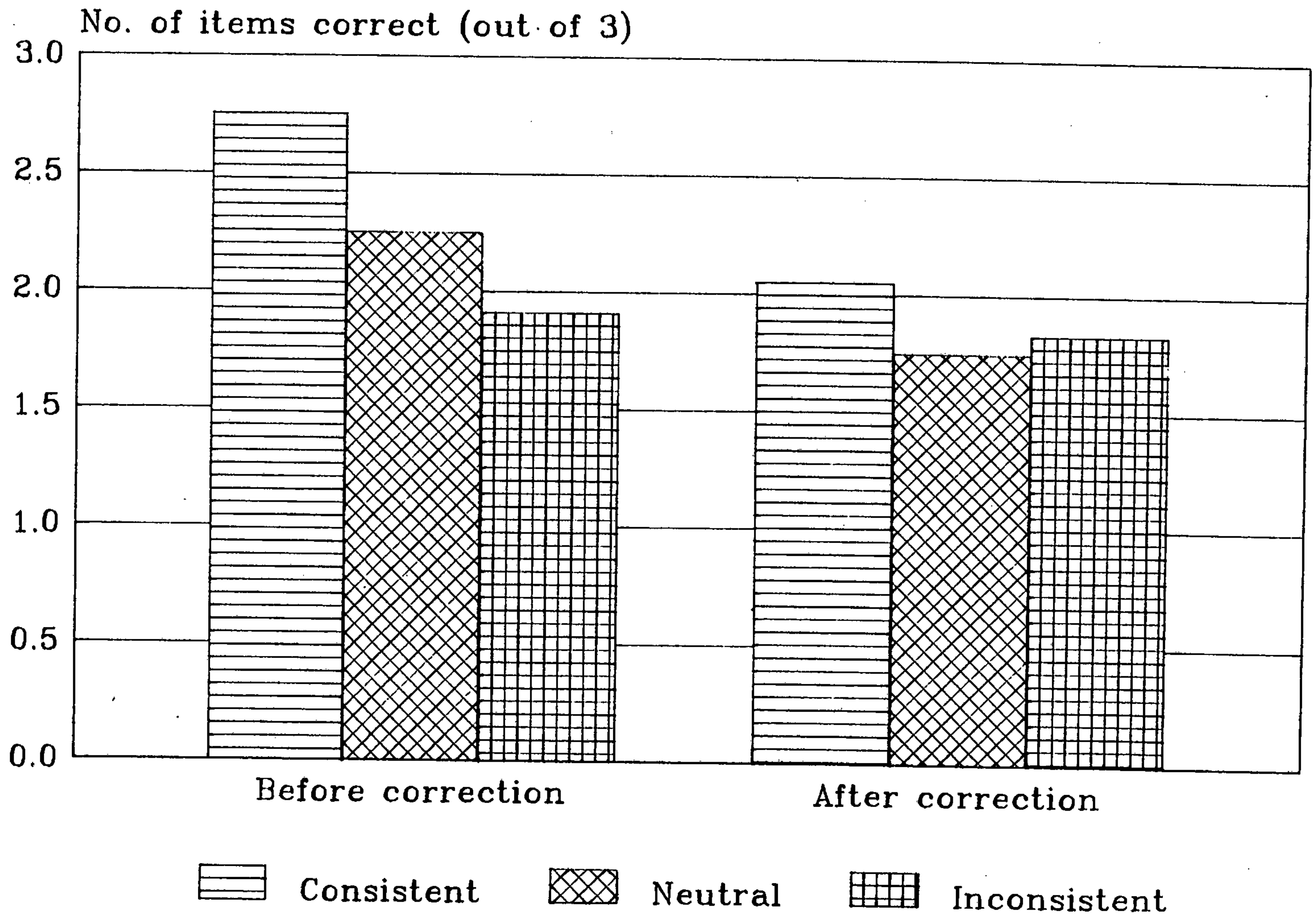
Table 6.27 Analysis of variance summary table: one way analysis of variance (repeated measures) of performance in the original test structure; scores uncorrected for initial accuracy

EFFECT	STATISTIC	F	DF	P
INFORMATION TYPE	WCP SS = 4.22 WCP MS = 2.11	5.50	2, 22	0.01
ERROR	WCP SS = 8.44 WCP MS = 0.38			

Table 6.28 Analysis of variance summary table: one way analysis of variance (repeated measures) of performance in the original test structure; scores corrected for initial accuracy

EFFECT	STATISTIC	F	DF	P
INFORMATION TYPE	WCP SS = 0.72 WCP MS = 0.36	1.09	2, 22	0.35
ERROR	WCP SS = 7.27 WCP MS = 0.33			

Fig 6.7. Performance on the Final Test corrections for initial accuracy



4.6 DISCUSSION

Taken as a whole, the results of this experiment support McCloskey & Zaragoza's contention that misinformation effects in postevent information experiments are artefactual.

In the first place, Loftus' assertion that memory is altered by misinformation is disconfirmed by (i) a replication of the attenuation of the misinformation effect using McCloskey & Zaragoza's test procedure, (ii) the lowering of performance of subjects given inconsistent information on the original and new test procedures, (iii) the raising of performance of subjects exposed to consistent information only on the original test. Likewise, the alternate explanation of misinformation effects usually advanced in postevent information studies viz. that misinformation makes original information difficult to retrieve (ie. the claim made by Headed Records Theory), is not supported by the finding that subjects given consistent information outperform subjects given neutral information on the original test. In Headed Records theory (Morton, Hammersley & Bekerian, 1985), the most popular of the 'coexistence' accounts, the tendency of subjects given inconsistent postevent information to perform at levels lower than subjects given neutral information is explained by positing that two different memory records are created for original and postevent information respectively and that the most recent of these (ie. the misleading information) is retrieved at the time of the test. Thus, a number of subjects will perform incorrectly on the test because they access the record containing the misleading information, resulting in poorer performance on the test. However, this explanation cannot account for the increased relative performance of subjects given consistent information: subjects will have memory records of both original and postevent information, but these will contain the same information (as the suggestion is consistent with the original information), and will not affect correct performance on the test.

