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UNIVERSITY OF OKLAHOMA

GRADUATE COLLEGE

ISSUES CONCERNING DECEPTION AND INFORMED CONSENT IN PSYCHOLOGY EXPERIMENTS

A DISSERTATION

SUBMITTED TO THE GRADUATE FACULTY

in partial fulfillment of the requirements for the

degree of

DOCTOR OF PHILOSOPHY

BY

C. D. HERRERA

Norman, Oklahoma

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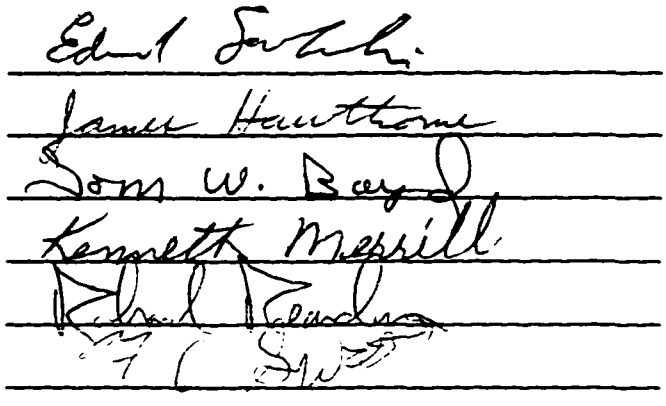
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A DISSERTATION

APPROVED FOR THE DEPARTMENT OF PHILOSOPHY

BY



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ABSTRACT

ISSUES CONCERNING DECEPTION AND INFORMED CONSENT IN PSYCHOLOGY EXPERIMENTS

Experimental psychology often involves the intentional deception or manipulation of human subjects. Psychologists typically defend deceptive experiments by first presupposing either the innocuousness of the deception or the importance of science. As I will show, psychologists have yet to justify deceptive experiments in terms that are not themselves contingent on value claims regarding such things as the freedom of inquiry or the role of scientific knowledge in Western societies. This dissertation offers a reexamination of deceptive psychology experiments, combined with an understanding of their historical and social background. My conclusions have implications for other areas of human-subject research, and the theory of informed consent. In that sense, I mean my discussion to show the need to ground all human-subject research within a common ethical framework.

CHAPTER ONE

INTRODUCTION

1.1 Survey of the Project

This collection of essays concerns a special category of psychology experiments. The experiments that I will discuss involve the intentional deception of human subjects; those subjects are typically college undergraduates. As I will explain, deceptive psychology experiments (DPEs) present several ethical problems. In each chapter essay I will address a different problem, acknowledging that this places a somewhat artificial distance between the problems.

Of the problems that I will cover, nearly all arise in a small branch of American psychology. I nonetheless try to remain alert to the fact that any meaningful inquiry of these problems will probably force upon us a much broader prospect. We may have to consider whether there is something wrong with our attitudes about human experimentation and institutionalized, academic science. Thus, I expect the force of my criticism to extend beyond psychological research, into all areas of human-subject research.

Nevertheless, I stop short of offering indictments of science as a potential subverter of values, a dangerous force, or illegitimate priesthood. Whatever its accuracy, that case is made better by other commentators. It is more important in our discussion to bear in mind this brand of skepticism, noting especially that critical attitudes

towards the relationship between science and morality are nothing new. Thinkers throughout the history of philosophy, poetry, science-fiction, and political theory (to name a few areas) warn that science and humanity might be natural antagonists rather than cohorts.

I say that we should bear in mind these critics because they are too often castigated as "luddites" (in what is probably not a fair portrayal of luddites either). But there is nothing inherently regressive in suggesting that we have misconceived the relationship between science and society. Midgley (1994) offers a recent argument to this point, that there is on the contrary a special, insidious risk in viewing science as savior and scientists as moral crusaders. There is thus a place for such sentiment in my analysis. And while I will not try to repeat or supplement such arguments, I would hope that the following essays might illustrate one area of particular concern.

1.2 Overview of the Essays

I like to think of these essays as offering a guided, though highly selective, tour of the ethical terrain of DPEs. In Chapter Two the tour begins where it should, with an exhibit that shows the problem at its widest expanse. Here you will find an overview of the traditional approach that commentators use when arguing over DPEs. I will sketch "the problem" of DPEs in broad terms, including mention of

this traditional approach, with suggestions for increased attention in areas of particular concern. My main purpose in this chapter is to show that DPEs pose ethical problems, and to show how we might increase the attention that these problems receive in applied ethics literature.

One area that does not get adequate treatment in the literature is the history of DPEs. In Chapter Three I show where added attention to historical factors is needed. For example, I expose popular misunderstandings about the history of DPEs, and show how these inaccurate views directly affect the ethical positions that commentators offer. To set the record straight on the history of DPEs, I will in this chapter offer an ethical interpretation of three defining episodes from that history. I will focus on the rise of psychological experimentation, the development of a code of ethics, and the way that psychologists and others reacted to some famous experiments.

In Chapter Four the tour turns most recognizably philosophical. Here I show one way we might apply a Kantian ethical perspective to the problem of DPEs. As I explain in the first essay, Kant receives a chilly reception, to put it mildly, within the debate over DPEs. This is in my view due to misunderstandings about Kantian ethics and bias or misunderstanding of psychological experimentation. As the debate stands, arguments for and against DPEs are nearly always utilitarian.

A Kantian view might sharpen the application of utilitarianism, and might also suggest an entirely different judgment. I acknowledge the risks in leaning too heavily on Kant's ethics. Some aspects of Kant's ethical theory do not lend themselves to the problem of DPEs. Yet some crucial elements of his theory do, and I will show where we might use these to understand the problem. In particular, Kant's view of autonomous end-setting reveals a poignant side of the ethical problem with deceptive experiments.

In Chapter Five we hear the case that psychologists make, the argument they use to justify DPEs. The usual argument has the psychologist alleging the benefits of DPEs outweigh the risks. We will see that even where this approach to justification works, it only gets at one potential method of analyzing the problem. And beyond any utilitarian conclusions, there remain questions about this being the most appropriate ethical theory. I show that whereas we should hear the utilitarian case, psychologists have yet to fully apply it, and even when they have, will not have begun to answer fundamental questions about DPEs.

In Chapter Six, I consider the ethical basis for what psychologists know as "debriefing." A debriefing is essentially a postexperimental interview and explanation session. Psychologists provide debriefings for a number of reasons, not the least of which is the debriefing requirement in the psychologist's code of ethics.

Commentators typically discuss deception and debriefing as if one is part or necessary accompanies the other. Whatever relation these two have, the cumulative effect is supposed to enhance the ethical standing of DPEs. I will raise some questions about this relationship; I find it odd, for instance, that anyone would argue an ethical point about deception by appealing to debriefing, or *vice-versa*.

It may seem equally odd to those acquainted with the literature that I have included a chapter on debriefing without a companion chapter on deception. In the debate over DPEs commentators make only oblique references to debriefing. This may be because it seems so natural to debrief. Still, we need to give debriefing the critical attention it deserves, and this requires separating it from the claims that commentators often make for deception when they really seem to apply to debriefing.

In Chapter Seven I reexamine a special aspect of the experimental context, the subject's autonomy. Autonomy, or autonomy-related concerns, play a significant role in many applied ethics discussions. This is especially so where commentators discuss human-subject research. Yet there have been few attempts to tailor an interpretation of autonomy to the DPE. Since "autonomy" often takes on different, sometimes contrasting, meanings, we will try to see what it might mean to the subject in a DPE, or to a psychologist conducting DPEs.

After surveying interpretations of autonomy and the questions that each interpretation raises, I will consider how researchers and subjects might rethink the experimental contract. This chapter concludes the body of the discussion, after showing how we might ethically combine deception and psychology experiments. While appeals to autonomy sometimes fail to clarify anything, autonomy makes a convenient standard, if we define it adequately, for the evaluation of deceptive experiments. Showing the need for and the plausibility of a more balanced evaluation of DPEs remains my ultimate goal.

Chapter Two

The Other Deceptive Experiments[1]

2.1 Overview

Deceptive psychology experiments (DPEs) seem ready-made for the kind of attention that commentators give medical experiments (*e.g.*, Beecher, 1970; Ramsey, 1970; Veatch, 1987). DPEs raise ethical issues commensurate with any that occupy the commentators who analyze medical or clinical experiments. These issues include the potential for exploitation, manipulation, and violation of the subject and his or her autonomy. In particular, two interrelated aspects of these areas of research, deception and informed consent, nearly always raise questions because they involve a clash between methodological and ethical values. Yet in the 1980s, for instance, few articles in the mainstream of bioethics treated these problems as they arise in DPEs.

A few articles, like Murray's (1980) autobiographical account of a psychologist's "Learning to Deceive," made interesting points about DPEs (as did Macklin and Sherwin, 1975) without fully locating these experiments within the bioethics realm. This means that whatever points such discussions make, their relevance seems sadly limited. There has clearly been no attempt at a systematic discussion, or

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[1] This chapter is based on a paper, "The Other Human-Subject Experiments," forthcoming in the *Journal of Medicine & Philosophy*.

even one that would address most readers of psychology journals.

To cite a more immediate measure of the lack of attention that DPEs receive, we might note that DPEs rarely receive coverage in the anthologies that pervade applied ethics.[2] And although a few discussions on medical ethics mention DPEs, the reference is usually indirect, part of a general discussion of some feature of clinical or human-subject research. Otherwise excellent texts like Beauchamp and Childress's *Principles of Biomedical Ethics* (1994) exemplify this approach.

In place of substantive discussion, commentators on medical experiments all too often offer enigmatic remarks about DPEs being in the same category, or claims to the effect that an argument about medical research naturally applies to these experiments. To these remarks commentators often add that space or time limitations prevent actually discussing DPEs; Jonas' (1970) influential discussion on medical experimentation provides a example of this practice, relegating DPEs to the footnote section. It is an interesting historical question to ask, whether the type of critique that Jonas offers might have changed our perception

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[2] Katz's (1972) work is an exception, though it is hardly the typical anthology. Katz discusses notorious DPEs, along with cases from medical research. Patten (1977a, 1977b) and Morelli (1983) discuss the Milgram obedience experiments, probably the most notorious DPEs. They offer a methodological, rather than ethical critique.

of DPEs. My hunch is that the debate might today be quite different, and our attention more inclusive of all human-subject research.

Perhaps it seems to commentators that too much methodological and philosophical space separates medical and psychological experiments. Although there are clearly differences between the various fields of human-subject research, there are reasons to think that psychology experiments should attract more equal billing with medical experiments. Again, nearly any ethical or philosophical issue that arises in one area arises in the other. This means that even if we aren't interested in the ethical issues that DPEs raise in their own right, our understanding of the ethical status of all human-subject research might improve if we gave more attention to DPEs. Not only that, it seems difficult to deny that there are aspects of DPEs that pose significant enough ethical problems to warrant concern.

I will therefore try to show in this essay what makes some features of DPEs problematic. My initial aim is to explain the nature of the debate over DPEs in the psychologist's literature. After summarizing this debate I will indicate where the argument deserves scrutiny. This will allow me to note areas that might be of special concern to commentators in mainstream bioethics. I will conclude by returning to the suggestion that commentators might apply wider, interdisciplinary attention to DPEs.

2.2 The Strong Case for DPEs

Psychologists who defend DPEs adopt a reasonably straightforward utilitarian position. This position forms what I will call the Strong Case for DPEs. It draws primarily on empirical claims about risks and benefits. The Strong Case issues from a methodological justification of deception, but concludes by touting the practical advantages of deception. Advocates contend, that is, that deception increases the psychologist's experimental control. Psychologists need this control because human subjects tend to become apprehensive while under study; they often try to anticipate or respond to cues within the experimental setting. To counteract this tendency psychologists resort to deception in various forms. Psychologists might intentionally offer false statements or they might leave relevant details out of an explanation of the experiment, for example.

The various forms and degrees of deception have the common feature of providing an information advantage. If, as one psychologist explains, "deception is a necessary commonplace in psychological research," it is in no small part because "one does not give subjects the California F Scale and ask them to 'fill out this test, which shows how authoritarian you are'" (Rosenthal, 1966, p. 165). There are, as we will see in subsequent chapters, other psychologists who provide more elaborate explanations, but

deception's practical or methodological advantages remain the key (*e.g.*, Reynolds, 1979; Sieber, 1992).

And to round out the Strong Case, advocates claim that deception does not harm anyone; those subjects who find the whole affair unsettling are always free to withdraw from participation without penalty. It is never clear where priority falls in this argument. We might wonder, for example, whether the supposed innocuousness of deception is the reason psychologists have explored its practical applications, or the other way round. In any event, the argument that begins with reference to a laboratory need ends with mention of ethical concerns.

2.3 The Critical Position

Against the Strong Case, critics claim that deception exposes subjects to risks. These risks are never really specified, but critics seem to have in mind forms of stress, confusion, or mere embarrassment that subjects might suffer from having been deceived. Critics, as we will see throughout these essays, are constrained to point out that subjects consent to participate with the knowledge that they will be deceived. Still, critics can mount a reasonable case that some wrongness remains where subjects are led to believe intentionally false information, and not merely because various risks might follow from this.

For example, even if the subject expects deception, it is not until the experiment is over that the psychologist reveals to the subject the extent of his or her gullibility. Critics also worry that since subjects are, by design, not told the truth about their participation before the experiment, they cannot readily assess the risks that might exist (we return to this point in Chapter Eight). And there is also a sense in which psychologists are in no position to predict what risks await the subjects, before or after the experiment. Hence, taking the voluntary nature of participation into account does not eliminate the doubts that the use of deception raises. These doubts center on the level of informed consent, the degree to which subjects really volunteer to serve in the DPEs, and the ethical ramifications of the psychologist's revealing the deception afterwards.

