Applied Psychology: Application of psychological knowledge, or nominalist error?

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Introduction

In this paper I focus on the drive to application that characterizes many branches of psychology which were formerly content to ruminate in the more remote pastures of academic life. This, I think is apposite, since the theme of the conference embraces the relation between academic disciplines and a larger society. In particular I want to make an argument that warns against pursuing real life issues when this is mere justificatory rhetoric. It is certainly not always this way with Applied Psychology, but it often is.

I try to make the argument clear by taking the example of a recent drive to application in cognitive psychology, and by showing that this has generated a great deal of reseach that has had very little application.

'Crises' and the drive to application

As long as most psychologists can remember, psychology has been in a state of crisis. Each generation re-asserts the

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existence of the crisis - usually as if the crisis had just been discovered - and re-defines its nature, with an eye to justifying its own solution to this intractable state. There is remarkable agreement about the existence of the crisis, especially since psychologists disagree about so many other things.

One statement of the crisis, which we associate with Tajfel, Israel, Gergen and others, from the late '60's, objects to the narrow conceptualization of both method and theory that mainstream social psychology exhibits. Laboratory experiments drive the discipline, and these are conducted as if in a vacuum, hence the title of Tajfel's (1972) paper 'experiments in a vacuum.' This cannot be good for the discipline; the arguments are sufficiently well known to pass them over here. Of course, more radical statements of the crisis can be found - of almost any description. Seve's 1976 Marxist statement of the crisis is an example, as is the recent work by the Loughborough group. The title of Ian Parker's recent book (1989) is indication enough of the prevalence of the view that the discipline is permanently besieged: *The crisis in modern social psychology and how to end it.*

I don't want to labour our progress here with an examination of the many formulations of the alleged crisis. I will look at one in detail. This is the view that psychology fails to concern itself with real problems, indeed with everyday life. There are many variants of this view; I would like to present the position adopted by the American cognitive psychologist Ulric Neisser, because

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his statement of crisis led directly - and indirectly - to a flourish of research that sought to concern itself with everyday life.

Neisser's position

Almost two decades ago, Ulric Neisser (1976), a leading cognitive psychologist, and author of a seminal text that spearheaded the 'cognitive revolution' (Neisser, 1967), seriously questioned the validity of cognitive research. He claimed that cognitive psychology lacked 'ecological validity', that it failed to secure relevance outside the laboratories in which it was constructed. Neisser recommended a radical change in cognitive psychology's orientation if it wanted to endure as a psychological discipline - he predicted a rapid demise if it failed to apply itself outside its laboratories.

> ... [Psychology must have] 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) pursued this argument with an edited collection of memory research papers that addressed problems in ways that were clearly more applicable to conditions outside 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.

Neisser dismissed the 'laboratory' approach to the study of memory on the grounds that in the hundred years of its existence it had 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 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.[1]

> 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 - our questions

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[1] This is not mere sarcasm on Neisser's part. Several studies have shown convincingly that children of pre-school age know most of the important experimental findings in traditional psychological laboratory research. (Kreutzer, Leonard and Flavell 1975).

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. 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. Here the point is stressed that research can very usefully be redirected for obvious and important practical gain.

The message is clear in *Memory Observed*: cognitive psychology lacks ecological validity and one of the ways of doing 'ecologically valid' cognitive research is to do research outside laboratories. It is to do what is commonly called 'Applied Psychology.'

Problems lurking beneath the surface notion of 'Applied Psychology'

I have chosen Neisser's formulation as an exemplification, really. There are many variants of the type of position he adopts, and I think that the argument I develop here applies generally.

The first thing I want to note is that there are many problematic issues that underlie the notion of Applied

Psychology, and indeed the prospect of 'applying 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*. (As a matter of some local interest, the first chair of Applied Psychology was established in South Africa at Stellenbosch University in 1925, and the first incumbent was one Hendrik Frensch Verwoerd). 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.