In the second place, correcting scores for initial accuracy attenuates the misinformation effect. This supports the hypothesis that biasing effects in the postevent information paradigm are due to the fact that some subjects don't encode the original information, but do encode the postevent information, choosing incorrectly on the final test and thus creating an artefactual misinformation effect. However, as indicated earlier, correcting scores for initial accuracy may overcorrect in the sense that the free recall test used to determine initial accuracy may be too insensitive a test of memory for original information, eliminating precisely those subjects (with so called 'weak' memories) who show the misinformation effect. Nevertheless, taken as corroborative evidence of McCloskey & Zaragoza's hypothesis, rather than as independent evidence, it strengthens the position that misinformation effects are artefactual, and not due to any substantial effects on memory.

5. CONCLUSIONS.

In earlier parts of the dissertation I indicated a desire to resolve the dispute concerning the validity of results produced by postevent information experiments. I have not managed to see out this desire here.

To summarize. The first of the *a priori* analyses, which modelled the hypothesis of artefact algebraically, produced results which question the adequacy and sufficiency of the hypothesis as an explanation of typically observed experimental results. The second of the *a priori* analyses (the small scale meta-analysis) equivocally supported the claim of artefact, drawing attention to differences in effect sizes unexpected under the hypothesis of artefact. The experimental investigation of the claim of artefact, on the other hand, produced results clearly supportive of McCloskey & Zaragoza's arguments.

The results of these investigations, on the balance of things, seem to support the claim of artefact. Nevertheless, there doesn't seem to me to be much point in arguing vigorously for this position: there's enough disconfirmatory data either way, both from other studies and from this one. In the next chapter, which is the final chapter of the dissertation, I shall advance arguments that may settle the matter a little more convincingly, albeit in a somewhat different light.

CHAPTER SEVEN: FINAL CONSIDERATIONS

1. INTRODUCTION

This, the final chapter of the thesis, is traditionally set aside for summaries and conclusions. I intend to usurp this problem solving tradition here as much as possible. Instead of providing accumulations I will raise issues that went unmentioned in the earlier parts of the thesis, and in the place of the usual integration of data I will introduce a new a line of evidence.

The chapter proceeds by way of an argument, which is at the same time an attempt to unify the two lines of approach to the question of postevent information taken in the thesis. (These lines of approach concern (i) the applied nature of postevent information research, and (ii) the validity of the misinformation effect). The argument makes the claim that the methodological failure of postevent information research is due to the neglect of the applied nature of the research. From this claim I argue further that the methodological dispute is only the culmination of a process of making legal problems psychological questions, which has the unfortunate consequence in this particular case of diverting attention from an important legal problem.

The best way to begin is by restating positions adopted earlier.

2. THE ARGUMENT

The primary justification of postevent information research has been its claim of application to a legal problem. The research itself, though, has failed to live out its justification. Thus, there is little more said about the legal problem of postevent information than the possible effects false information may have on a witness' memory for an event; the choice of research methods has been restrictively experimental; and the application of results from particular studies to legal practice borders on the baneful.

The theoretical products of postevent information research are little better. Initial claims of memorial alteration have had to be restrained to the contention that misinformation may render particular memories difficult to retrieve, and it is not clear that the 'misinformation effect', which is the foundation of postevent information research, is not simply the upshot of experimental artefact.

The cognitive implications of the research are few indeed, and the applications to legal practice still fewer. Postevent information research is essentially valueless: bearing in mind the arguments developed in earlier chapters, there is no other reasonable conclusion to draw. I want to suggest in what follows that this is a peculiar and unfortunate state of affairs, for the problems that confront legal practice when faced with a witness who has been exposed to suggestive information after the witnessing of an event are not only very real, but some of the most difficult faced by a court. The methodological dispute that mars the paradigm is, in terms of this understanding of the phenomenon of postevent information, a non-issue.

To make this point I have to introduce a new line of evidence. This evidence stems from a consideration of what the problem of postevent information means to legal practice, and I shall introduce the evidence here through a brief analysis of three important cases heard by the Supreme court of South Africa, in which the problem of postevent information elicits the Court's attention.

2.1 R. v. Jackson. 1955 (4) S.A. 85 (SR.)

In this case, the defendant (a young woman) was convicted in a magistrate's court for stealing a cheque, forging the payee's signature as endorsement upon the cheque, and uttering the forged cheque to a shopkeeper. The defendant was identified at a corporeal identification parade by the shopkeeper, but not by his assistant, who had also dealt with the person who forged the cheque. Both the shopkeeper and the assistant, however, identified the defendant as the cheque forger in court. On appeal at a higher court, counsel for the defence argued that both sets of identifications had been prejudiced: the police officer investigating the case had shown both the shopkeeper and his assistant a photograph of the defendant during a pretrial attempt to establish the identity of the cheque forger. (Both the shopkeeper and his assistant positively identified the person in the photograph as the offender). The court upheld this objection, accepting the proffered argument that the identifications made may have been based on the photograph rather than on the original event: the presentation of the photograph was unduly suggestive. The court laid down an important rule on the basis of this, which has been followed by Supreme and Appellate division courts on several occasions:

> When photographs are employed to identify a suspected person, the police should use a series of photographs from which the suspected person should be picked. (At 85)

In later cases, this principle was modified so that an identification from 'photospreads' is not followed by an identification from a corporeal identification parade, as witnesses are unlikely to choose any member of the parade other than the person they chose in the photospread (they would look foolish if they did), and would thus serve little purpose (Hoffman & Zeffertt, 1983). (In fact, one case points out that such a procedure may create the false impression of the witness' unerring ability to identify the perpetrator of the crime[1]).

The problem of postevent information here is undeniably real. Cases where witnesses have the offender under observation for only a brief period of time are a well publicized danger[2] in all legal systems, and the court here acknowledges that the information a witness acquires after an event may determine subsequent identifications. The approach the court takes to the problem is exceedingly

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1. Kola v. R. 1949 (1) P.H. H100 (A)

2. A recent decision in an English court, for example, has made it impermissible to convict suspects in cases where the witness only had a 'momentary glimpse' of the offender, and where there is no other evidence tying the suspect to the crime (Shepherd et al. 1982).

simple: procedures are modified to eliminate an identifiable instance where the possibility exists that witnesses *may* be exposed to such information.

2.2 R. v. Nara Sammy 1956 (4) S.A. 629 (T)

The appellant in this case was convicted in a magistrate's court on 5 counts of theft. The conviction rested solely on identifications made by a number of witnesses at an identification parade at which the appellant was present. The appellant's counsel alleged that the conduct of the identification parade had been irregular, in that witnesses had been assembled in a room immediately prior to the parade, and had had the opportunity to communicate with each other. That the witnesses concurred in their identifications may have been due to communication between witnesses prior to the parade, and not to the identity of the suspect as the perpetrator in question.