There are critics, slightly on the fringe of this ethical issue, who raise methodological questions about deception. Some allege that introducing deception into the experiment undermines the validity of the psychologist's observations. In effect, this argument holds that deception makes an artificial setting that much less realistic. This, critics argue, reduces the usefulness of the experimental findings, since it becomes harder to envision an extrapolation of these findings to real-world behaviors.

Still other critics argue from a self-protection position. They warn that continued use of deception will harm the psychologist's professional reputation. Eventually those who do not even use human subjects might be affected. Those who deceive might find it more and more difficult to locate useful (*i.e.*, naive) subjects. Not unexpectedly, there is a great deal of research in this area, exploring the attitudes and impressions that subjects bring into the experimental setting.

There are two things to note at this point about the critic's position.[3] First, as we will see, this position remains squarely within the utilitarian boundaries of the argument it attacks. Arguments against deception emphasize empirically verifiable and immediate consequences; in other cases, the critics project empirical results of continued deception. A handful of psychologists have occasionally tried to construct an eclectic position. This position would integrate principles like dignity or respect into the empirical-utilitarian framework. But values other than those expressed in the consequentialist position are rarely the focus, and the eclectic position turns out to be not so diversified after all.

The second point to note is that critics have not been very successful. Not only have they failed to offer a

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[3] Readers interested in more details of this position might consult Greenberg and Folger's (1988) survey and excellent bibliography.

persuasive utilitarian case, they have shown little interest in devising a plausible case that would move the debate past utilitarianism. In a sense, critics have yet to demonstrate that DPEs pose a serious ethical problem within psychological circles; and critics have made no progress in showing that commentators outside of experimental psychology should be concerned.

2.4 The Rejoinder to Critics

Mindful of the critic, advocates of the Strong Case would probably admit to the difficulties inherent in risk-benefit calculations. In particular, few psychologists attempt to show how they would calculate the risks or benefits of, say, administering California F Scales under deception. Psychologists tend instead to fall back on the claim that the benefits that deception provides justify any risks to subjects. As for questions about expressing *benefits* in measurable form, psychologists can argue that *harm* is the more crucial parameter, and they have tested for that.

In other words, psychologists who may not clearly understand the benefits of deception, might still suggest that successful experiments constitute some kind of benefit. Furthermore, psychologists have been careful to present the deception in DPEs as harmless and temporary. The

psychologist proves this last claim by gathering empirical data from the psychology experiments themselves.

> Although the ethical acceptability of the use of deception per se depends upon the ethical theory (*e.g.*, utilitarianism) one espouses, the presumed consequences of the use of deception in psychological research are subject to scientific investigation. . . . Although the critics of deception have not empirically investigated the impact of deception, a number of other investigators have. Such information needs to be considered when assessing the ethical acceptability of the use of deception. (Christensen, 1988, p. 665)

The investigations mentioned here make up what is perhaps the most direct way of addressing ethical issues. Psychologists ask subjects if they mind deception, and they test subjects for adverse effects. Each psychologist might rely on a different measure, but there enough innovative tools, such as surveys and role-playing, usually applied during the post-experimental debriefing, to support the Strong Case.

One advantage of these tools, and the appeal to empirical details, is that they eliminate the need to draw on abstract philosophical principles or even ordinary morality. "The evidence," one psychologist reports,

> overwhelmingly reveals that research participants do not mind being deceived or having their privacy invaded, which seems to tip the scale in favor of using deception in important research that is properly designed and conducted. (Christensen, 1988, p. 671; cf. Smith & Bernard, 1982).

This might understate the case. Studies reveal little concern, much less harm, among subjects. Subjects seem not to mind deception, or in general, worry over ethical issues associated with experimentation.

Possibly because critics do not make clear what damage might come to psychology's public image from DPEs, the defense of DPEs pays little notice to this potential risk. Advocates for DPEs might look instead to one direct measure: the continued public and institutional support DPEs receive. There have also been few substantive attempts to validate claims about deception undermining the integrity of experimental findings. Here too, the psychologist may see no practical incentive in answering the charge. Every published report of a DPE offers an important, though informal, "confirmation" of the method. In any event, psychologists can readily claim that, methodological flaws aside, DPEs remain the only option in some areas of experimental psychology. This response reflects the code of ethics, which as we will see shortly, requires that psychologists consider

other methods (and the risk-benefit picture) before using deception.

Finally, there is another side to the defense of the Strong Case, though it builds on claims that are at best indirectly utilitarian. Psychologists typically supplement talk of a favorable risk-benefit assessment with two arguments from comparison. Under one comparison, the DPE advocate maintains that subjects are more likely to encounter deception *outside* the laboratory, and in ways that might pose a greater risk than anything they face in the lab. As the following excerpt shows, this comparison supports the concern that the critic is being unfair, or overactive.

> Many of the ethical sermons being preached to social scientists seem to assume that those participating in research projects would never encounter given discomforts if they did not participate in the research. This is, of course, ridiculous, since such 'discomforts' are part of everyday life: any unfamiliar situation will create anxiety, everyone finds it interesting to have 'private' information about another, and deceptive information is presented at every turn, particularly in advertising and political speeches. . . If a salesman deliberately deceives a prospective customer, he makes no attempt, after

> the sale, to reveal this deception. If social scientists were not so honest, subjects would not be aware of the deception and, hence, not so upset about their treatment. (Reynolds, 1972, p. 699; cf. Gergen, 1973; Milgram, 1977)

Compared to ordinary or "white lies" then, experimental deception is supposed to be innocuous. It is supposed to appear at least comparatively better than the garden-variety deception, if only because psychologists conduct DPEs under what are presumably beneficent intentions, with the expectation of social and scientific gain.

The second, related comparison is a more complicated, but it aims at roughly the same point. The psychologist assumes that some medical research justifiably uses deception, and that this research poses far more danger to subjects than a DPE. If, therefore, the more risky experiments in medicine are justified, the psychologist contends that DPEs have to be at least as justified. Stated another way, however problematic DPEs are, subjects are never at risk the way they would be in a medical study that involved untested drugs or placebos, for instance. And if it is permissible (even obligatory under some interpretations of beneficence) to supply a placebo to a subject who might expect medication, arguing against relatively safe psychology experiments is as misguided.

Such arguments are probably as misguided, the DPE advocate might add, as viewing DPEs in a harsher light than the ordinary deception in everyday life. With this comparison the DPE advocate might hope to show as well that DPEs are ethical, because they involve deception in ways that are preferable to what is otherwise accepted as innocuous or unavoidable. DPEs would on that account be a necessary evil on one hand, and a lesser evil on the other.

In summary, the Strong Case asks how there can be offense to character or principle where there is no serious risk or complaint, and where there seems to be positive, social and scientific gain. The utilitarian, risk-benefit portion of the Strong Case succeeds, to cite one reason, because psychologists set criteria for justifying deception that they appear well-positioned to satisfy. Experimental psychology is supposed to be the science of observing reactions, assessing first-person reports, and quantifying value statements. This means that psychologists, by their own account, *should* know best when deception harms a person. Psychologists should also be able to discern deception's effects on experimental validity. Who could begin to explore what harm is, if not the psychologist? And who could better determine the practical advantages of deception or explain why it has to be used?

2.5 Specific Areas of Concern

2.6 Utilitarianism

Since I will in the following essays attempt to directly refute the Strong Case (see especially Chapter 5), I will here only try to show where a critic might apply scrutiny to the general framework I have just described. Most important, we might ask tough questions about the utilitarian basis of the Strong Case; as a secondary aim, we might challenge the argument from comparison, or at least the Strong Case's reliance on these comparisons.

Most of the utilitarian problems arise from the emphasis on empirical claims, the psychologist's ability to validate them, and the assumption that consequentialist concerns should dictate the argument for justification. In a sense, the questions here are germane to most utilitarian positions, and should not appear novel to any student of ethical theory. First, a critic might object that the psychologist's ability to assign values to risk and benefit is unproven. Particularly where it rests on assumptions or predictions about results, the Strong Case inherits the drawbacks of the kind of risk-benefit analysis that utilitarian arguments often rely on.

Admittedly, it is unclear precisely what psychologists would have to prove to establish an ethical point about deception *via* empiricism. It does seem obvious nonetheless

that there are concerns regarding the type or nature of the harms that might come from DPEs. It does not follow from this concern that psychologists would be able to define "harmless" as a lack of empirical effects, unless we grant that this could merely obscure some of the social meaning of deceiving another person. Hence, while it would be wrong (though perhaps not too unfair) to deny the importance of data that suggest that subjects do not mind being deceived, it would be equally mistaken to afford these data too much relevance.

In addition to some fairly general questions about the risks of deception, the critic can raise difficult questions about benefits. Are the benefits that deception is supposed to provide possible without deception? Are they objectively meaningful, in the sense that they are valuable in themselves, or is the value of these benefits contingent on as yet unproven claims about the utility of DPEs or experimental psychology? I will pursue such questions at length in Chapters Five and Seven. I will only mention them here to show that both arms of the utilitarian balance deserve attention.

The previous questions get at the difference between a convincing utilitarian case and one built more on preliminary assumptions. But we might ask even more substantive questions about the details of utilitarian argument. We might question the standard practice of

appealing to utilitarianism in the first place. There are also significant, and usually unaddressed assumptions regarding what is to count as a "value," a "risk," and so on.

Another assumption supporting the Strong Case holds that psychologists only need to calculate *expected* consequences, rather than actual risks or benefits. Before conceding the ground to the utilitarian, the critic might ask for greater proof of the psychologist's ability to calculate such values. We have noted that the critic has good reason to be skeptical of the psychologists' claim that deception is innocuous because they have uncovered no measurable effects. It might be that psychologists are not in a position to objectively assess the merits of DPEs, even if it turns out that utilitarianism is the best theory in application.

This last concern should not be dismissed as *ad hominem*; the critic need not allege that psychologists are depraved and cannot be trusted, or that psychologists appeal to utilitarianism because it provides the best fit of evidence. There are many questions associated with utilitarianism and other theories that those who would apply them should be prepared to answer. In evaluating the answers that psychologists give to these questions, we should consider potential biases. Psychologists genuinely concerned about deception might still not be able to avoid the

potential for bias. For example, as opposed to letting psychologists decide which test will show that deception is ethical, and allowing them to rely on their own methods and values to meet this test, the critic might simply want to bring those who can evaluate the claims at a distance, with perhaps less risk of bias or circularity.

I would hope that my comments do not suggest that the Strong Case for deception is the only source of concern in the debate over DPEs. There are unfortunately aspects of the critic's argument that deserve scrutiny as well. Most important, in their attempt to bring counter-utilitarian arguments against the Strong Case, some critics display an apparent misunderstanding of utilitarianism and its application to DPEs. It seems that critics sometimes argue within an artificially narrow view of utilitarianism. From this view, the critics offer very selective interpretations of the theory.

I will discuss a few noteworthy examples of where the critic's argument goes wrong. I should preface this section by saying that these selections from the psychologist's literature offer some of the better examples. In contrast, some discussions on DPEs never reach a point where appeal to ethical theory would make sense. In the published proceedings of a symposium on deception (see Kennedy, 1975), for example, two psychologists argue whether the other has attained the appropriate level of moral development. This is

a less than encouraging approach to clarifying the deception issue, to say the least. The quarrel eventually turns to Nietzsche's place on Lawrence Kohlberg's hypothesized moral-development scale, and at that point readers might be forgiven for becoming skeptical about closure.

One recurrent problem in the better discussions is the attempt to bring values or principles not ordinarily associated with utilitarianism into the application of that theory to DPEs. For example, critics of DPEs sometimes talk as if respect should be a deciding principle, for example, when the focus of their argument is on the empirical effects that deception has on subjects or even psychologists (*e.g.*, Baumrind, 1979; 1985).

But where we might want to conceive of a utilitarian "respect," we will have to temper our expectations. This kind of respect will have to compete with all other parameters in the calculations. There is a risk in trying to elevate the utilitarianism to a higher ethical plane by stipulating that respect holds special value. That approach raises problems about the need to cater to empirical effects of deception in any event. The hybrid or "cafeteria" approach will also rarely settle an ethical question. "It is," one commentator on utilitarianism notes,

> fatally easy to lump together all the good and useful principles that one knows, to formulate them in one of the 'exact' mathematical languages

> used by economists and analytical philosophers, and to claim that the result is a revised and sensitive version of utilitarianism. The moral principles involved are often mutually contradictory, or consistent in only in trivial cases. On the other hand, even assuming that the various principles can be adequately prioritized and harmonized, it is not evident that the resulting doctrines should be classed under the heading of utilitarianism. (Hayry, 1994, p. 75)

Problems associated with the hybrid ethical theories also affects otherwise good discussions on DPEs, such as the *Belmont Report* (1978) and the psychologist's code of ethics (American Psychological Association, 1992).

These documents urge that psychologists maintain an emphasis on risk-benefit analysis while still catering to autonomy concerns or Kantian respect-for-persons. These are undoubtedly important values and parameters for psychologists to consider. It remains to be seen whether these can be integrated into a consistent utilitarian position, or any ethical position at all. Perhaps the fact that documents like the *Belmont Report* typically guide the Institutional Review Boards that pass judgment on DPEs should raise special concern.[4]

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[4] There has been some interesting critical analyses of the ethical positions prescribed by the psychologist's code of ethics and the *Belmont Report* (see *e.g.*, Blackstone, 1975;

As there are problems with the attempt to import too much into a utilitarian position, another problem is that critics often appeal to utilitarianism without really showing that it has met an adequate test. In an oft-cited critique of deception, Baumrind (1985) rejects "act-utilitarianism." In her view it "falls short as a metaethical system of justification." Act-utilitarianism allegedly "fails to consider the substantive rights of the minority," and "fails to take long-range costs into account." Worst of all, act-utilitarianism is supposed to be "subjective and not generalizable" (p. 166).