However, there is little conceptual clarity indeed about what it is that makes Applied Psychology a subdiscipline, or indeed what entitles it to the claim that it is 'applied' psychology. The enterprise depends on a shared, but deeply flawed, two stage model of scientific activity. This is the familiar distinction between 'basic' and 'applied' science. The task of basic science is the painstaking construction of universal theory, and applied science sees to it that this universal theory is applied outside the laboratory. The conceptualization specifically indicates a

direction of information flow, namely from pure to applied. One finds this view propagated much unchanged in countless discussions of applied psychology; Danziger (1990) traces the rise of its popularity in the late nineteenth and early twentieth centuries.

In a provocative paper, Jonathan 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. 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 science which is applied. For example, research on the US. 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, in Potter, 1982: p 23)

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: this is the sense in which watermills (among other technological innovations) existed before, and were prerequisites for Newtonian mechanics.

That knowledge "flows" from the pure pole of the pure - applied dimension to the applied pole is a questionable thesis. There are cases to be made for connections between basic and applied science in particular instances, but the suggested dependency is probably false.[2]

The matter does not end here. There is a very useful and very relevant distinction between *applied* psychology and *applicable* psychology, made pertinently by Belbin (1979)

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[2] There is certainly room here for empirical examination of so-called 'applied research', and specifically, for the evaluation of the suggested dependency. On the face of it, the evidence suggests that there is no such dependency, but systematic empirical examination may tell a different story.

in an address to the British Psychological Society. The point is that most of what we 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 'applied research', much of what happens is that academics simply pluck problems from the outside world as justification for their work. The research is called 'applied research' because it addresses a social problem.[3]

The 'applied' in applied psychology, is read, 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. This is a type of nominalist error: 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

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[3] An anonymous reviewer has pointed out the need for empirical support of this proposition. I accept this criticism, and direct interested readers to a paper by Potter (1982), and a recent book by Edwards (1990), where the matter is discussed apropos of specific instances of research.

often constitutes that which goes by the name of applied psychological research.

The notion of an 'applied psychology' is a very problematic one indeed, then. Neisser's recommendation that memory research apply itself to practical problems 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. What is there that is more 'ecologically valid' about research that exhibits a superfluous concern with social problems than research that utilizes traditional methods of laboratory experimentation?

I want to draw the preceding discussion to a single conclusion, before I discuss the effect of Neisser's statement of crisis on cognitive and social psychology. The conclusion is very simple: a concern with making research more applicable to a society's problems may simply serve as a form of justificatory rhetoric. In order to ensure its survival, a discipline studies social problems. In this way it acquires research funding, it appears to be socially useful, it attracts students, it survives. Of course, this is not the only reason driving research on social problems! The point I wish to make is that disciplines can be easily tempted into this research, without a clear understanding

of the issues that it raises in respect of application of the research.

The uptake of Neisser: the case of eyewitness psychology

The conclusion immediately above refers to a real danger, and not to mere possibility. As substantiation, I would like to consider the case of a tradition of cognitive research, namely eyewitness psychology, or the psychology of eyewitness testimony. That I choose this tradition is no accident. Neisser specifically singled it out for praise in his 1976 book: he considered it an area of cognitive psychology that concerned itself with a real problem. It is also an historically interesting tradition of research as far as Applied Psychology is concerned, since the earliest Applied Psychologists took it as one of the obvious areas for an applied psychological science. Hugo Münsterberg, often called the father of Applied Psychology, wrote a book and several papers on the subject in the first decade of the twentieth century (Münsterberg, 1908). Stern (1910), Whipple (1912) and many others, in continental Europe, in England, and the U.S.A., did likewise.

This early research petered out in the second and third decades of the century, and there was very little recently published work at the time of Neisser's statement of the crisis. After Neisser's message, the area boomed. Between 1979 and 1986 seven books on the subject were published (see, for examples, Loftus, 1979; Yarmey 1979; Wells and Loftus, 1984; Lloyd-Bostock and Clifford,

1983), and by 1993 this number had risen to close to twenty. Over 1000 articles appeared between the years 1977 and 1994. There are presumably many reasons for this explosion of research, but I think Neisser's message to 'go forth into the world' had resonated loudly down the corridors of academic psychology, and explains some of it.