The Court (*per* Dowling, J) accepted this reasoning. At page 630 the Judge remarks:

> ...it would be the most natural thing in the world for a group of witnesses, called for the purpose of identification, to discuss with each other any peculiar features or characteristics of the offender and assist with each other in bringing such person to justice. That something of this kind occurred in the present case is likely. One witness actually testified to a discussion about the build of the accused. ..it is impossible to say how far the unanimity [of identifications of the suspect] was based upon information circulated amongst the prospective witnesses when they were confined to the room prior to being led onto the parade.

In addition, Dowling, J. draws attention to the fact that the constable leading the witnesses into the parade room was aware of the identity of the accused, and was thus in a position to communicate this knowledge to the witnesses.

This he considers an irregularity[3]. Finally, Dowling, J. expresses misgivings about the instructions given to the witnesses:

> [Hulle] ..was versoek om die persoon uit te wys wie op 27.5.55 om ongeveer 1.30 n.m. die besigheid binne gekom het, n draadloos geneem het en weer ingestap het, dit draadloos in straat neergesit het, toe hy agtervolg was. Hy het beskuldige uitgewys na 20 sekondes. (The constable's account of instructions given to witnesses. At 631).

This form of admonition carries with it the implication that the guilty person is on the parade and the task is simply for the witness to point him out. Witnesses may consequently feel obliged to identify a member of the identification parade. On the basis of this, Dowling, J. suggests that the important words "if such person is present on the parade" be added to instructions given to witnesses. (This principle has been followed in many subsequent cases and can be considered good law[4]).

On the basis of these considerations, the conviction was set aside.

2.3 R. v. Kumalo 1948 (2) P.H. H200 (A)

In this Appellate Division case, a 14 year old African girl was accosted and raped while walking through a plantation with her 9 year old brother. She described her assailant to the police who arrested a man of that description known to them. Under cross examination from the accused, she

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3. The meaning of the term 'regular' in this context may be a bit elusive. Elsewhere in the transcript of the judge's decision, the judge suggests that a regularly conducted parade is one that is conducted in "such a manner that there can be no doubt whatsoever as to the genuineness of the identification." (Dowling, J. at 629, following R. v. Olia 1935 T.P.D. 213.)

4. See Hoffman & Zeffertt (1983).

described the conditions under which she submitted the description of her assailant:

> On this Saturday on which I was assaulted I went to the Europeans. I there described my assailant to the native employees there and then received certain information from Masitole which caused me to describe and give accused's name to the police. (At 330)

When asked how Masitole knew that the accused had assaulted her, she replied

> Masitole told me she had seen you in the vicinity on this occasion. (At 330)

The Appellate division (*per* van den Heever, JA) said the following about the description given to the police:

> It seems clear that complainant's description of her assailant given to the police was a composite picture based partly upon her own observation and partly upon information she had received from Masitole.. The constable is fortunate indeed. Severe reproof would not have been out of place, for the method adopted is calculated to endanger the liberty of innocent persons... It seems to me that the identification of the accused at this stage and in these circumstances becomes worthless... (At 331)

This passage gives an indication of the weight South African courts attach to the problem of postevent information. (Nevertheless, despite the problems with the witness' testimony the accused's appeal was overruled - cross examination showed that his *alibi* was invented, and exposed several contradictions in the account of his movements at the time of the crime. This makes the important point that the dealings of the court with cases in which postevent information is an issue is not to be understood narrowly: it requires comprehension of the entire legal machinery).

3. IMPLICATIONS OF THE CASES

These cases have a few obvious and important implications for the discussion. In the first place, they draw attention to the gravity that the court attaches to the problem of postevent information. In all of the cases the fact that identifications *may* have been influenced by information acquired by witnesses after the event is considered sufficient to set the case aside.[5] In the second place, the response of the courts to the problem in two of the cases involves making new law: in both *R.* v. *Jackson* and *R.* v. *Nara Sammy* binding changes to existing procedures are enforced, and these changes limit the *opportunities* for exposure to postevent information. The question for the courts is not whether witnesses that have been exposed to misinformation have been influenced by it, but how to find ways to eliminate the opportunities for its occurrence.

The problem of postevent information, as faced by the Court in these cases, differs vastly from the treatment afforded it by psychological research. As we have seen, psychological research has concerned itself with the examination of variables that modulate the postevent information effect: temporal intervals between presentation of misleading information and memorial reports; hypnotic trances; sensory modality of the postevent information, and so on. It has treated the phenomenon as a theoretical construct, and not as a problem confronted in practice. The legal problem has been turned into a psychological question.

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5. That the case against the accused was not dismissed in R. v. Kumalo is not to deny this assertion: other evidence settled the matter, and this, as I said previously, points to the need to take the entire legal machinery as the object of study.

The methodological dispute about the validity of experimental results in postevent information research represents the culmination of this process of turning practical problems into objects of theoretical inquiry. The issues at stake in the methodological dispute are the validity of results produced by experiments of a particular form, and (consequently) the tenability of the claim that postevent information affects memory for an event. These issues are irrelevant to the problem that courts are faced with: courts are faced with *reports* delivered by witnesses, and whether these reports show that witnesses have difficulty retrieving original information, or that memory for an event has been destroyed, or even whether postevent information has affected *memory* for the original event at all, has little bearing on the problem.

This point can be made quickly by assuming that McCloskey & Zaragoza's claim of artefact is true, and tracing the consequences for legal practice.

Assume that postevent information does not affect memory for the original event in any of the ways suggested by postevent information research. Assume further that the difference between experimental and control groups is due solely to the fact that some subjects fail to encode original information, but succeed in encoding postevent information, subsequently choosing postevent information in the recognition test. Transporting this hypothesized state of affairs to a 'true' witness scenario, some witnesses will fail to encode critical information at the time of the event they are witnesses to, but will encode additional information relating to the critical details not originally encoded. At the time of delivering a report on the event they observed, witnesses will include the postevent information in their reports. Their reports will thus show the influence of postevent information, but this need not indicate anything of importance about the nature of the memorial processes involved - as far as this is concerned, the presence of postevent information in their reports is artefactual.

From the perspective of the Court, this is irrelevant: the notion of artefact is meaningless when the scenario hypothesized under the claim of artefact is transported into legal practice. Whatever the source of the misinformation embedded in the final report, the report represents a grave danger to the proper functioning of the court.