All this might be true on a special interpretation of utilitarianism. But Baumrind does not fully defend that interpretation, and when she advocates rule-utilitarianism instead (p. 167), Baumrind fails to explain why act-utilitarianism (or any other utilitarianism) would only count short-term consequences. Baumrind and those who support an act-utilitarian approach also fail to show what makes rule-utilitarianism any less subjective, than the approach it would replace.

The confusion in this critic's position extends into more recent attempts to refute the Strong Case. Two other psychologists contend that

> moral discussions involving deception include consequential philosophies, most notably act-

and Koocher, 1985; Marshall, 1976; and Smith, 1976).

> utilitarianism and rule-utilitarianism, and non-consequential positions, most notably the deontological perspective. . . . The rule-utilitarian applies a consequential argument resulting in the nonrelativistic position that deception in research is never acceptable . . . . The deontological argument rests on the nonconsequential Kantian position that we must not treat an individual as a means to an end. From this moral perspective, psychologists should never conduct studies that either violate an individual's autonomy, and thus personhood, or cause harm to an individual. (Fisher and Fyrberg, 1994, p. 1)

These authors advocate a rule-utilitarian position (citing Baumrind), yet their selection also does not appear to be based on an adequate understanding of the ethical theory, or the task in applying it. The question of adequate understanding is particularly relevant since one of the authors, Celia Fisher, is at the time of this writing a member of the Ethics Committee of the American Psychological Association.

Without belaboring this point, we should note that there is some technical knowledge regarding ethical theory and application that is prerequisite to challenging or defending DPEs. Whereas no critic needs to supplement an

argument against DPEs with a scholarly study of utilitarianism, moves in that direction would go a long way. This is not because utilitarianism is a weak theory that needs constant resuscitation; I will argue that utilitarianism holds some promise. But this debate may never approach closure if the two sides cannot be clear on what they are alleging and why.

This leads to my final point about utilitarianism. This point applies to DPE advocates and critics alike. Too often commentators discuss DPEs as if utilitarianism (in some form) is the only theoretical option. Aside from the occasional references to rights or autonomy, deontology seems particularly unwelcome.

Interestingly, *critics* of DPEs often express the strongest apprehension. One warns that a deontological position against deception would be far more dangerous than the deception itself (Gergen, 1973, p. 908). To others deontology threatens scientific exploration and the right to research and only offers "moral posturing" in return (Adair, *et al.*, 1985, p. 65; cf. Kimmel, 1979; Wulff, 1979). Warnings go out to the "working experimental social psychologist," against "conceiv[ing] the issue in absolutist terms" when he or she should be "well aware of the fact that there are good reasons for using deception. . . ." (Kelman, 1967, p. 210; Oliansky, 1991). Once commentators retell the story of Immanuel Kant's refusing to lie even to save a

friend's life, it is understandable that deontology appears "dogmatic" (Baumrind, 1985; Elms, 1982; Reynolds, 1979).

My aim in these essays is not to make sure that each ethical position is represented in the debate over DPEs. As rich as that idea sounds, it seems both unwieldy and unconvincing. The two major positions in this debate, again, for better or worse, are deontology and utilitarianism. Deontology strikes some (perhaps some Kantians) as dogmatic in other applications as well. But as I hope to show in the following discussion, the one-line refutations or ridicule that deontology receives should make even utilitarians suspect the thoroughness of the inquiry and the security of their position.

2.7 The Argument from Comparison

The comparison between DPEs and other forms of deception is part of the Strong Case. But since it is often taken as a separate claim, I have decided to sketch the problems with the comparison here and in delve more deeply into the comparison and the rest of the argument for DPEs in Chapter Five. We need only to bear in mind here that the comparison attempts to supplement the consequentialist position, and becomes in the end meaningful only in the context of the Strong Case.

There are first of all questions about the accuracy of this argument from comparison, and we will discuss these in

a moment. It would be simpler to set the stage with an examination of the extreme forms that the comparison can take. In particular, there are commentators who cite the comparison to other types of deception and research as evidence that DPEs raise no real ethical problems. One commentator warns, for example, that

> many of the ethical sermons being preached to social scientists seem to assume that those participating in research projects would never encounter given discomforts if they did not participate in the research. This is, of course, ridiculous, since such 'discomforts' are part of everyday life: any unfamiliar situation will create anxiety, everyone finds it interesting to have 'private' information about another, and deceptive information is presented at every turn, particularly in advertising and political speeches. . . If social scientists were not so honest, subjects would not be aware of the deception and, hence, not so upset about their treatment. (Reynolds, 1972, p. 699)

The obvious problem with this line of argument is that prospective subjects are not compelled to participate in either medical or psychological research. Their options are not restricted to the selection of high- or low-risk deception. Subjects have the option of not participating

where deception occurs, and perhaps not participating at all.

This is another way of stating the options at their most basic level: physicians and psychologists might not conduct any research, deceptive or otherwise. It stands to reason, given these real options, that to allege that subjects in a DPE are better off than those in medical tests for AIDS vaccines, for example, is to misrepresent the options. This false dilemma is problematic as well for its vagueness: what, precisely, are we to infer from this presentation of the options? There might be an argument that could pose the situation that faces the subject in these terms, but it would still need to have some bearing on the ethical issue at hand. At best, then, this type of comparison reveals the need for a further argument that would justify such a restriction of options.

In addition, for this comparison to work, the psychologist may need to show why medical experiments should serve as a standard of risk. Psychologists might also explain why lies encountered in everyday life should count as ethical benchmarks. Lacking such an explanation, the comparison is bound to appear biased. It is biased in the direction of showing DPEs to be innocuous, and equally biased towards showing that deception elsewhere is supposed to be at least as safe (this last point becomes a difficult one to reconcile with the claim that DPEs are justified

because they are less harmful than deceptive medical experiments).

The ethical status of some medical experiments, like Randomized Clinical Trials, is uncertain, in part because they involve some deceptive elements. Even where the patient does not risk life, it is anything but established that doctors might ethically distribute a placebo or untested drug instead of known remedies. The comparison to real-world deception fails for a similar reason. It makes no sense to judge the DPE by standards that are supposed to be excessive. That people are so often deceived in the normal course of their lives might even provide a reason to avoid deception in the lab. That deception is generally inconsistent with most everything else about science and higher-education provides another reason.

There are arguments that psychologists might make in response to these charges. They might, for instance, allege that *their* science is only viable with some deception, and that deception elsewhere is avoidable. But given the usually negative features of deception, it is difficult to defend deception by claiming that it occurs elsewhere or that one form is better than another. There might also be a problem in stipulating that experimentation, as a certain type of inquiry, is so dependent on deception.

Another example might show the difficulty in using a utilitarian comparison to defend the deception in a DPE. It

is relatively easy to concede that the deception in a psychology experiment is morally preferable to what I might suffer in a deceptive business deal. It is preferable at least in the sense that I lose less that I might later claim I was entitled to. But if the consequences of the deception are at issue, where presumably someone benefits and another takes all the risks, it is again hard to see why this won't reflect on the DPEs. Subjects in the DPE bear the risks, while psychologists control the nature and duration of the deception, and so forth. It seems, then, that if there is a distinction between the two types of deceptive scenarios, it is only apparent on a narrow view of the situation. In the end, if the comparison proves anything, it proves how similar, and how wrong, most forms of deception are.

In addition, where psychologists mention placebos in their arguments defending DPEs (*e.g.*, Sieber, 1992), they may offer a distorted view of facts that are crucial to the comparison. First, the psychologist may not be on safe ground in assuming that placebos are "deceptive" in the way that the psychologist's methods are. No doubt there is something less than honest about a placebo. But researchers can use a placebo while minimizing the outright deception concerning the placebo itself. Physicians typically administer placebos "blind," that is, without knowing any individual subject's chances of getting one. And even then few physicians would allege that there are no ethical

problems with placebo-use or other potentially deceptive methods (though the immediate problem with placebos may not involve deception *per se*).

Stricter criteria also apply to placebo-use and informed-consent in medical experiments than apply to the psychologist's deception. As I show in the following chapter, psychologists have historically refused to adopt such criteria (Faden & Beauchamp, 1986). The psychologist may offer an argument that would justify the difference in ethical standards; but the comparison of DPEs to placebos or randomized clinical trials would still appear more genuine if it included some acknowledgement of this difference. It does not seem that claims about the greater risk that subjects face from medical experiments will prove the case. As true as these claims may be, a critic can meet them on utilitarian terms: medical researchers can show a greater prospect of scientific and personal benefit to subjects.

One final point regarding the argument from comparison. In the comparisons to medical research psychologists often include claims to the effect that they, like medical researchers, have a duty to provide scientific knowledge. The idea that such a duty might exist is supposed to offer some support for deception. It could, for instance, seem more harmful not to perform DPEs if psychologists could show that these experiments provide some benefit. I think the safest course, however, would be to think only

metaphorically about a "duty" to advance knowledge of human behavior or health.

The concern in this case is that, even assuming that such a duty really exists, it still might not follow that *deception* is justified. This is a complex issue that I do not pretend to have settled with a few comments. But perhaps that gets at the problem. There is more to justifying deception than citing, for instance, the benefits of research in general. (Analogously, if we are attempting to justify scientific research, we cannot tout the benefits derived from a smallpox vaccine without saying something about the moral justification of deaths from atomic weapons programs.)

Aside from this issue of justification (which I return to in Chapter Five), there are potential utilitarian responses to the idea of psychologists duty-bound to deceive subjects. By allowing psychologists to justify deviations from informed consent or ordinary morality on the basis of some duty, we risk viewing informed consent as an impediment. Here there are good reasons to move away from thought experiment. The notion that a duty to perform research can override consideration for autonomy or subject welfare pervaded some notorious examples from the history of medical research, such as the Tuskegee Syphilis studies (Pence, 1990).

So while we psychologists clearly have some duties, and some of these may relate to a duty to pursue knowledge, it may be best to limit our claims about those duties. That failing, we should carefully delineate these duties from the duties that psychologists have towards the subjects. An analysis of the competing rights and duties in the experimental situation is overdue. For now, that type of analysis is not something that the psychologist must supply before justifying DPEs. In the absence of such an analysis or interpretation, the continued appeal to duties and comparisons will merely add another layer of uncertainty onto an already vague argument.

This concludes discussion of what I see as the main areas of concern for psychologists and commentators in applied ethics. Most of these areas will receive more consideration in subsequent chapters. I grant that in the interest of making headway in the debate over DPEs there is a temptation to set aside questions about the approach commentators use or the form of arguments they offer about DPEs. But we should bear in mind that any ethical debate is only a meaningful exchange of claims where both sides can understand each other.

In addition, the integrity or character of the debate becomes important to the extent that it makes it possible for those outside of psychology to identify and respond to the main positions. In short, if more commentators see what

they consider a healthy debate occurring in the pages of psychology journals, they might be encouraged to register their views. The debate can only gain from such an infusion of ideas and perspectives. If, in contrast, commentators in medical ethics cannot make sense of the opposing arguments, or cannot see where the debate has advanced beyond relatively simple utilitarian claims, they are unlikely to see a clear point of engagement. In that case, would-be commentators might be excused for concentrating more on traditional areas of applied-ethics like abortion and euthanasia.

On the assumption that it is possible to present the problem of DPEs in terms interesting and intelligible to bioethics commentators, I will in the concluding section of this discussion say a bit more about bringing DPEs into the mainstream of bioethics.

2.8 A Closing Suggestion

In a sense, we can construe the problem of getting others interested in DPEs as one of presentation or rhetoric. Along this rhetorical dimension, perhaps DPEs have not been made to look like serious topics. If so, one way to make them seem more like traditional bioethics topics would be to apply a more inclusive, interdisciplinary approach to DPEs. Currently, it is not uncommon to find lawyers, physicians, theologians, and philosophers debating medical

research. Such an approach might better illuminate some of the issues I have sketched.

One issue that bears on the ethical status of the deception, and that falls squarely into contemporary bioethics categories, is the nature of the interaction between psychologists and subjects. Again, the discussion that has taken place for half of this century has remained almost entirely within the psychologist's literature. It is not surprising therefore, that opinions tend to express prevailing value-judgments within experimental psychology about what the proper relationship between those in the laboratory should be.

Turning somewhat towards a medical-ethics model would serve as an important interim step to establishing a generalized theory or model of human experimentation. The value in such a model would be its delineation of roles and consideration for the researcher and subject. Bioethicist Robert Veatch (1987) provides a model for medical experimentation that holds promise for wider application. He emphasizes informed consent, utilitarian calculation (*i.e.*, beneficence), and autonomy.

A model like Veatch's is especially promising for the application to psychology experiments because of its focus on researcher-subject interaction. Veatch suggests, for example, that the researcher and subject interact as equals, collaborating in the search for knowledge.

> The metaphor of partners is exact. Partners normally come together not because they share exactly the same interests and abilities, but because there is some mutuality of interests, some common point of intersection where each can help the other. (Veatch, 1987, p. 6)

Ramsey (1970), and to a lesser degree Faden and Beauchamp (1986), also advocate bilateral models for experimentation. Regrettably, no commentator is clear on exactly what this collaboration between researcher and subject would amount to. It is far from clear (something Veatch acknowledges) how revising the roles and even allotting more power to the subject will take care of ethical issues. Yet the intention behind these models, what we might call their general value stance, might extend across disciplines, even if the details have to remain sketchy for now. (We might ask whether the relationship between psychologist and subject is any clearer under the prevailing model of interaction.)