Two routes of enquiry

Eyewitness testimony is, in the first place, a persistent and well known legal problem. Witnesses who observe events and who are later called to testify on what they observed are frequently incorrect in their testimony, despite their best intention to be as accurate as possible. Several legal commentators have assembled evidence to show that hundreds of mistaken convictions based on eyewitness reports have sent innocent people to gaol, and indeed, to worse (Borchard, 1965; Loftus, 1979).

As far as psychological work on eyewitness testimony is concerned, there are two types of studies, to use a taxonomy suggested by Wells (1978).

A. Estimator variables

In the first type of study, researchers concern themselves with *estimator variables.* These are attributes of the witness or the event, that cannot be controlled. The physical lighting of the crime scenario, the length of time the witness observed the perpetrator, the gender of the witness are examples. These variables may all affect the

accuracy of the testimony, but since they cannot be controlled, one can only estimate the effect that the variables had on the accuracy of the witness' report.

Such an estimation is of course an extremely imprecise matter, and from the beginning commentators on eyewitness research have questioned the utility of research on estimator variables (Wells, 1978).

B. System variables

The second type of study concerns itself with *system variables*. These are attributes of the legal system that can be controlled and modulated. The type and composition of an identification parade, and the interrogatory style of a police officer are examples of such attributes. The idea here is that one can improve the quality of a witness' testimony by using well researched, optimal procedures.

Of the substantial number of eywitness research articles that appeared after Neisser's invocation, the overwhelming majority have investigated estimator variables. The great majority of eyewitness research concerns itself with estimator variable research. Thus it has been found that child witnesses are slightly less accurate than adult witnesses, but this depends to some extent on just how young the child is (Ceci, Toglia & Ross, 1987). Generally, poor lighting conditions lead to less accurate testimony (Loftus, 1979), as do violent events (Loftus and Burns, 1982). I single out these findings, because they were

acknowledged in South African case law long before publication of the research (Tredoux & Tredoux, 1987).

I don't want to suggest that there is no important eywitness research. There have been important studies on system variables. One example should suffice.

The age-old procedure for conducting an identification parade is to line up the suspect alongside 6 or 7 similar looking innocent people and ask the witness to choose the suspect, if she is able. Psychological research has shown convincingly that this is far from optimal: in particular it appears to invite a choosing strategy in which the witness chooses that person from the lineup who looks most like the perpetrator (i.e. a relative judgement strategy). A much better way of doing things is to introduce the members of the parade to the witness one at a time. The witness is required to indicate whether the introduced person is the perpetrator or not. This produces a much lower rate of false positive identification (Lindsay & Wells, 1985). What is particularly interesting about this research is that police in America are following its cue, and it is now estimated that about 25% of all police lineups in the USA are sequential (Malpass, personal communication).

So, there certainly are examples of application to a real world problem in the eyewitness research tradition, but the point I want to make is that this has been rare. For the most part, eyewitness research has not gone significantly beyond the kind of cognitive psychology Neisser rallied against. It has adopted an apparent concern with a real-life

poblem, and this concern turns out to be superfluous on closer investigation.

Conclusion

To conclude this case study of eyewitness research, and indeed the paper, I want to assert again that a concern with making research more applicable to a society's problems may simply serve as a form of justificatory rhetoric. This seems to be the case for much eyewitness research. Here it is clear that the research so maligned by Neisser simply draped an everyday mantle over its spots.

There is an extra step required; that is, a step extra to the mere identification of a social problem or a 'real-life' concern. That step takes us beyond the scope of this paper, but it is worth noting that a useful first line of enquiry may be to examine examples of research that result in application, with the intention of identifying characteristic, enabling features. An example of this kind of enquiry can be found in Stolz (1981).

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