4. WHAT IS TO BE DONE?

I argued a few pages ago that the correct conclusion to draw from the psychological literature is that existing research on postevent information is essentially valueless. This does not mean that we ought to give up on the problem of postevent information: the considerations outlined in the previous five pages should dispel any such inclinations. The problem remains very real and very pressing for legal personnel, and psychological research may well be able to do something of value in this regard. The way forward from the present quandary though, is unclear, and what is required in the first place is a bit of clear and hardheaded thinking. This is not the place to settle the question of what is to be done, but there is room for some clear thought. So, in what remains of the current chapter I will introduce an important distinction, restate a few points made earlier in the thesis, and show how they are particularly relevant here.

In an earlier chapter I hinted that a solution to current woes is dependent on a reconceptualization of the notion of applied research (in psychology), and in particular, on the reconceptualization of applied eyewitness research. A beginning in this regard is to take a distinction made by Wells (1978) seriously.

Wells (1978), in his discussion of issues that applied witness research may address, distinguishes between *estimator variables* and *system variables*. Estimator variables are variables that affect the reliability of testimony, but whose effect on the reliability of a particular witness report can only be estimated (eg. the race of the witness and the race of the offender). Research findings on estimator variables, as presented in court by expert witnesses, can only be used by the jury (or, in South Africa, the judge), to support probabilistic estimates of the accuracy of particular eyewitnesses. The role of the psychologist in legal proceedings would thus be to present the research findings to the judge and jury. Other ways of 'applying' estimator variable research to legal procedure are, of course, imaginable. In several European countries, for instance, where the 'adversarial' model of law is less rigidly adhered to, and where expert witnesses are often used extensively in pretrial hearings, psychologists serving as expert witnesses could doubtless apply the research in other ways (Clapham, 1981). As it stands in most western countries, though, the only way that estimator research can be applied is by presentation of the research to the jury or judge(s), who could use it to estimate the likelihood that a particular witnesses' testimony is accurate. Application of research in this way is quite obviously problematic. It may, for one, undermine one of the cornerstones of western legal systems: the right of the accused to stand a fair trial. 'Estimation' of the likelihood that a witness is accurate is the generalization of research findings to specific instances, whereas the legal principle of the right of the accused to stand trial grants immunity from generalization! Lawyers and judges are wary of this particular hazard of expert psychological testimony, and indeed, frequently disallow it in American trials.

These issues have recently been the focus of enormous controversy in the mouthpiece of the American Psychological Association (American Psychologist)[6] - several years after Wells' seminal discussion of the problems - and from recent American Supreme Court decisions, it appears that the door has shut firmly on this type of application (Loftus, 1983b). Wells, in the 1978 publication under consideration, argues too that it is a problematic way of applying eyewitness research, and suggests that psychologists would be better advised to address their research attention to so called 'system variables'. These variables are said to systematically affect the reliability of a witnesses' report. What Wells specifically has in mind are features of the legal and criminal system that systematically affect the accuracy of a report or an identification. These features are not the rarities they might seem to be: courts have frequently questioned the identification procedures used by police, especially corporeal and photographic lineups. In the South African case of *R vs Masemang*[7], for instance, the

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6. See McCloskey & Egeth (1983a, 1983b), Loftus (1983a, 1983b), Wells (1984), McCloskey & Egeth (1984). Although many relevant and important issues were raised in these publications, the point is made succintly enough here for the purposes of this chapter.

7. R vs Masemang 1950 (2) SA 488 (A).

complainant reported to the police that she had been raped and that her assailant had worn dark trousers and a maroon pullover. At a corporeal identification parade held afterwards the complainant identified the suspect, who was the only man in the parade wearing dark trousers and a maroon pullover. The Appellate Division allowed an appeal from the accused on the grounds that the identification had been prejudiced. In cases like this, the need for applied research is obvious. Here, unlike research on 'estimator variables', application would not involve generalizing results from experimental scenarios to specific witness cases, but would instead involve (for example) research on optimal corporeal lineup techniques. No estimate would have to be made of either the witnesses' veracity or ability: the research is simply a matter of optimizing procedures used to obtain identifications.

Wells' distinction is important because most of the recent debate about eyewitness research has revolved around the questionable practice of generalizing laboratory findings to real life scenarios quite unlike those in which the findings were originally made. In the case of system variable research this particular problem is eased somewhat.

The point that I wish to take from this long digression is that this distinction between 'estimator' and 'system' variables must be a new starting point for research on the problem of postevent information. Instead of determining *whether* postevent information affects a witness' subsequent memorial report, and whether temporal intervals attenuate the transmission of the effect, we should aim at identifying particular legal situations in which postevent information is a problem and in which our research attempts may have a salutary effect. We should concentrate on procedural aspects of the legal process that may be responsible for the introduction of postevent information into the course of events leading up to, and including, the trial.

Research on identification parades and the practice of showing 'mugshots' to witnesses seems especially promising. Here the question is not to generalize findings to all possible eyewitnesses, but simply to optimize procedures used by the courts and police to secure personal identification. The 'population' to generalize to here is thus the practice of securing an identification, not the act of witnessing an event.

The first proposition I wish to make then, is that we take the distinction between estimator and system variables seriously. The trick is to tackle something about which something can be done.

The second proposition is that we keep in mind the observation introduced in chapter 2; namely that 'application' requires an infrastructure, and that applied research is no exception. The study of postevent information is best pursued in conjunction with legal personnel, that is with those who possess the jurisdiction to enact legal application; and more than this, research on postevent information requires detailed consideration of legal issues, beginning with a full analysis of the problem of postevent information in material terms.

These seem to me to be two sets of considerations that we would do well to heed, and that could be usefully deployed in the requisite redefinition of the problem of postevent information.

I am reluctant to insist that this is the way forward for the psychological study of postevent information, bearing in mind the fate of a similar admonition by Neisser, but if something is to be done then this may be a useful starting point.

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APPENDIX 1. NARRATIVE PRESENTED TO SUBJECTS (ORIGINAL EVENT INFORMATION). FROM BARTLETT (1932)

The following story is a loose adaptation of a North American folktale. Please read it carefully and then turn the page.

THE WAR OF THE GHOSTS

One night two young men from Egulac went down to the river to hunt seals, and while they were it became foggy and calm. Then they heard warcries, and they thought: "Maybe this is a war party." They escaped to the shore, and hid behind a log. Now canoes came up, and they heard the noise of paddles, and saw one canoe coming up to them. There were five men in the canoe, and they said:

"What do you think? We wish to take you along. We are going up the river to make war on the people."