Suppose that we interpreted Veatch's suggestion that researcher and subject design the experiment together as a way for participants to accommodate each other's differing values or needs. Keeping in mind that such a portrait will best serve as an ideal, we could see where psychologists have objectives that will naturally exclude some of the subject's values. Likewise, we can see where the best

conditions of informed consent could leave the subjects at an epistemic or informational disadvantage.

As an ideal nonetheless, a more participatory model of researcher-subject interaction should lead to a

> convergence of interests where both researcher and subject have something to gain by participating and where each is called upon to make some sacrifice for the benefit of the other. These are the true makings of a partnership, it only it is allowed to flourish. . . . If researcher and subject are seen as partners who are both autonomous, responsible, dignified human agents coming together to form a limited covenant for pursuit of mutual interest, virtually all aspects of the ethics of clinical research are affected. (Veatch, 1987, p. 8)

To this we might add that there has been some moves in this direction. A few psychologists concerned with ethical issues (*e.g.*, Kelman, 1972; Schuler, 1982) suggested a similar reorientation some time ago. Their suggested revisions to the prevailing experimental model met with little apparent success. Yet these suggestions may simply have lacked a sufficiently developed theory of ethics or experimental interaction. Renewed interest in DPEs might be able to compensate for this lack.

Whatever the reasons that suggestions like this failed to attract sufficient attention, that we might now apply the spirit of the suggestions within the framework of a medical-ethics model is an enticing possibility. The suggestions about revising the rules of the researcher-subject interaction gives an indication of how far from the current debate we might travel. It shows as well the kind of contribution that psychologists and others can provide. Perhaps there is also some indication here that the disciplines are not as far apart as we might have suspected.

I will in the concluding chapter show one way that such a revision might begin, with particular emphasis on subject autonomy. For now I will only mention the need for the revision, and I will concede that devising a more inclusive and philosophically consistent position on deception will not be easy. There are practical barriers to an interdisciplinary approach to DPEs. Psychologists have more to gain from reputations as research scientists than as applied-ethics specialists (or even in some cases as academics). The allocation of status may play a role in the philosopher's self-imposed distance from the DPE issue. While few commentators in higher education, where most DPEs occur, have more training in ethical analysis than philosophers (certainly few people have more time to devote to such matters), serving on the ethics committee of a small

research hospital carries more prestige for the philosopher than sitting on an IRB at a major university.

An analysis of the institutional and financial determinants of this problem are beyond my concern in this discussion. The *status quo* may reveal something negative about the psychologist and the philosopher; it likely shows where the traditional division of labor adds to the difficulty in resolving the deception issue. This drawback is evidenced, for example, in the fact that the psychologists who actually perform DPEs can for a number of reasons least afford to debate the ethical status of these experiments. And again, philosophers, who might be best equipped for critical discussion on the problem, show little interest (understandably in some cases) in examining this branch of human experimentation.

Perhaps with such practical, disciplinary barriers in mind, psychologists and some bioethics commentators might hesitate to incorporate psychology experiments into the mainstream. They might assume that while medical and psychological experiments both involve human subjects and deception, the axiological and scientific differences are too substantial. It might appear that medicine and medical ethics rest on a theological and humanistic foundation. This foundation allows commentators to incorporate notions like autonomy and respect-for-persons into discussions of

deception in medical ethics, but it may not be accessible or applicable to psychology.

If psychology does not have an equivalent foundation, this might keep such ideas and principles outside the sphere of ethical concern. It might seem to make no sense to try to resolve ethical issues about DPEs with criteria more applicable to medical ethics. Why, that is, expect psychologists to cozy up to a value structure from a healing or helping profession when debating the deception issue? But while these barriers to an interdisciplinary approach are real, they need not be insurmountable.

As much as important differences will always exist within the branches of human-subject research, a re-visioned approach to the problem of DPEs could take these into account. Indeed, to provide a meaningful ethical analysis, the interdisciplinary approach would have to accommodate differences between the fields of experimentation. This means that critics can point out obvious differences between the fields of research, but they should also note that there are obvious enough similarities to justify the integration of, say, a medical-ethics model of researcher-subject interaction.

The point that I hope to make in the following chapters is that the psychologist, and anyone else performing experiments with humans, should be able to justify such things as intentional deception. It should not matter that

the psychologist uses a unique brand of deception. Instead, the justification requirement reflects the belief that, at the most basic level, the ethical problem with DPEs is a problem relevant to any human research. And as I hope to have shown, even if a medical-ethics model cannot provide immediate solutions to ethical problems like this, combining this model with the traditional approach to DPEs might provide a broader basis for discussing these experiments.

Chapter Three

Milgram's Obedience-to-Authority Experiments in Historical Context[5]

3.1 Overview: The Standard Account

In the previous chapter I sought to show how commentators might give greater attention to the ethics of deceptive psychology experiments (DPEs). In this chapter I advocate a similar line regarding attention to the history of DPEs. I will argue from the position, or assumption, that a critical analysis of DPEs requires at least a rough understanding of the history of DPEs. Accordingly, I will in this chapter offer an admittedly selective interpretation of what I will call the Standard Account of this history.

My interpretation of the Standard Account is meant to counteract the most frequent portrayal of the historical development of our attitudes towards DPEs. This portrayal is, in a word, positive or progressive. The generally positive view that most often surfaces in discussions of DPEs asserts that psychologists now conduct DPEs under conditions different from those that prevailed in the past. Similarly, the Standard Account holds that the use of DPEs

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[5] Much of the argument in this chapter parallels that in "A Historical Interpretation of Deceptive Experiments in American Psychology," forthcoming in *The History of the Human Sciences*. In that paper I make more of an attempt to link the problem of deception with the methodological developments in experimental psychology. I am especially interested in showing there, as I am not here, the influence of the behavioristic model of experimentation.

is declining, or should be, because experimental psychologists have changed their value-orientation. In particular, we are supposed to view psychologists as more sensitive to ethical problems with deception than they were in the decades immediately after the Second World War.

To make this point, historians often contrast current DPEs with well-known experiments from the past. The noted Milgram obedience studies conducted during the 1960s provide a favorite point of historical and ethical reference.[6] As the Standard Account would have it, both attitudes and experiments have changed since Milgram's research. Where psychologists still rely on deception, they presumably deceive in a different way, or at any rate, with a different feeling towards the deception and the subjects.

Indeed, the obedience studies are cited as turning points in the general attitude towards deception, and in the specific treatment of experimental subjects. One historian claims, for example, that "the Milgram studies . . . have been a focal point in the ongoing debate over experimental ethics." It is arguable, he continues, "whether this research has been more influential in the area of social psychology or in the policy formation on the ethical

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[6] In this chapter I presuppose some familiarity with the Milgram studies. My description of these studies does not deviate greatly from the versions given in Milgram's (1974) own account of his work or Miller's (1986) commentary.

treatment of human subjects in psychological research" (Hock, 1992, p. 308).

There is a reassurance in this and other versions of the Standard Account. Another historian, discussing DPEs, asks

> Do psychologists routinely place research participants in such stressful situations . . . with shock machines? . . . The answer is a resounding 'No!' [These] studies were conducted in the 'pre-ethical principles era' in psychological research. Indeed, the study by Milgram aroused so much controversy among psychologists and the lay public that it ultimately led the American Psychological Association to revise its code of ethical principles. (Suter *et al.*, 1989, p. 27)

As we will see in this chapter, the Standard Account, in examples like these, attempts to integrate ethical and historical points. The current DPEs are supposed to be ethically preferable to experiments of the past (especially the Milgram experiments). In the same way, attitudes towards DPEs are supposed to reflect some historical improvement in the way that psychologists view their work and their subjects.

3.2 The Experimental Transition

Some details of this historical picture are depicted accurately enough. Still, until we clarify the general thrust of the Standard Account, it will be difficult to clarify the ethical issues surrounding DPEs. To do this, we might follow most historians who cover this issue, and begin by focusing on the Milgram studies. In doing so, I will show that we can be more critical of our reading of the background and aftermath of these celebrated DPEs.

One way to better understand the context of the Milgram studies and the continuing use of DPEs, is to consider just how deceptive experiments became "experimental" in the first place.

In the early 1960s when Milgram began his series of experiments into what he called "destructive obedience," he operated within a model of experimentation adapted from the early behaviorists. But this model had not long been the dominant one in experimental psychology. Before the behaviorists revolutionized the methodology of psychology (and to some extent the orientation), the dominant laboratory model was based on an introspectionist platform.

Introspectionist experiments involved a subject and a researcher, though not in the way that we are accustomed to thinking of this relationship today. In introspectionist experiments the researcher typically was the subject. These studies, that is, were essentially first-person reports of

mental phenomena. It is not difficult to see why psychologists would have taken this approach.

It was at the turn of this century that psychology emerged as a science, something other than a branch of natural philosophy. Scientific psychology nonetheless retained two philosophical ideas that would play a role in the development of DPEs. Like introspectionist philosophers before them, introspectionist psychologists assumed that a researcher could have direct or immediate access to his or her own mind. This was a scientific version of ideas made popular by empiricist philosophers of the 18th and 19th century, such as Locke, Hume, and Reid. (It is worth noting that most early psychologists were trained as philosophers.) Closely linked to the idea of direct access was the philosopher's assumption about human egalitarianism. Philosophers who saw the mind as a blank slate found no reason to hold that one person's mind was superior. Accordingly, they had no reason to believe that any person was by nature superior.

The introspectionist psychologists adopted a decidedly methodological interpretation of these ideas. But the interpretation that they adopted shaped (and continues to shape) the ethical problems that arise in psychology experiments. Foremost among these problems, of course, are the questions associated with deception. The introspectionists interpreted the idea of egalitarianism to

mean that no person was cognitively superior in the laboratory. Assuming that the proper training was available, which was often quite elaborate (see *e.g.*, Titchener, 1924), subjects and researchers were considered interchangeable and equally valuable.

But before introspectionists were ever really able to test such assumptions in practice, advances in other areas of psychology indicated the benefits of a different approach. Drawing on experimental refinements in animal and comparative psychology, the *behaviorist* psychologists insisted that the proper object of study was not mind or its supposed mental operations. Instead, they looked exclusively to behavior and observable adaptation. First-person accounts of mind, the behaviorists held, were not only subjective by definition, they presupposed the existence of something that was in question, the inner self.

Rather than concentrate on this self or consciousness, behaviorists contended that true experimental psychology should concern itself only with phenomena that researchers could objectively isolate and test (Watson, 1913; Zuriff, 1985). This emphasis cohered with the behaviorist's general distrust of the philosophy of introspection. It also met many of the strictures of the logical positivism that was beginning to shape the physical sciences. Of psychologists, J. B. Watson, in particular, saw the virtue in behavioristic approaches to experimentation. He saw the emphasis on mental

phenomena and laboratory egalitarianism as holdovers from discredited Cartesian thinking that could offer little to the experimental scientist (Dunlap, 1912).

A kind of laboratory separation was the behaviorist's means to obtaining objective, experimental data. Behaviorists placed physical and metaphorical or role distance between the psychologist and the subject. This gave psychologists the ability to go even further. They could now, in effect, distance some aspect of the subjects from the subjects themselves. With this "distancing" the behaviorist psychologists could objectively and scientifically describe aspects of behavior as introspectionists never could (Sarason, 1981).

There was an obvious methodological justification for this separation. For our purposes we should note that it was only possible because of an accompanying shift in the political or social structure in the laboratory. In other words, separation would have accomplished little without stratification. The transition from introspectionism to controlled experimentalism was also "a shift away from the expert observer, toward the manipulated object of observation" (Danziger, 1988, p. 44).

Developments in statistical modeling and prediction made this transition easier. Subjects became subordinate, numerically nondescript objects. These developments in statistics originally came in psychometrics. Eventually,

work in various sub-fields of psychology, including phrenology, advanced the application of mathematical models to the study of humans (O'Donnell, 1985). Under the rapidly developing laboratory model, the subject in non-introspectionist experiments became, one historian notes, a subject-as-object (Schultz, 1969).

No longer were subjects leading figures in the experiment, much less fellow researchers. Psychologists could by about 1950 view subjects as mechanistic "reagents" or physio-chemical organisms, rather than conscious, moral agents. The move from introspectionism to the behaviorist model thereby signalled the combination of old scientific objectives with a special, practical view of the subject.

It is important to note that there were various branches of behaviorism. Some versions of behaviorism placed more emphasis on separation and stratification than others. There also existed non-behaviorist experimental models, including a few that played minor roles in the shaping of early experimental psychology. As far as subject consideration is concerned, however, these other influences were not long-standing.

Kurt Lewin's work in group and social psychology, for example, was, to all appearances, "experimental." But for a few historical events, this work may have become a significant factor in the prevailing laboratory models. But Lewin's approach to research and subjects had little

resemblance to the stricter model of the behaviorists, and it was in the long run nowhere as influential.

The strength of the classical behaviorist influence on psychology was sufficient to cast doubt on all other forms of experimentation. At the very least, the behaviorist shadow tended to restrict discussion of competing forms of experimentation and subject roles to the boundary areas of psychology. In the areas where deceptive methods would soon take hold, experimental psychologists adapted what was generally a behavioristic approach. Many of the psychologists in these areas may have initially rejected much of the scientific platform of the behaviorists; some claims were simply too odd and *ad hoc*, such as Watson's theorized connection between thinking and vocal-cord movement. But experimenters who would dissent from the scientific claims of the behaviorist could appreciate the degree of laboratory control and objectivity that the behaviorist model promised (Hillner, 1984).

Advocates of the new experimental model may not have expected that their new laboratory separation would instead lead to what would turn out to be merely a different kind of subjectivity. The problem was that in separating the subject and the researcher, both became conscious of their status. For the researcher, this meant that observations would have to be made of subjects who were expecting it. These subjects would become, as psychologists soon discovered, eager to

perform as "good" subjects. However the behaviorists viewed the subjects, the subjects themselves tended to behave as specific, thinking individuals while under observation. Enter deception.