One of the young men said: "I have no arrows."

"Arrows are in the canoe," they said.

"I will not go along. I might be killed. My relatives do not know where I have gone. But you," he said, turning to the other, "may go with them."

So one of the young men went, but the other returned home.

And the warriors went on up the river to a town on the other side of Kalama. The people came down to the water, and they began to fight, and many were killed. But presently the young man heard one of the warriors say:

"Quick, let us go home: that Indian has been hit." Now he thought: "Oh, they are ghosts." He did not feel sick, but they said he had been shot.

So the canoes went back to Egulac, and the young man went ashore to his house, and made a fire. And he told everybody and said: "Behold I accompanied the ghosts, and we went to fight. Many of our fellows were killed, and many of those who attacked us were killed. They said I was hit, and I did not feel sick."

He told it all, and then he became quiet. When the sun rose he fell down. Something black came out of his mouth. His face became contorted. The people jumped up and cried.

He was dead.

APPENDIX 2. QUESTIONNAIRES CONTAINING POSTEVENT INFORMATION.

(Inconsistent, Neutral, and Consistent versions of the critical questions are indicated by listing alternatives to sentences).

THINK BACK TO THE STORY "THE WAR OF THE GHOSTS". THE FOLLOWING QUESTIONNAIRE IS A TEST OF YOUR MEMORY FOR THE STORY. PLEASE COMPLETE IT AS ACCURATELY AS YOU CAN.

1. What was the first young man's excuse for not going down the river in the canoe?

2. What had the two young men heard when they escaped to the river shore and hid there behind a log? (rock, (no mention), log).

3. How many young men, who went down to the river to hunt otters, were there? (otters, (no mention), seals)

4. What are the 'ghosts' in the story?

5. What came out of the wounded young man's mouth when he fell down and died? (sunset, (no mention), sunrise)

6. What alerted the men to the fact that there were canoes coming up the river?

7. What happened when the canoe had got to the town on the other side of Kalama? (Kalgari, (no mention), Kalama)

8. Why did the warriors suddenly decide to go home when they were in mid - battle?

9. After the young man who had gone up the river had returned home to his tent, what did he tell everybody? (tent, home, house)

10. What was the name of the town that the two young men were from?

11. What did the young man (who declined to join the warparty) say when the men in the canoe told him that there were weapons in the canoe? (guns, weapons, arrows)

12. Try to recall exactly what the men in the canoe said to the two young men when they first spoke.

13. What did the 5 men in the canoe say to the two young men? (3 men, the men, 5 men)

14. Where did the battle in the story take place?

15. After one of the young men had agreed to accompany the warriors in the canoe, and the other had returned hunting, where did the canoe go? (returned hunting, (no mention), returned home)

16. Was it day or night when the two young men went to the river? (where it was stormy, (no mention), where it was foggy and calm)

17. What did the young man who had been wounded in battle do when he came home?

18. What was the colour of the substance that came out of the young man's mouth when he died?

APPENDIX 3.: INITIAL ACCURACY TEST.

THE FOLLOWING TWELVE QUESTIONS TEST YOUR RECALL OF SOME OF THE DETAILS OF THE STORY YOU HAVE JUST READ. PLEASE TRY TO ANSWER THEM AS ACCURATELY AS POSSIBLE. YOU MAY NOT LOOK BACK AT THE STORY.

1. What did the two young men hide behind on the river shore in the begginning of the story?

2. What did the young men go down to the river to hunt?

3. How did the young men know that there were canoes coming up the river?

4. What was the name of the town the canoe passed on the way up the river?

5. What kind of weapon did the warriors have in the canoe with them?

6

6. What type of dwelling did the young man who was wounded in battle live in?

7. Where (ie. which town) were the two young men from?

8. How many warriors were there in the canoe when it stopped to ask the two young men whether they wanted to join the war party?

9. One of the young men accompanied the warriors up the river. What did the other young man do?

10. What did the warriors give as their reason for leaving the battle and returning down the river?

11. What was the weather like when the two young men went down to the riverside right at the begginning of the story?

7

12. When (ie. what time of day) did the young man, who had been wounded, die?

APPENDIX 4. RECOGNITION TEST: ORIGINAL STRUCTURE

THIS IS ANOTHER TEST OF YOUR MEMORY FOR THE STORY "THE WAR OF THE GHOSTS". PLEASE COMPLETE IT BY CIRCLING THE CORRECT OPTION BELOW EACH QUESTION. IF YOU DON'T REMEMBER THE CORRECT ANSWER THEN CIRCLE THE OPTION THAT SAYS 'CAN'T REMEMBER'. DO THIS AS ACCURATELY AS YOU CAN. YOU MAY NOT LOOK BACK AT ANY OF THE ASSIGNMENTS YOU HAVE ALREADY COMPLETED.

1. What did the young men go down to the river to hunt?

   a) seals   b) otters   c) can't remember

2. What time of day was it when the two young men went down to the river?

   a) night   b) day   c) can't remember

3. What alerted the young men to the fact that a warparty was approaching down the river?

   a) warcries   b) the sound of paddles cutting the water   c) can't remember

4. What did the two young men hide behind when they heard the canoes approaching?

   a) a rock   b) a log   c) can't remember

5. How many warriors where there in the canoe?

   a) 5   b) 3   c) can't remember

6. How many canoes came up the river towards the two young men?

   a) one   b) more than one   c) can't remember

7. Where did the warriors say they were going to make war on the people?

a) up the river b) down the river c) can't remember

8. When did the young man who had been wounded fall down and die?

a) sunrise b) sunset c) can't remember

9. What was the name of the town the warparty passed on the way up the river?

a) Kalgari b) Kalami c) can't remember

10. How many of the two young men in the begginning of the story went on to battle?

a) one b) two c) can't remember

11. What were the warriors carrying in the canoe when they met the two young men?

a) guns b) arrows c) can't remember

12. One of the young men went to battle with the warriors. What did the other young man do?

a) returned home b) carried on hunting c) can't remember

13. Where was the battle itself?

a) up on a plateau b) down at the riverside
c) can't remember

14. What was the weather like at the beginning of the story when the two young men went down to the river?

a) stormy b) foggy and calm c) can't r
emember

15. How many people were killed in the battle?
a) many on the young man's side
b) many on both sides c) can't remember

16. What did the people do when the young man fell over and died?

a) jumped up and cried b) did nothing c
) can't remember

17. What did the young man who had been wounded live in in his hometown, Egulac?

a) a tent b) a house c) can't remember

18. Was the young man who died actually wounded or not?

a) yes b) no c) can't remember

APPENDIX 5. RECOGNITION TEST: MODIFIED STRUCTURE

THIS IS ANOTHER TEST OF YOUR MEMORY FOR THE STORY "THE WAR OF THE GHOSTS". PLEASE COMPLETE IT BY CIRCLING THE CORRECT OPTION BELOW EACH QUESTION. IF YOU DON'T REMEMBER THE CORRECT ANSWER THEN CIRCLE THE OPTION THAT SAYS 'CAN'T REMEMBER'. DO THIS AS ACCURATELY AS YOU CAN. YOU MAY NOT LOOK BACK AT ANY OF THE ASSIGNMENTS YOU HAVE ALREADY COMPLETED.

1. What did the young men go down to the river to hunt?

a) seals b) fish c) can't remember

2. What time of day was it when the two young men went down to the river?

a) night b) day c) can't remember

3. What alerted the young men to the fact that a warparty was approaching down the river?

a) warcries b) the sound of paddles cutting the water c) can't remember

4. What did the two young men hide behind when they heard the canoes approaching?

a) a tree b) a log c) can't remember

5. How many warriors where there in the canoe?

a) 5 b) 8 c) can't remember

6. How many canoes came up the river towards the two young men?

a) one b) more than one c) can't remember

7. Where did the warriors say they were going to make war on the people?

a) up the river b) down the river c) can't remember

8. When did the young man who had been wounded fall down and die?

a) sunrise b) midnight c) can't remember

9. What was the name of the town the warparty passed on the way up the river?

a) Bangali b) Kalami c) can't remember

10. How many of the two young men in the beggining of the story went on to battle?

a) one b) two c) can't remember

11. What were the warriors carrying in the canoe when they met the two young men?

a) spears b) arrows c) can't remember

12. One of the young men went to battle with the warriors. What did the other young man do?

a) returned home b) went up to the mountain c
) can't remember

13. Where was the battle itself?

a) up on a plateau b) down at the riverside c
) can't remember

14. What was the weather like at the beginning of the story when the two young men went down to the river?

a) sunny b) foggy and calm c) can't r
emember

15. How many people were killed in the battle?

a) many on the young man's side b) many on both s
ides c) can't remember

16. What did the people do when the young man fell over and died?

a) jumped up and cried b) did nothing c
) can't remember

17. What did the young man who had been wounded live in in his hometown, Egulac?

a) a cabin b) a house c) can't remember

18. Was the young man who died actually wounded or not?

a) yes b) no c) can't remember

APPENDIX 6. RECOGNITION TEST: NEW STRUCTURE

THIS IS ANOTHER TEST OF YOUR MEMORY FOR THE STORY "THE WAR OF THE GHOSTS". PLEASE COMPLETE IT BY CIRCLING THE CORRECT OPTION BELOW EACH QUESTION. IF YOU DON'T REMEMBER THE CORRECT ANSWER THEN CIRCLE THE OPTION THAT SAYS 'CAN'T REMEMBER'. DO THIS AS ACCURATELY AS YOU CAN. YOU MAY NOT LOOK BACK AT ANY OF THE ASSIGNMENTS YOU HAVE ALREADY COMPLETED.

1. What did the young men go down to the river to hunt?

   a) fish b) otters c) can't remember
d) neither of a) or b)

2. What time of day was it when the two young men went down to the river?

   a) night b) day c) can't remember
d) neither of a) or b)

3. What alerted the young men to the fact that a warparty was approaching down the river?

   a) warcries b) the sound of paddles cutting the water c) can't remember d) neither of a) or b)

4. What did the two young men hide behind when they heard the canoes approaching?

   a) a rock b) a tree c) can't remember
d) neither of a) or b)

5. How many warriors where there in the canoe?

   a) 8 b) 3 c) can't remember
d) neither of a) or b)

6. How many canoes came up the river towards the two young men?

a) one b) more than one c) can't remember
d) neither of a) or b)

7. Where did the warriors say they were going to make war on the people?

a) up the river b) down the river c) can't remember d) neither of a) or b)

8. When did the young man who had been wounded fall down and die?

a) midnight b) sunset c) can't remember
d) neither of a) or b)

9. What was the name of the town the warparty passed on the way up the river?

a) Kalgari b) Bangali c) can't remember d) neither of a) or b)

10. How many of the two young men in the beggining of the story went on to battle?

a) one b) two c) can't remember
d) neither of a) or b)

11. What were the warriors carrying in the canoe when they met the two young men?

a) guns b) spears c) can't remember
d) neither of a) or b)

12. One of the young men went to battle with the warriors. What did the other young man do?

a) went up to the mountain b) carried on hunting
c) can't remember d) neither of a) or b)

13. Where was the battle itself?

a) up on a plateau b) down at the riverside
c) can't remember d) neither of a) or b)

14. What was the weather like at the beginning of the story when the two young men went down to the river?

a) stormy b) sunny c) can't remember d) neither of a) or b)

15. How many people were killed in the battle?

a) many on the young man's side b) many on both sides c) can't remember d) neither of a) or b)

16. What did the people do when the young man fell over and died?

a) jumped up and cried b) did nothing c) can't remember d) neither of a) or b)

17. What did the young man who had been wounded live in in his hometown, Egulac?

a) a tent b) a cabin c) can't remember d) neither of a) or b)

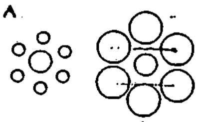
18. Was the young man who died actually wounded or not?

a) yes b) no c) can't remember d) neither of a) or b)

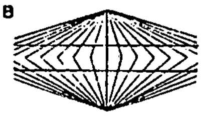
APPENDIX 7: FILLER TASKS USED IN THE EXPERIMENT

Each of the diagrams below is a visual illusion - something is 'wrong' with the diagram in each case. Please study them carefully and indicate what is wrong with each diagram next to the letter corresponding to the diagram at the bottom of the page.