By deceiving the subjects, psychologists could maintain a cognitive separation as well as a physical one. Of course, if deception could keep subjects in the dark about their own participation, they might still try to anticipate the psychologist's motives. Only now the subjects would be operating at such a disadvantage that observations of their behavior would still prove to be reliable.

Incidentally, deception critics would soon charge that, from a methodological standpoint, the use of deception reduced the validity of the observations, by making the experiment even more artificial. It may have been true that deception made the experimental setting appear that much more artificial to the subjects, who would still try to respond to their environment. It is nevertheless more accurate to say that deception made a contrived situation a controlled one.

The trade-off between realism and objectivity may have been favorable in any case. Deception gave psychologists an advantage over their subjects that no introspectionist ever had (or perhaps would have wanted) (Scheibe, 1988). As one psychologist noted, "psychologists with access to college students are indeed fortunate. One would be hard put to

think of any other laboratory animal possessing so many advantages" (Farber, 1952, p. 102). It seems that psychologists were only as fortunate where they could use deception to obtain reliable observations from subjects who knew they were being observed and manipulated.

We should take stock of the discussion thus far. We can note that what was in some ways clearly an experimental transition was in other respects a continuation of introspectionist aims. The transition did involve laboratory separation and deception, and in that sense experiments afterwards had little in common with introspectionist studies. But deception was only useful to the extent that it led to objectivity and control. Introspectionists like Wundt and Titchener also realized the value in pursuing and hopefully attaining objectivity (O'Donnell, 1985, p. 19).

When psychologists turned to deception, they could do so because the introspectionists made it a meaningful option. We might also note that the experimental transition established a position on the conflict between scientific and ethical values. Experimental psychologists decided that objectivity was worth the separation that it required, and that deception was not too high a price to pay for experimental success.

3.3 The Psychologist's Ethics Code

Having partially accounted for the *methodological* acceptance of deception, we can now consider the way that psychologists established deception's officially *ethical* status. This bit of the history of DPEs is important, recall, because the Standard Account has psychologists at some point after the Milgram studies deciding that deception is unethical (or sometimes unwarranted), despite its methodological virtues. It is difficult to understand where this change of heart may have come from unless we first examine the psychologist's stance on deception.

The first code of ethics is an obvious place to look for indications of that stance. And since psychologists have occasionally revised their code, we might track the official position on deception and DPEs over the years. The American Psychological Association (APA) suggests such an approach in its first code. By examining that code, the APA remarks, "one should be able to learn much of the character of the profession of psychology." This is presumably because the code of ethics represents an "effort on the part of psychologists to define and give explicit expression of the ethical values which are regarded as important in their professional relationships" (American Psychological Association, 1953, p. *v*).

A code of ethics is a comparatively recent addition to the field of experimental psychology. The APA organized in

1892, roughly the time psychology developed as a field distinct from philosophy and medicine. As late as 1938-40 the APA membership had rejected the idea of a formal code. Approximately ten years later social psychology, where most deception occurs, was becoming popular. This popularity coincided with the aftermath of the Nuremberg War Crimes trials, which made research ethics a topic in its own right. Shortly after the trials physicians busied themselves with drawing up various codes of ethics, and psychologists considered again the development of an official position on ethical issues.

Deception and privacy were the primary concerns of early commentators like Nicholas Hobbs (1948). He and other experimental psychologists raised questions about the unwritten code of ethics that applied at the time. Hobbs noted that this informal code was "tenuous, elusive, and unsatisfactory" (p. 80) regarding deception. The unwritten code left deception to the psychologist's discretion, which meant that there was no general rule about the values that were to guide the psychologist's decision. There was also a concern that experimental psychology was falling behind the times. As Hobbs noted, formal ethics codes were increasingly becoming a part of human-subject research. And by then, Hobbs added, even Funeral Directors and Peanut Butter manufacturers had published professional codes.

Perhaps moved by such remarks, an *ad hoc* Committee of the APA solicited member suggestions for what would be the new code (APA, 1952). Again, deception was a key issue. Referring to the Nuremberg and Helsinki medical codes, Hobbs (now a committee member) argued that psychologists needed "a much more inclusive code" (p. 81) regarding deception and informed consent. Of the suggestions that offered a position on deception, the "Principles of Professional Ethics" (1952) that Cornell University psychologists submitted stand out.

The Cornell code urged psychologists to emulate medical researchers. This code pointed to the balancing act that every ethics code has had to deal with since. It reminded psychologists that "consent to an unknown experience is not regarded as true consent" (Cornell Studies, p. 453). The proposed code of ethics also envisioned experimental psychologists treating people "as individuals, not as subjects to be exploited. . . . tak[ing] every precaution to preserve the security and privacy of the individuals and groups under study" (p. 453).

It is difficult to measure the degree to which such sentiments moved the majority of psychologists. Exploitation was certainly something that the ethics code could not avoid discussing, but there was also a practical necessity. Psychologists simply could not conduct some types of experiments without deception. Accordingly, some skepticism greeted prospective sections of the APA code, draft versions

of which the APA published in the 1950-51 editions of *American Psychologist* (*e.g.*, Hall, 1952; Hobbs, 1959).

The version that the APA finally adopted in 1953 was perhaps a compromise between those who saw deception as generally unethical and those who argued that psychology's future depended on some deceptive experiments. The 1953 edition of the code of ethics left psychologists the option of "withholding information from or giving misinformation to research subjects," where, "in [the psychologist's] judgment this is clearly required by [the] research problem . . . ." (American Psychological Association, 1953, p. 122).

While this position did not rule honesty out of the laboratory, it did set a precedent. Under the new code, psychologists were to calculate the costs of using or not using deception; this meant that honesty was to take on a value measured against such things as progress, knowledge, and the satisfaction of curiosity. In establishing this guideline, the APA provided official status for the subordinate, objective position of the volunteer subject. Whereas experimental results demonstrated deception's utility, the new official position on deception provided ethical legitimacy.

There are probably various reasons that critics of deception, who were typically advocates of stronger informed consent requirements, were generally unsuccessful. The few published criticisms of deception may have appeared to rest

on vague and the relatively novel ideas about subject exploitation, at a time when there was some novelty in serving as a subject. In the early 1950s there was little attention to the subject's proper role. What may have undermined the critic's position the most was the lack of support historical backing for a restriction of deception.

There may, that is, have been too few instances of psychologists or other scientists voluntarily restricting their methods in the name of ethics once the methods in question had achieved methodological acceptance. The anti-vivisection movement makes an interesting contrast here. The increasing restrictions on the use of animals and cadavers in medicine were the result of a genuine movement, with advertisements in popular press and at the national political level (Lederer, 1995). (There might be interesting historical commentary in the fact that DPEs and other areas of experimental psychology have never attracted the concern that vivisection has.)

We now have a wider context for viewing the comparison we discussed in the previous chapter, between DPEs and medical research. The early calls to align psychology's stance on deception with the position taken in medical ethics perhaps never had a chance of success. The presuppositions within experimental medicine, even where researchers use deception, give priority to subject consideration and welfare (see, *e.g.*, Beecher, 1970). As I

mentioned in the previous chapter, one requirement of deceptive medical studies, for example, holds that subjects must benefit directly from their participation.

Differences like this lead some historians of informed consent and deceptive experiments to allege that there probably has never been an adequate basis for an ethical comparison between DPEs and medical research (*e.g.*, Faden & Beauchamp, 1986). It is true that in the 1950s there was no clear, ethical basis for comparison between the two fields. Psychologists had recently distanced themselves from physiology, and were probably no longer content to view their work in the context of medicine, much less therapy. And psychologists concerned about scientific respectability were more inclined to model their research on the physical sciences.

This is not to deny that, as with the experimental transition, the adoption of a formal code might have been more of a turning point in the history of DPEs than it was. The first official code was a positive development in the sense that it subscribed consistent parameters for the interaction between subjects and psychologists. But where deception was concerned these limits were left just vague enough to allow the psychologist a significant advantage over the subject.

It is perhaps interesting to speculate on the possible results of a stronger stance on deception at this early

juncture. I hesitate to take such speculations too far, mainly because I question whether such ideas would in any case be translatable into reliable assessments of the current situation. For instance, the published code of ethics for psychologists formalized the value of deception. This formalization had the effect of making the search for alternative methods for all intents unnecessary, and in the end counterproductive. The psychologist was to deceive only when necessary. The code and the history of DPEs may have made it too easy for psychologists to believe that this necessity was the rule rather than the exception.

There is another aspect of the first ethics code that continues to influence the debate over DPEs. If the promise of more reliable experimental data made it difficult to argue against deception before 1953, the official position further insulated deception. As they do today, critics of deception then had to argue against much more than deception. The critic had to contend with charges that an anti-deception clause would, in the words of one deception advocate "retard the growth of knowledge in areas which . . . give promise of doing more to promote the welfare of human beings than anything invented by psychologists since the days of [psychometric researchers] Binet and Simon" (Krout, 1954, p. 589). It was difficult enough to construct arguments against these values before the publication of the ethics code. With the idea that "a

competent psychologist should be privileged to design his experiment in a way that is most useful to his problem" (Krout, p. 589), arguments over deception took on new significance. Yet, as we will see in the next section, the arguments fell into a fairly predictable form.

3.4 The Milgram Experiments in Context

We have so far interpreted the experimental transition as a move to enhance the psychologist's ability to conduct worthwhile research. We can interpret the adoption of the first ethics code as a statement of the acceptable methods of research. Combining these gives the following interpretation. Neither the experimental transition nor the adoption of the ethics code represented a new position on the values within psychology. Just as the experimental transition was in effect a continuation or refinement of the introspectionist's general objectives, the ethics code was an acknowledgement of the discretion that gave some hope of obtaining those objectives. The psychologist sought scientifically reliable findings about human nature and performance, through the direct observation of human subjects. Within limits, the psychologist was now justified in using deception when observing or manipulating those subjects.

This interpretation provides the appropriate context for gauging the relevance of the Milgram obedience studies

for the history and ethics of DPEs. For a decade, beginning in the early 1960s, Stanley Milgram conducted a series of experiments centering on the attempt to order or coerce one subject to harm another. Milgram's methodology included the use of research assistants working covertly as subjects ("confederates" as they were then called), and realistic-looking "shock generators." Milgram claimed to be interested not in what one person will do to another, but what one can be ordered or influenced to do.

Of the several sources of inspiration that Milgram listed, the Holocaust was most prominent; he took the phrase "destructive obedience" from Nazis who excused their behavior by claiming that they were merely following orders. Milgram also cited the work of Solomon Asch, who conducted experiments where one group tried to influence the perceptual judgment of another group's members.

Two things seemed to bother critics of Milgram's experiments. First, there was the sheer effectiveness of Milgram's deception. Some so believed that they were shocking other subjects that they became agitated to the point of near collapse. Under orders from the researchers, the subjects continued to inflict what they believed were electrical shocks. Second, there was the fact that Milgram would even investigate such behavior. It appeared, that is, that Milgram had arranged for one person to receive orders to harm another. No one, some critics argued, should have to

find out whether he or she would carry out orders like these. Milgram's experiments thus represented an ethical entrapment.

We have to now return to the Standard Account. It claims that the Milgram studies were a turning point in the history of DPEs, and contends that the change was essentially concerned with the psychologist's attitudes towards deception. Yet a striking feature of the Milgram studies is how little changed after ethical questions were asked. It may not be clear how we might measure this heightened awareness about deception. It is clear that there were criticisms of deception at least ten years before Milgram's work. The debate over DPEs fell into the pattern that already existed and that persists today. While there are undoubtedly more discussions on DPEs now, the sheer volume is an inadequate historical (or ethical) measure. More relevant would be the content or spirit of those discussions. And in those terms, there has been little or no change since the 1950s.

In his defense of the obedience studies, Milgram did not invoke any ideas or defend any values that were not current in the early versions of what I described in the previous chapter as the Strong Case for deception. For example, in 1954 Vinacke noted that

> the issue seems to boil down to the question of whether it is more important to avoid deceiving

> anyone, or, in the interests of science, to sacrifice a few people in the ultimate expectation of helping many via the knowledge gained (p. 155)

Milgram assured critics that he provided post-experimental counseling sessions (later to be called "debriefings") for his subjects. (I discuss debriefings in Chapter 6.) The results of psychiatric interviews, Milgram noted, showed no long-term effects from the deception or the participation. After receiving a "friendly reconciliation" (Milgram, 1964, p. 849) with the researchers, most subjects were reportedly "glad to have been in the experiment" (p. 849).

More important, Milgram noted that the mere possibility of negative effects would not have given sufficient reason to avoid conducting the experiments. In this Milgram was not excusing deception at any cost (though in none of his discussions does Milgram seem willing to grant that a psychologist would ever not want to use deception if results were promising). Milgram was instead suggesting that psychologists weigh those costs, and bear in mind that there are countervailing benefits from DPEs, and often costs from not conducting experiments. "The laboratory psychologist," Milgram claimed, "senses his work will lead to human betterment, not only because enlightenment is more dignified than ignorance, but because new knowledge is pregnant with humane consequences" (Milgram, 1964; cf. Milgram, 1977).

There was also the charge that Milgram created a situation where subjects might learn something emotionally disturbing about their own willingness to follow orders. To this, Milgram responded that his critics were only incensed by his findings, not his objectives. Had he discovered that subjects were typically unwilling to punish another person on command, for example, the critics might have welcomed Milgram's work. This is a specious argument, though one that coheres with the terms of the experimental manifesto and the APA position on deception. Milgram agreed that his findings were disturbing. He simply reiterated that

> foreknowledge of results can never be the invariable accompaniment of an experimental probe. Understanding grows because we examine situations in which the end is unknown. An investigator unwilling to accept this degree of risk must give up the idea of scientific inquiry. (Milgram, 1964, p. 849)

Milgram saw his work as falling within APA guidelines for the use of deception. This it probably did. More clearly, Milgram's work was consistent with the scientific values inherent in the experimental transition. The appeal to social and scientific benefits, and a lack of harmful effects, was a natural course for Milgram. The same appeal works for psychologists today, in part because experimental

aims are supposed to be reflected by and justified within the code of ethics.