A



B



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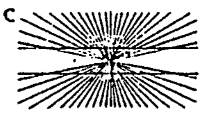

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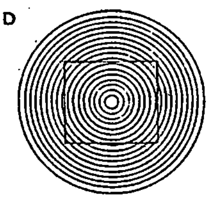

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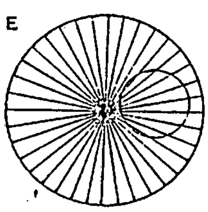

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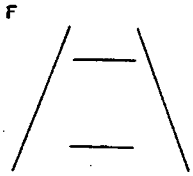

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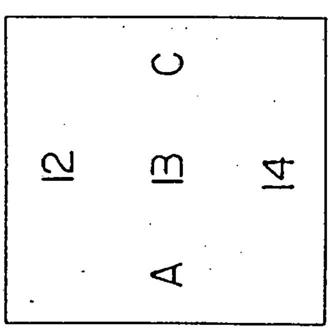

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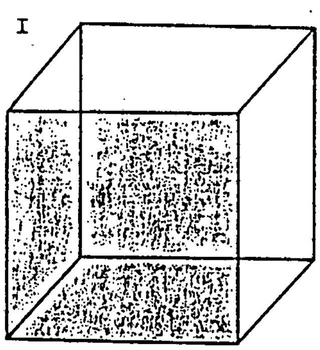
I



PLEASE TURN OVER

Now read the following paragraph and answer the questions that follow.

Pavlov's experiments

A dog is prepared for Pavlov's experiment by having a minor operation performed on its cheek so that part of the salivary gland is exposed to the surface. A capsule is attached to the cheek to measure salivary flow. The dog is brought to a soundproof laboratory on several occasions and is placed in a harness on a table. The preliminary training is needed so the animal will stand quietly in the harness once the actual experiment begins. The laboratory is so arranged that meat powder can be delivered to a pan in front of the dog by remote control. Salivation is recorded automatically. The experimenter can view the animal through a one-way glass panel, but the dog is alone is the laboratory, isolated from extraneous sights and noises (see Figure 7-1).

A light is turned on. The dog may move a bit, but it does not salivate. After a few seconds, meat powder is delivered; the dog is hungry and eats. The recording device registers copious salivation. The procedure is repeated a number of times. Then the experimenter turns on the light but does not deliver any meat powder. The dog salivates nonetheless. It has learned to associate the light with food.

Pavlov called this a *conditioned response* (CR). The dog has been taught, or conditioned, to associate the light with food and to respond to it by salivating. Naturally, a dog salivates when it tastes meat. This is an *unconditioned response* (UR); no learning is involved. By the same token, meat is an *unconditioned stimulus* (US). It automatically makes the dog salivate. Again, no learning is involved. Ordinarily a light would not produce this response, however. Only when the dog has learned that the light signals food does it salivate to the light. Hence, the light is a *conditioned stimulus* (CS) that acquires its power to elicit salivation through association. Pavlov's experiment is diagramed in Figure 7-2.

PLEASE READ THE FOLLOWING STORY AND COMPLETE THE ASSIGNMENT OVER THE PAGE.

The cellarer was a stout man, vulgar in appearance but jolly, white-haired but still strong, small but quick. He led us to our cells in the pilgrims' hospice. Or, rather, he led us to the cell assigned to my master, promising me that by the next day he would have cleared one for me also, since, though a novice, I was their guest and therefore to be treated with all honor. For that night I could sleep in a long and wide niche in the wall of the cell, in which he had had some nice fresh straw prepared.

Then the monks brought us wine, cheese, olives, bread, and excellent raisins, and left us to our refreshment. We ate and drank heartily. My master did not share the austere habits of the Benedictines and did not like to eat in silence. For that matter, he spoke always of things so good and wise that it was as if a monk were reading to us the lives of the saints.

That day I could not refrain from questioning him further about the matter of the horse.

"All the same," I said, "when you read the prints in the snow and the evidence of the branches, you did not yet know Brunellus. In a certain sense those prints spoke of all horses, or at least all horses of that breed. Mustn't we say, then, that the book of nature speaks to us only of essences, as many distinguished theologians teach?"

"Not entirely, dear Adso," my master replied. "True, that kind of print expressed to me, if you like, the idea of 'horse,' the verbum mentis, and would have expressed the same to me wherever I might have found it. But the print in that place and at that hour of the day told me that at least one of all possible horses had passed that way. So I found myself halfway between the perception of the concept 'horse' and the knowledge of an individual horse. And in any case, what I knew of the universal horse had been given me by those traces, which were singular. I could say I was caught at that moment between the singularity of the traces and my ignorance, which assumed the quite diaphanous form of a universal idea. If you see something from a distance, and you do not understand what it is, you will be content with defining it as a body of some dimension. When you come closer, you will then define it as an animal, even if you do not yet know whether it is a horse or an ass. And finally, when it is still closer, you will be able to say it is a horse even if you do not yet know whether it is Brunellus or Niger. And only when you are at the proper distance will you see that it is Brunellus (or, rather, that horse and not another, however you decide to call it). And that will be full knowledge, the learning of the singular. So an hour ago I could expect all horses, but not because of the vastness of my intellect, but because of the paucity of my deduction. And my intellect's hunger was sated only when I saw the single horse that the monks were leading by the halter. Only then did I truly know that my previous reasoning had brought me close to the truth. And so the ideas, which I was using earlier to imagine a horse I had not yet seen, were pure signs, as the hoofprints in the snow were signs of the idea of 'horse'; and signs and the signs of signs are used only when we are lacking things."

On other occasions I had heard him speak with great skepticism about universal ideas and with great respect about individual things; and afterward, too, I thought this tendency came to him from his being both a Briton and a Franciscan. But that day he did not have the strength to face theological disputes, so I curled up in the space allotted me, wrapped myself in a blanket, and fell sound asleep.

Anyone coming in could have mistaken me for a bundle. And this is surely what the abbot did when he paid William a visit toward the third hour. So it was that I could listen, unnoticed, to their first conversation.