I suggest that we look instead at the Milgram experiments as significant in another way, for the questions that *might* have been raised and perhaps answered. Some of these questions we canvassed in the preceding chapter. For instance, the critical attention that arose after the experiments provided an opportunity to reassess the accuracy of the practice of determining whether subjects have been harmed by asking them (a curiously introspectionist idea, having subjects report their feelings, after the fact). What evidence is there for believing that this is an acceptable way to quantify the potential harm from deception? To what extent should the ethical issue of DPEs be settled by asking subjects (and psychologists) what they think of deception or even being deceived?

We saw in the previous chapter that a sort of cottage industry exists within the psychologist's literature, surveying the attitudes of deceived subjects and even psychologists themselves. The psychologist's code of ethics relies somewhat on such research (American Psychological Association, 1992; Golann, 1970). Psychologists on either side of the debate apparently place a great deal of weight on these first-person reports (perhaps evidence that introspection is not completely dead). But there has been next to no effort to show exactly how the data is supposed

to support a consistent ethical position on deception or DPEs.

In Milgram's response to one critic, he explained that even he did not know what to expect before his experiments, and that "an investigator unwilling to accept this degree of risk must give up the idea of scientific inquiry" (Milgram, 1964b, p. 849). This simply restated the APA's justified-deception clause, adding a few points about the social utility of his work. But there has yet to be any study of how we might measure the value of DPEs, let alone experimental psychology. Ten years later Milgram was still talking about "the value of inquiry in social psychology, of its potential to enlighten us about human social behavior, and ultimately to benefit us in important ways . . . ." (Milgram, 1977, p. 19). It might not be too late to ask for validation of such claims.

Perhaps to keep things straight, I should emphasize that my interpretation of the history of DPEs differs from the Standard Account on this point. Milgram's work was within APA guidelines. His defense of his work was consistent with values that supported, and still pervade, experimental psychology. In the same way that Milgram's defense would be applicable today, the conduct of his experiments would be within current APA guidelines. Ten years after Milgram's first published obedience study, the APA code of ethics would still speak of what was "by far the

most common reason for limiting information." If subjects were "fully informed about the purpose and procedures of the research and the experiences to be anticipated, valid data could not be used" (American Psychological Association, 1973, p. 27).

The 1992 version of the code maintains this position, with some qualifications. Subjects today, for instance, have more freedom to withdraw from participation than Milgram's subjects had. As we will see in chapters six and seven, it is less than clear what ethical gain this and other qualifications represent. Here we should note that psychologists today may have more difficulty in gaining the Institutional Review Board's acceptance of their work. But no historical evidence links this with Milgram's work or any other DPE. To the extent that Boards make explicit their reasoning, the fear of litigation seems to be a far more common concern than ethical attitudes towards deception.

Since the mid-1960s there have been occasional calls to adopt non-deceptive methods (*e.g.*, Mixon, 1974) or for psychologists to "humanize" their experiments (Severin, 1973). But there has never been a great deal of incentive for psychologists to develop alternative methods or attitudes towards their methods or the subjects. If we judge from the persistence of DPEs, it seems that Milgram-era suggestions about non-deceptive methods like role-playing,

were weak solutions. The may have been solutions proposed to psychologists who were not convinced that a problem existed.

I will close this chapter by suggesting another way to view the final relevance of the Milgram studies. Like the experimental transition and the adoption of the code of ethics, the studies provided an opportunity for psychologists and others to clarify the stance on DPEs. But psychologists instead left the code vague enough to protect subjects from any serious harm and protect experimenters from any serious restrictions.

Chapter Four

Kantian Interpretations of Deceptive Psychology Experiments[7]

4.1 Overview

There are various forms of deception that occur during the psychology experiments that we are concerned with. In some cases the psychologist's "deception" might be more concealment of details about the experiment; in other cases, the psychologist might give knowingly false or misleading claims. We have so far seen that these various forms rest on nearly a century of tradition and carry official sanction from the psychologist's code of ethics.

The positive portrayal that some advocates of deceptive psychology experiments (DPEs) give includes some strong claims about the treatment of the subject as well. One commentator alleges, for example, that

> if the effectiveness of the experiment depends on the subject's being unaware of certain facts or conditions, the psychologist is per force obliged to keep such information from him. The situation, however, is clarified as soon as the conditions of the experiment allow. . . Needless to say, the orientation of the psychologist is basically humanitarian. . . . Avid as the psychologist is

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[7] This essay appeared as "Applying Kant's Ethics to Deceptive Experiments," in the *Southwest Philosophy Review* (1996). That paper was also presented at the 1995 meeting of the Southwestern Philosophical Society.

> for new knowledge, he is unwilling to have others pay the price for its acquisition. (Hoch, 1962, p. 264; cf. Baron, 1981)

Here the argument is supposed to show that expediency and scientific aims render deception innocuous if not "humanitarian." Furthermore, since psychologists must reveal their deception once the experiment is over, there is a view that psychologists never lie to begin with; they merely resort to temporary deception, misinforming, or hoaxing (terms popular in the psychological literature).

Unfortunately, commentators discussing this alleged distinction between the kinds of deception, or the general defense of DPEs, rarely make explicit reference to ethical theory or principle. Occasionally, however, select portions of Kant's ethical theory do find some application in the literature. A few texts cover the problem of deception. Some of these go beyond simply mentioning the familiar applied-ethics triad of Kant, Aristotle, J. S. Mill (*e.g.*, Carroll *et al.*, 1985; Cassell, 1982; Korn, 1984; and Reynolds, 1979).

References to Kant or deontology do surface in these works. Yet commentators who invoke notions central to Kant's ethics, such as dignity, value, and respect, show little interest in applying Kant's broader theory to the problem of DPEs. It is useful to consider one or two aspects of DPEs according to something like Kantian respect. It is more

useful to construct possible interpretations of the DPE according to the main body of Kant's ethics. In this chapter we will consider a few of these interpretations.

4.2 An Application of Kant's Ethical Theory

While I think I can show that there are good reasons to pursue a Kantian application, we should understand at the outset that there are barriers to the task. These barriers in some respects have nothing to do with the nature of DPEs. Kant had, for obvious reasons, no reason to touch on research-ethics. He grudgingly conceded in his *Anthropology* (1974) that empirical observation of humans might have some value. Nevertheless, Kant directs much of the first *Critique* against the idea of empirical psychology.

Not only was Kant skeptical of the "human sciences," he saw ethics as a system of self-imposed, internal constraints. For Kant only *legal* constraints could be externally imposed (see e.g., MoM, AK219-220, 394-395, 418). Accordingly, Kant may have given only a curious glance at the idea of a "Code of Ethics."

As much as we might want to ignore what Kant might say about scientific psychology and the ethical-legal distinction, that will not greatly simplify the application of his ethics. Kant has a frustrating habit of avoiding any direct engagement of the ethical problems that lying occasions. This is a bit odd, since as one Kant scholar

points out, "we do not have to read very extensively in Kant's ethical writings to discover that lying, in one form or another, is taken to represent the epitome of moral wrongness" (Atwell, 1986, p. 193). Yet Kant more often analyzes "false promises" and deliberate instances of misinformation than outright lying.

Aside for a brief discussion in *The Metaphysics of Morals* (pp. AK429-432), Kant only focused on lying in the so-called "Benevolent Lie" (1909) essay. This is more often caricatured than studied, perhaps for good reason. In the essay Kant considers whether a person might ethically lie to save the life of a friend. Kant wrote in response to an article published by contemporary French writer Benjamin Constant, who posed this hypothetical situation. Constant's reference to an unnamed German philosopher struck Kant (with some justification) as a veiled attack on his ethical theory.

Kant's response to this attack does not proceed directly to the question as it has been cast by later commentators. Instead, Kant discusses the *legal* culpability arising from dishonesty (1909, p. 362n). Readers interested in this essay might compare Sullivan (1989), Paton (1953), and Sedgwick (1991). I suspect that overall, the benevolent lie essay will disappointment those who looking to construct a Kantian position on lying. More important for our

purposes, the sentiments that Kant expresses in the essay are especially unhelpful for an application to DPEs.

It seems that such an application should concentrate instead on Kant's general works on ethics. Hence, after noting some cautions, we might look to the best place to start, with Kant's discussion of the Categorical Imperative (CI) in his *Groundwork of the Metaphysics of Morals* (1959). The CI is notoriously easy to apply *as* Kant's ethics, which it is not, and the *Groundwork* is Kant's most accessible work on ethics and his least self-contained. It might be best to focus on the CI and the GRW and use what Kant says in other works to bridge the inevitable gaps.[8]

Kant assumes that we can reduce all free human acts to rule-following. These rules he thinks are based on maxims that the person adopts. Perhaps the best-known version of CI tests these maxims to distinguish ethical actions from unethical ones. Specifically, the CI has me asking if I could in theory base the rule, "I should do *x* to attain *y*," on a "maxim which I can at the same time will that it should become a universal law" (GRW, p. AK422).

According to Kant, I have to be able to express this maxim as broadly as possible, as "I, *and everyone else*, should do *x* to attain *y* . . . ." By doing this I model my

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[8] In citations I will abbreviate Kant's *Metaphysics of Morals* as MoM, his *Lectures on Ethics* as Lect., his *Critique of Practical Reason* as CrPr, and his *Groundwork of the Metaphysics of Morals* as GRW, using Academy pagination wherever appropriate.

proposed moral law on necessary, physical laws. Why would we want to envision moral laws with the same universal scope as physical laws? Because, Kant thinks, only that way can those who would be guided by ethical intentions be able to attribute the degree of applicability or force that the ordinary conception of "ethics" requires.

There are immediate problems with this comparison between physical and moral law. Most of these problems are not crucial to our purposes, though they are significant issues within the broader question of Kantian coherence. I suggest, then, that we can for the moment overlook these problems and see where Kant may offer a plausible explanation of the ways that the CI reveals which actions (or maxims) I cannot rationally conceive as universal laws.

Kant is especially concerned that my intentions and my maxim could conflict, that is, one could undermine the other. "If the maxim of action is not so constituted as to stand the test of being made the form of a natural law in general [*i.e.*, possessing both necessity and universality], it is morally impossible" (CrPr, p. AK70). Kant believes that the ordinary person does not allow for an ethic of exceptions. Our moral duties, Kant thinks, have to apply universally, and regardless of contingencies, if morality itself is to mean anything. Thus, Kant's account of practical reason holds that actions supported by maxims that only apply in special cases or with qualifications are not

within the ordinary conception of ethics. These actions are unethical (GRW, p. AK424-425).

It is notoriously difficult to see what Kant is getting at, until we turn to apply the CI. Kant himself often uses some form of deception to elaborate his CI. In explaining how lying derives from a maxim that no one would rationally will as a universal law, Kant invokes the example of the "deceitful promisor." Here a debt-laden man contemplates borrowing with no intention of repaying the money. After asking himself, "Would I be content that my maxim (of extricating myself from difficulty by a false promise) should hold as a universal law for myself as well as for others?" (GRW, p. AK403), the borrower is supposed to realize that "with such a law there would be no promises at all."

Kant's example is meant to show how a maxim might undermine the institution of promising and possibly trust as well. Kant thinks that any maxim having this effect would prove to be self-defeating since the would-be borrowing depends on promising and trust. Kant thinks that even lies would become conceptually impossible if all promises lost their meaning. Again, by Kant's reasoning, a bare logical impossibility is supposed to reveal that lying is unethical; there is no attention given to, for example, why the person might lie.

From this it might appear that psychologists act unethically when they deceive, simply because deception is supposed to be self-defeating. But this judgment would hardly signal moral closure. A psychologist would understandably be concerned that Kant's CI summarily rejects intentional dishonesty, malicious lying, bluffing, exaggerations, in addition to experimental deception. The psychologist might object that Kant's example only shows that promising or perhaps even borrowing would be threatened by a universal tendency to deceive.

To the psychologist, the Kantian judgment against lies may rest on an unwarranted assumption about communication. Kant holds that "the exchange of our sentiments is the principle factor in social intercourse, and truth must be the guiding principle therein. Without truth, social intercourse and conversation become valueless" (Lect, p. 224; cf. MoM, p. AK429). The liar does hope to gain from such a promise. But Kant is arguing that the liar cannot rationally will a situation where *every* liar would undermine his or her intentions. This claim we might expect psychologists to question as a confusion between logical and empirical details.

After all, Kant claims that no rational person could envision communication functioning where truth or sincerity was completely arbitrary, and that universalized deception would undermine any objective in communicating. But it still

is not clear that these claims, if true, would logically support the rejection of the limited or specialized sort of "communicating" that psychologists use. At issue is the psychologist's attempt to gain something through deception, not the general public's.

The risk here is that our using the CI to test universalized lying will have accomplished little if it only condemns what was never at issue. The debate does not concern universalized lying or the nature of communication. No one argues that psychologists shouldn't lie to each other or that subjects shouldn't lie to psychologists, for example.[9] It might be that a meaningful application of Kant's ethics cannot afford to conceal such details. These details may, that is, give content to the ethical problems associated with DPEs to begin with.