APPENDIX 8. RAW DATA FOR EXPERIMENT REPORTED IN CHAPTER 6

Counter-balancing design	Type of recognition test assigned to	Consistent Score*	Neutral Score*	Inconsistent score*
1	1	3	3	2
1	1	2	3	1
1	1	2	2	2
1	1	2	3	0
1	1	2	2	1
1	1	3	2	1
1	1	3	2	1
1	1	3	3	3
1	1	3	3	3
1	1	3	2	1
1	1	3	3	3
1	2	3	2	2
1	2	3	2	2
1	2	2	2	2
1	2	3	1	2
1	2	3	3	2
1	2	2	2	1
1	2	3	3	2
1	2	2	2	2
1	3	1	3	1
1	3	1	3	1
1	3	2	3	2
1	3	3	1	3
1	3	1	1	1
1	3	3	3	2
1	3	2	1	2
2	1	2	2	2
2	1	2	2	2
2	1	3	2	1
2	1	3	2	2
2	1	2	2	0
2	1	3	1	1
2	1	3	2	2
2	1	3	3	0
2	1	3	3	2
2	1	3	2	1
2	1	3	1	2
2	1	3	3	2
2	1	3	2	2
2	2	3	2	2
2	2	3	2	2

2	2	0	1	2
2	2	2	2	3
2	2	2	2	3
2	2	1	1	3
2	2	3	2	3
2	2	2	1	3
2	2	3	2	3
2	2	2	3	3
2	3	3	2	0
2	3	1	2	1
2	3	2	1	2
2	3	3	1	0
2	3	2	1	2
2	3	1	3	2
3	1	2	2	2
3	1	2	2	2
3	1	2	1	3
3	1	2	0	1
3	1	2	2	3
3	1	1	1	0
3	1	2	3	3
3	1	3	1	1
3	1	3	3	3
3	2	2	3	2
3	2	3	3	1
3	2	1	2	2
3	2	2	1	1
3	2	2	2	3
3	2	2	3	3
3	2	2	3	3
3	2	2	2	2
3	2	2	2	3
3	3	2	2	2
3	3	2	2	2
3	3	3	3	2
3	3	3	3	2
3	3	1	1	3

* These scores represent performance on the final recognition test: scores are out of 3.

APPENDIX [illegible]: DATA FROM STUDIES IDENTIFIED FOR META-ANALYSIS 3

AUTHORS	PROPORTION [control group]	PROPORTION [exp. group]	EFFECT SIZE	SAMPLE SIZE
Bekerian & Bowers (1983)	0.9400	0.6000	1.3000	18.0000
Bekerian & Bowers (1983)	0.8500	0.8700	-0.0900	28.0000
Bekerian & Bowers (1984)	0.9300	0.8600	0.3900	14.0000
Bekerian & Bowers (1984)	0.8000	0.4700	0.9300	15.0000
Bekerian & Bowers (1984)	0.8300	0.6000	0.7000	24.5000
Bekerian & Bowers (1984)	0.8700	0.5800	0.9200	13.5000
Christiaansen et al. (1983)	0.9500	0.9500	0.0000	17.0000
Christiaansen et al. (1983)	0.3600	0.8500	-1.4000	21.5000
Christiaansen et al. (1983)	0.3600	0.4300	-0.1800	20.0000
Christiaansen et al. (1983)	0.3600	0.4900	-0.3300	21.5000
Christiaansen et al. (1983)	0.9500	0.4100	1.8700	15.5000
Christiaansen et al. (1983)	0.9500	0.6200	1.3300	14.0000
Christiaansen et al. (1983)	0.3600	0.2100	0.4500	21.5000
Johnson (1979)	0.2000	0.1800	0.0800	40.0000
Johnson (1979)	0.2800	0.1800	0.3300	40.0000
Loftus et al. (1978)	0.7080	0.5530	0.4000	40.0000
Loftus et al. (1978)	0.6300	0.4300	0.5200	30.0000
Loftus et al. (1978)	0.6300	0.7000	-0.1900	30.0000
Loftus et al. (1978)	0.7500	0.4100	0.9100	97.5000
Loftus & Greene (1980)	0.9500	0.6600	1.2200	100.0000
Loftus & Zanni (1975)	0.6900	0.5600	0.4800	30.0000
Loftus & Zanni (1975)	0.7200	0.5500	0.5800	50.0000
Loftus (1975)	0.1730	0.0270	1.1000	75.0000
Loftus (1975)	0.5400	0.2600	0.7600	50.0000
Loftus (1975)	0.3500	0.5300	-0.4700	75.0000
Loftus (1975)	0.1800	0.0200	1.1400	50.0000
Loftus (1975)	0.2600	0.0600	0.9100	50.0000
Loftus (1975)	0.2600	0.0800	0.7600	50.0000
Loftus (1975)	0.2200	0.0000	0.7800	50.0000
Loftus (1977)	0.7400	0.4800	0.6580	50.0000
Loftus (1981)	0.9600	0.6100	1.4800	50.0000
Loftus (1981)	0.9600	0.7400	1.1000	50.0000
McCloskey & Zaragoza (1985b)	0.7000	0.6800	0.0600	36.0000
McCloskey & Zaragoza (1985b)	0.7100	0.6600	0.1500	30.0000
McCloskey & Zaragoza (1985b)	0.7400	0.8100	-0.2300	36.0000
McCloskey & Zaragoza (1985b)	0.7100	0.7700	-0.1800	42.0000
McCloskey & Zaragoza (1985b)	0.7700	0.7300	0.1200	36.0000
McCloskey & Zaragoza (1985b)	0.7100	0.7700	-0.1800	42.0000
McCloskey & Zaragoza (1985)	0.7500	0.4200	0.8900	36.0000
McCloskey & Zaragoza (1985)	0.7500	0.3600	1.0400	18.0000

APPENDIX 9 : DATA FROM STUDIES IDENTIFIED FOR META-ANALYSIS

McCloskey & Zaragoza (1985)	0.6700	0.4500	0.5700	30.0000
McCloskey & Zaragoza (1985)	0.7000	0.3000	1.0500	24.0000
McCloskey & Zaragoza (1985)	0.7200	0.4000	0.8500	42.0000
McCloskey & Zaragoza (1985)	0.7000	0.3500	0.9200	24.0000
Miller & Loftus (1976)	0.5200	0.3100	0.5500	26.0000
Miller & Loftus (1976)	0.8000	0.7200	0.2700	50.0000
Schiffman & Davis (1985)	0.9000	0.6900	0.7900	14.0000
Sheehan & Tilden (1983)	0.7500	0.4100	0.7100	48.0000
Sheehan & Tilden (1983)	0.4500	0.3500	0.2600	48.0000
Sheehan & Tilden (1983)	0.6800	0.5000	0.4700	48.0000
Sheehan & Tilden (1984)	0.9000	0.4500	1.4200	20.0000
Sheehan & Tilden (1984)	0.1100	0.0400	0.5300	19.0000
Sheehan & Tilden (1984)	0.7000	0.2500	1.2100	0.0000
Sheehan & Tilden (1984)	0.7000	0.1400	1.6100	19.0000
Sheehan & Tilden (1984)	0.0000	0.0500	0.0100	20.0000
Sheehan & Tilden (1984)	0.8800	0.7100	0.6200	19.0000
Ward & Loftus (1985)	0.8700	0.6400	0.7700	66.0000