We might defend against this charge of glossing over relevant details if we delve into a most contested technical issue in Kant's ethics: the role of empirical details in practical reasoning. An easy, and fairly common, reading holds that any empirical details or information cloud the pure rationality of Kant's ethics. It is not hard to see where such an interpretation comes from. Commentators often cite passages like this one from the GRW: "even a precept which is in certain respects universal, so far as it leans

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[9] Though the practice of psychologists deceiving their research assistants is not unheard of (Rosenthal, 1966).

in the least on empirical grounds . . . . may be called a practical rule but never a moral law" (p. AK389; cf. AK401).[10]

Yet Kant's warning about "hypothetical imperatives" and empiricism may mislead commentators (*e.g.* GRW, p. 419-420; 441-445). Kant means to emphasize the difference between allowing empirical considerations to dictate moral judgement (which a theory like utilitarianism would under most interpretations do) and allowing them to dictate the moral law (*i.e.*, the CI). Hence, a careful reading of Kant reveals that ethical deliberation *requires* empirical considerations or information. "[Moral] laws require a power of judgment sharpened by experience, partly in order to decide in what cases they apply and partly to procure for them an access to man's will and an impetus to their practice" (GRW, p. AK389).

Kant asks us to consider empirical details in his own examples. He has us envision someone genuinely needing the money and able to obtain it by making a false promise (GRW, p. AK408-411). In explaining his example, Kant makes it clear that the promisor's intended gain is as least as relevant as the fact that the promise is intentionally false. Indeed, one purpose of ethical deliberation and

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[10] For commentators who read in Kant a strong view against empirical details, compare Potter (1975), Nell (1975, pp. 32-42). Singer (1954, esp. pp. 585-586) and Ward (1972, pp. 113-117) offer a fuller discussion on the empirical-details issue than I can provide here.

maxim-testing is to determine which empirical considerations are relevant. Without some attention to empirical details we cannot appreciate the ethical problem.

With this in mind, the psychologist might argue for taking scientific roles and special communication needs into consideration when testing maxims *via* the CI. The psychologist might want the ethical focus placed on a particular group that must successfully deceive another group to accomplish scientific ends. The maxim relevant to that act might sound like this: "I and every other psychologist shall use deception to obtain data from subjects . . . ." This would leave deception as an atypical, though effective, way to communicate with subjects.

One problem with the Kantian idea of communication becoming impossible in a world of universalized deception is that psychologists work within a similar scenario now. Psychologists know that subjects distrust them, even when the experiment does not involve overt deception (Cook *et al.*, 1970; Reynolds, 1979, esp. pp. 147-148). To all appearances, the psychologists and subjects communicate nonetheless. Psychologists who deceive in light of this distrust make more work for themselves. But that doesn't mean that continued deception is inconsistent with the psychologist's objectives.

The conflicting judgments on deception might arise from ambiguities in the maxim-testing version of the CI. Mindful

of Kant's claim that all formulations of the CI are equivalent (GRW, p. 437-438), we might look to another version that is less affected to contentious interpretations of empirical details. Another version of the CI frequently surfaces in applied-ethics discussions under the guise of respect-for-persons (RFP). With the RFP version, Kant places emphasis on what was merely implicit in the first formulation, the idea that any unethical act necessarily involves disrespect. It also makes a new distinction between a person's using someone who can share in a common goal, and using someone who is not able to share, and thereby becomes a mere tool for satisfying the first person's objectives.

Kant thinks that all rational beings are worthy of unconditional respect, primarily because they are capable of deliberately deciding which actions to pursue and which maxims to adopt. We respect others when we "treat humanity, whether in [our] own person or in that of another, always as an end and never as a means only" (GRW, p. AK429; cf. Lect, p. 120). The ability to act rationally, or as Kant notes, "the capacity to set oneself an end--any end whatsoever--is what characterizes humanity (as opposed to animality)" (MoM, p. AK392; Lect, p. 197).

Throughout his ethical works Kant maintains that respect is the *a priori* entitlement of rational beings, with no qualification for the intentions or attitudes of others (*e.g.*, MoM, p. AK435).[11] The entitlement to respect

disregards professional positions or social status as much as any other empirical factors. It is also not important why the psychologist wants to deceive, or whether the deception harms the subject.

This strong view of respect avoids the ambiguities (and the apparent flexibility) of a variable, context-dependent type of respect that would make respect simply one value among others. The psychologist's code of ethics, for example, suggests that scientific priorities may restrict the subject's "self-determination" (American Psychological Association, 1992, p. 1599). In Kantian terms, this means that experimental design and objectives will compete with respect. It means that psychologists may in some cases not be able to show a level of respect higher than the minimum amount of control they want to exert through deception. This makes the level of respect dependent on empirical factors, something Kant would reject.

Kant sees disrespect as an unwarranted form of using another person. But it is not the use of subjects *per se*, even as means to scientific or experimental ends, that Kant would object to. Kant allows a person to respectfully use another. He grants that the act of using others is part of a normal life. The "user" reaches the ethical limit of respect

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[11] Kant may see autonomy as *a priori* and respect as based on this. Thomas Hill (1992) provides an excellent discussion of this relation, though the details may not be important here.

only when he or she uses the other *merely* as a means to an end. This occurs when one person makes it impossible or unlikely that the other could share in the end he or she is being used for.

Psychologists might reach this limit when their deception prevents (or when they mean it to prevent) the subjects from seeking their own ends as the subjects could do if they knew the truth about their participation. For Kant, a diminished capacity for free choice is, in effect, a loss of respect. In particular, respect is respect for freedom of choice. Thus, using turns to disrespect when the psychologist and the subject can no longer share the same end or objective.

This suggests a problem, since deception is only effective when the subject is kept in the dark regarding anything beyond the psychologist's most general ends. So even where subjects volunteer, it can by design never be too clear to them what they are volunteering for. It is this that leads to ethical questions about DPEs.

> Not only will the investigator have expertise and specialized knowledge in the field which the subject does not possess, but he is also operating in a situation that is constructed entirely by him and defined in his own terms. The investigator is the only one who knows the dimensions of the situation, who knows the nature of the business to

be transacted, and the way it is to be transacted. (Kelman, 1972, p. 992)

Any subject who truly shares the psychologist's objective during the research will likely be seen as compromising the experimental manipulation. Psychologists use deception and other means specifically to prevent subjects from knowing too much about the research, such as the objectives or the real reason they are asked to perform various tasks.

Subjects often do share a very general interest in psychology (*e.g.*, Rosenthal, 1966; Rosenthal & Rosnow, 1969; Adair, 1982). But as we have discussed in the two previous chapters, psychologists gain little from volunteers who share too much of the psychologist's ends (or what they believe are the psychologist's ends). And since one justification of deception is that the subject's tendency to guess the psychologist's motives detracts from the validity of the experimental findings, it seems odd to argue that subjects are allowed to seek an end they share with the psychologists.

Psychologists sometimes contend that subjects volunteer out of a desire to advance science. Psychologists also report that other subjects often say they would consent to the kind of experiment the psychologist is considering. But a Kantian interpretation would not allow for psychologists to deceive on these grounds. Kant would allege that

disrespect comes to the subjects because they can only share in the most general or vague research objective.

That these subjects, or similar subjects, might share this end if they knew about it counts for nothing. Kant's ethics leaves no room for anticipating what another person's ends might be. Another person cannot anticipate my thoughts, and ethically act on these anticipations in a way that affects me. This injunction holds even if that person turns out to be a shrewd judge of my character. Rational decision-making requires that I select my own end independent of external forces, and that I abide only by the internal inclination to be moral.

As Kant explains, "to have an end that I have not myself made an end is self-contradictory, an act of freedom that is yet not free" (MoM, p. AK382). For our interpretation, the gist of Kant's remark is that a psychologist cannot set an end for the subject, or try to anticipate the end that the subject might adopt. And where psychologists act on this kind of anticipation they are coercing the subjects. One commentator rightly points out that anticipated consent under deceptive conditions is usually regarded as seduction (O'Neill, 1989).

According to the RFP formulation in Kant's ethics, psychologists could claim to respectfully deceive subjects only by distorting the meaning of deception or respect. The psychologist intends to minimize the subject's ability to

share the research objective. This means that the subject becomes nothing more than means to the psychologist's ends.

4.3 Implicating the Subjects?

Subjects show little concern over deception and they tend to distrust psychologists even during non-deceptive experiments. This leads some commentators to argue that participation in a DPE involves no disrespect or unwarranted using. One commentator alleges, for example, that there is a kind of ethical waiver associated with participation. Subjects are supposed to share in the "the widespread knowledge that one is likely to be fooled when going into a psychology lab" (Dworkin, 1982, p. 252). As a result, subjects who expect deception, and willingly share in the psychologist's aims are supposed to absolve the psychologist.

But this position misrepresents Kant, and far from absolving anyone, may implicate the subjects as well as the psychologists. We have so far viewed the subjects as passively being deceived, when in fact they volunteer to participate. In most cases psychologists will inform the subjects that the experiment may involve some deception. Kant maintains that self-respect and self-control are not capacities rational beings should ever relinquish (MoM, AK pp. 434-437). "[I]n all actions which affect himself a man should so conduct himself that every exercise of his power

is compatible with the fullest employment of them" (Lect, p. 123). It might seem, therefore, that by their participation the subjects are restricting the very capacity that makes their end-seeking behavior possible.

Subjects might knowingly enter into an agreement that makes them the target of deception. Yet on a Kantian interpretation this could be precisely the point *against* such participation. According to Kant's view of respect, subjects who volunteer for experiments they believe may be deceptive may be, in effect, offering themselves as useful research apparatus, and degrading themselves. It makes no difference what the psychologist's intentions and the subject's feelings on deception are. There is no escaping the need for a person to set his or her own ends. From the Kantian perspective, deception and agreeing to be deceived are unethical, since subjects cannot trade their dignity for an interest in research goals.

4.4 Conclusion

Kant's ethics give an interpretation of DPE that shows the interaction between psychologist and subject in a negative light. The strongest interpretation against DPEs arises primarily from the difficulty of conceiving of simultaneous deception and respect. Kant's ethics also appears to reveal that there is at least something problematic about the

subject's willing consent to participate, an issue we will return to in Chapter 7.

There are still reasons to suspect that Kant's ethics leaves important situational details out of the calculation. In that sense, the ethical judgment against DPEs may manifest some of the sins often alleged against Kant's ethics. Nevertheless, one ultimate benefit of appealing to Kant's ethics might be that this application might not require that we stray from ordinary, non-philosophical conceptions of respect and dignity to illustrate where a scientific tradition goes wrong.[12]

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[12] Mary Gregor provided invaluable criticism and encouragement through successive versions of this paper. Her suggestions and her understanding of Kantian ethics also played a guiding role in the final form of this dissertation.

Chapter Five

Rethinking the Risk-Benefit Approach to Justified Deception

5.1 Taking our Bearings

At this midpoint in our discussion it would be helpful to pause long enough to consider where we have been and where we will try to go. We have canvassed some ethical problems associated with deceptive psychology experiments (DPEs). We have also seen where these arise from value judgments that psychologists have historically made within the context of studying human beings. In the last chapter we considered how we might reveal an unethical side to DPEs by appealing to an ethical theory not too far removed from ordinary moral thinking. We have, however, yet to directly criticize the argument that psychologists give to support DPEs. In the first chapter we noted some problem areas in this argument, and in this chapter we are finally ready to address these.

While there are more details in this chapter, there should not be any real surprises. The arguments that psychologists offer for deception are, as we noted in Chapter Three, somewhat predictable or standardized by now. In this chapter I hope to cover both the overall argument for deception and a few of the more important assumptions. These assumptions are play a crucial role in the current acceptance of deception in psychology experiments. Two of the most important assumptions have to do with the

psychologist's ability to assess risks and benefits from deception. To the extent that I can accomplish this, I will clear the way for a deeper exploration of two issues related to the psychologist's argument, the practice of "debriefing" subjects, and the interpretation of subject autonomy in the DPE.

We will discuss post-experimental debriefing in a separate chapter because, in part, one of the problems surrounding debriefing is its exact relation to deception and the deception argument. We will consider autonomy issues separately. They bear on the prospect of constructing a more ethically balanced model of human experimentation, and autonomy is a significant enough value in any such model to warrant separate treatment.

5.2 Overview of the Argument for Justified Deception

With this pause for orientation, we are now ready to rethink the argument for what psychologists present as justified deception. Psychologists value deception because it gives access to "information that would otherwise be unobtainable because of subjects' defensiveness, embarrassment, shame, or fear of reprisal" (Sieber, 1992, p. 64; cf. APA, 1973). From our discussion of the history of DPEs, we know that deception helps psychologists minimize demand characteristics and other unwanted experimental artifacts. Deception thereby contributes to increased

scientific knowledge, as well as personal or professional gain. It is supposed to do this and more without harming anyone. Indeed, the ethical justification of deception is supposed to lie in this equitable trade of harms and rewards.

Admittedly, psychologists are expected to have some reservations, and to use deception only when there is "no other feasible way to obtain the desired information" (Elms, 1982, p. 234). But deception achieves roughly the same condition of cognitive advantage in the laboratory as it does outside the lab. And since some experiments require this advantage, psychologists have good reason to continue deceiving. Thus, psychologists are understandably concerned with justifying their need for, and use of, deceptive methods.

5.3 Risk-Benefit Assessment

In its most basic form, the argument for justified deception appears quite simple. The psychologist presents the case as a tradeoff between needs that scientists and society have for knowledge and prohibitions that exist against deception or manipulation. Psychologists contend that deception is justified when there is a favorable balance between competing ethical and scientific values. In particular, when this balance is favorable to scientific

progress and subject welfare, deception is said to be justified.

We have seen this much in previous chapters. Now we can fill in some of the details of the justified-deception argument. The most common forms of this argument center on the idea of categorizing all values within the experimental scenario as risks or benefits. Psychologists then derive the ethical judgment from the preponderance of benefits. The general thrust of the argument is utilitarianism, though this is rarely made explicit, and references to utilitarian ethical theory are very infrequent. More often, psychologists emphasize a quasi-formal process of Risk-Benefit Analysis (RBA) that operates within utilitarian constraints.

In theory, RBA is supposed to provide a means of redefining anything that has even potential value in the experimental encounter. In this, RBA reflects the value commitments of the psychologists who apply it. Honesty, deception, and experimental goals are all potential values for the RBA to balance. But it is possible that a psychologist might apply RBA without considering any of these values; again, the process is more selective than it is encompassing.

Perhaps because some critics of RBA do not understand how the human element works in the use of RBA, there are often accusations that RBA ignores this or that ethical

consideration. A common complaint, one that arises not only in applied-ethics discussions, likens RBA to a cold, heartless balance-sheet. RBA is criticized for not paying attention to the human side of the issues it quantifies. Critics sometimes complain that RBA "puts a price tag" on life or the environment, for example. When properly used, however, the RBA simply accounts for everything of value to those who apply it.[13] This might include ethical considerations, but it may not cover the considerations that the alternative view deems crucial.

This relates to the justified deception argument in that psychologists choose to assess only risks and benefits, whereas there is reason to wonder if other values that do not easily fit into either category might be relevant. This possibility is central to critical discussion of DPEs. If there are values that the typical application of RBA ignores or minimizes, the focus of the ethical critique might be directed at the RBA itself, but a portion of the criticism must also fall on the psychologists. There are two questions to ask, in other words. We need to know whether RBA is adequate for the job, and whether it is assisting psychologists in a task they should be performing anyway.

In summary, RBA works by translating or re-valuing the often competing ethical and methodological values within the

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[13] Thompson (1980) discusses this and other misunderstandings associated with RBA.

DPE into quantified terms of risk, harm, benefit, and progress. There is an actuarial or administrative strain to this, as there an obvious linkage to utilitarian ethical theory. As psychologists interpret that theory, ethical justification exists wherever RBA shows a net gain from DPEs. There are some restrictions; stricter guidelines apply when deceiving children, for instance. But if benefits outweigh risks to the subjects, or even to ethical principles against deceiving, the justification holds. And because society presumably benefits from increased knowledge, there seems to be a clear instance of ends justifying means. There also appears to be the potential for *greater* risk from a restriction on DPEs.

5.4 Rethinking the Argument and the Concept of Justified Deception

The argument for justified deception deserves a rethinking. The appeal to RBA, and the application it receives, deserve special critical attention. Before getting to that, we first need to address some preliminary questions regarding two crucial concepts, justification and deception. As important as these concepts are to the justification argument, it is surprising how rarely commentators attempt to define them or to show what they would really mean in the overall argument. I will not offer an exhaustive definition or taxonomy here, though I will attempt to define these

concepts in a way that brings out the salient features of the justification argument, and that paves the way for a critique of that argument's reliance on RBA.

5.5 Justification

For our purposes, "justification" will refer to the special ethical exemption that an otherwise prohibited act may receive when pre-arranged conditions exist. There are some general points to note regarding this definition. First, justification is an interactive process. One person seeks to prove to another that the exemption should be granted, or else constructs an argument that would anticipate potential objections. This means that the act of justification will necessarily involve proof that pre-arranged conditions really exist. To take a simple example, we might justify spraying water into a person's house on the basis of an implicit agreement. That agreement might work like this. When evidence of a fire exists, a firefighter (or someone else with a portable water supply) is justified in doing what we would otherwise prohibit.

This example brings out the second general point about justification. The act must be one that would ordinarily be cause for moral condemnation. If, to continue the example, people routinely soaked the inside of their neighbors' houses, there would be no sense in speaking of "justification" for the firefighter's action. There would

perhaps be a need to explain the act, but no cause to talk of sanctioning or justifying it.

How does this apply to the DPE issue? We know that the psychologist's code of ethics grants that deception is *prima facie* unethical. We know that psychologists are to avoid deceptive methods whenever possible. Yet the code also grants (without being terribly explicit) that there are certain conditions that would provide for an exemption. Under these conditions, apparently, psychologists are justified in deceiving.

It is a question too large to address here whether this means that deception can in some circumstances "become" ethical. But the safest course seems would seem to be something like this. Unless we want to say that acts can, in effect, blink back and forth between ethical and non-ethical, we may have to stipulate that an act exempted (*i.e.*, justified) always remains unethical. The existence of agreed-upon conditions does not change the features of the act, after all. What does change is the social permissibility of carrying out the act. In some cases, this permissibility would become an obligation. Psychologists sometimes talk as if they are obligated to deceive. They may invoke a perceived need that society has for knowledge, but we might envision something a bit less contentious, like the surgeon's obligation to cut another person's body.

This means that the psychologist's deception does not become ethical as soon as, for instance, a favorable balance of risks and benefits obtains. Deception remains unethical; there are simply times or situations in which the psychologist can openly deviate from ordinary morality. There are conditions that make the DPE a justified use of deceptive methods. It is now easier to see what the social nature of justification is. The psychologist would by this conception of justification be bound to prove two things. First, psychologists would have to show that conditions were appropriate for exemption. Psychologists would then have to show that the conditions persist throughout the period that deception is used. Deception might seem justified initially, for example, but if predicted or anticipated conditions do not materialize, the ethical exemption "expires."

5.6 Deception

We might at first seem to have more to go on when defining "deception." But the tendency in the literature is to make some very subtle distinctions (*e.g.*, Geller, 1982). These distinctions might be based on intentions. For example, psychologists like to point out that they do not mean for their deception to harm anyone. This was the point of the argument from comparison that we looked at in Chapter Two. There, as we saw, the psychologist argues that the subject is better off being deceived in the laboratory than

on the used-car lot. In this light, it is interesting that intention-based ethical theories (*e.g.*, deontology) are typically unwelcome in discussions on DPEs. Still, regardless of the theoretical backing, psychologists are apt to distinguish what they do from what the liar does by an appeal to their beneficent intentions.

Distinctions are more commonly drawn based on the consequences of the deception. The psychologist might allege that truly justified deception leaves no emotional or physical trace on the subject after the experiment. The code of ethics also mentions the need to predict consequences, again giving prominence to the likelihood that subjects will experience undue stress. Since the code leaves unstated what would amount to excessive stress, the psychologist is able to point to the expectation of positive consequences in order to show that some level of stress is acceptable.

In summary, the "deception" that the psychologist sees as problematic, that is worth debating at all, manifests in methods or procedures that leave the subject at a temporary and relatively secure cognitive disadvantage. Anything beyond this is supposed to be outright lying. Because the code of ethics assumes that psychologists would avoid that, lying is rarely the focus of any ethical discussion, just as "lying" is not used in the literature.

The point in wrangling over these distinctions is that a recurring theme in the literature on DPEs suggests that

closure will come from criticizing only *some* forms of deception. Presumably, these are the justified forms. "It would be unfortunate," Sieber, et. al (1995) claim,

> if criticism of deception methods caused psychologists to stop studying some important phenomena. A more desirable outcome of criticism is for researchers to employ the least harmful or wrongful method of deception that yields valid, nontrivial knowledge. (p. 75)

Yet commentators fail to offer more than a stipulated distinction between the forms of deception. What is lacking in such references to a taxonomy of deception is the moral basis of a distinction between lying and "hoaxing," for example (Sieber, 1982).

I admit to the temptation to embrace the idea of criticizing only the extreme varieties of deception (and labelling these "unjustified"). I think it is still preferable to temper that urge with an acknowledgment that lack a general rule telling us which deceptions are extreme. In the absence of rules or definitions that will satisfy everyone, I suggest that we aim to err on the side of being conservative regarding subject welfare. This way we might avoid defining "deception" in a way that only secures the psychologist's advantage or restates ethical assumptions that psychologists have not substantiated.

In this discussion, I will consider as "deceptive" any element that the psychologist introduces, or allows to remain in the experiment. I will conceive of deception in a way that makes necessary reference to some information or lack of information that intentionally affects the subject's capacity for autonomous choice. This definition begs some important questions about autonomy, and I will turn to this issue in chapter seven; for now, these questions are not crucial to the justification argument.

This way of defining deception cuts a broad swath across the concept of deception. But that should make it easier to draw some general conclusions about the issue of justification, and to see the relationship between the two concepts, justification and deception. Armed with these preliminary definitions, we can now turn to the key elements of the justification argument itself.

5.7 The Question of Risk

In Chapter Two I posed a few questions about DPEs that commentators in applied ethics might concern themselves with. I will now elaborate on these, paying much more attention to potential responses that psychologists might offer.

One important question gets at the negative side of the justification argument. What has to be justified, that is, are any risks and deviations from ordinary morality, from

the things that psychologists would normally be allowed to do. The justified-deception argument only works if psychologists can truly balance risks and benefits beyond a metaphorical sense.

An important factor in accomplishing that task is the measurement and quantification of risks from deception. There is, however, a difficulty in evaluating these in a way appropriate for RBA. Studies provide apparent evidence that deception does not harm or even bother subjects (Diener and Crandall, 1978; Reynolds, 1979). I will not address the issue of the validity of the findings from these studies, except to point out some questions that psychologists might answer. These questions also confront those critics who would meet the psychologist on utilitarian terms. That is, the critics who argue against deception by showing that, contrary to the prevailing claims, measurable risks from deception outweigh measurable benefits.

One question we might ask concerns the appropriateness of the empirical, utilitarian approach. It is one thing, after all, to debate a psychologist's assessment of risks and benefits, and another to explore the idea of applying an RBA. This question doesn't side-step empirical details, it simply asks what we would have to know to apply these or any other kind of details to RBA or utilitarianism. Hence, this is not a question of any given evidence, so much as a discussion of the type of evidence that would be required to

secure justification. RBA requires that we stipulate what we will accept as evidence. The challenge is therefore to show that such stipulations have a solid foundation, that they will get at the ethical issues involved, and that RBA is the best tool for the job.

One commentator critical of RBA points to the difficulty in showing this.

> Which elements are recorded in the cost-benefit ledger as benefits or costs for the persons involved and show the contract to be an overall profit or loss? The psychology of the psychological experiment has provided us with little that is definite. The subject motives explored have been primarily those that explain the subjects' behavior *in* the experimental situation. Motives that would explain the subjects' participation in the experiment or their evaluation of the situation are given little attention, and even less information is available about the experimenter. Thus, if we are to enumerate tentatively the costs and benefits of the experiment, intuition and nonscientific experience, introspection and speculation must supplement the scanty hard empirical data that the scientific community has agreed to consider more valuable. (Schuler, 1982, p. 49)

On hearing such criticism, psychologists tend to get unnecessarily defensive about their version of the risks of deception. This may be because such comments get at the psychologist's ability to measure these risks. Suppose, the psychologist might argue, that the concept of "risk" is vague, and in the case of DPEs, arrived at by fiat. Isn't the deception critic in the same bind when trying to show the risk in deceiving? Doesn't the vagueness infect the critic's position?

From our definition of justification, we can see where this response misses the point. The psychologist's code of ethics concedes that deception is generally wrong; the moral status of deception is therefore not in question. To the extent that psychologists abide by the code of ethics, they acknowledge a *prima facie* position on the wrongness of deception. This means that the psychologist who wants to show that deception is justified assumes the burden of explaining which factors will apply to the justification.

To do this, the psychologist has to give a compelling account of the basic parameters, such as risk, that are to count in the justification of DPEs. Since the psychologist typically looks to a utilitarian, empirical argument to demonstrate justification, the critic is within safe bounds in asking about the parameters of risk. The psychologist, not the critic, has to offer proof of being able to define

and measure, or at least predict, the negative consequences of deception.

There are some fairly obvious risks associated with DPEs. Physical or psychological harm are immediate concerns. Of course, on this score psychologists have what purports to be empirical evidence that deception does not place the subjects at undue risk. But there remain questions about this evidence. First, it is not clear that psychologists can devise a test that will get at the effects of deception. In part, this is because some of the negative features of deception may not lend themselves to empirical measurement. The research into subject attitudes focuses on subjects who report their feelings about the deception that they, in some sense , expected. This context makes it difficult to know what would be measured and what would be measurable.

Lost in the debate over the psychologist's prowess in measuring "harm" is the possibility that psychologists have defined harm so that it is *only* an empirically verifiable phenomenon. Exhaustive quantifications of potential harm from deception are unlikely. This is not because of some defect in psychological explanation, but the challenge in even knowing where to begin to formulate a conclusive theory of how deception might affect subjects or researchers.

In addition, as Schuler points out above, knowledge of the influences within the experimental situation is sketchy. Our perception of the risk and the tendency of those who

evaluate it to be selective about what gets quantified remains limited at best. I don't mean to suggest that such issues make the justification impossible. But problems like this would have to limit the psychologist's ability to make a convincing case based on RBA.

The second question concerning risk is whether some not so obvious risks might be relevant to the justification of DPEs. We can take a wide view of risks. This view might include some potentially negative results of deception, such as a loss of autonomy (Kupfer (1990), self-esteem (Baumrind, 1964; 1985), and respect for honesty and institutions (Eisner, 1977; Kelman, 1968; Schultz, 1969). These seem reasonably conceptions of risk, and they also seem especially resistant to empirical test.

That may provide, however, all the more reason to consider such risks. As psychologists apply RBA it probably remains blind to such effects. Consider a few ways that we might improvise on the traditional conceptions of "risk." There might be an interesting sort of risk in the prevalence of psychology experiments in higher education. DPEs may be one of the few places in education, or science for that matter, where deception occurs openly.[14] DPEs are

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[14] Of course, we have seen that there are clinical medical studies where student-subjects might expect to be deceived. But this is not the place to reopen the question of a comparison between the two types.

incongruous with the emphasis on ethics and accountability elsewhere on campus, as in the business and life-sciences.

Accordingly, there may be a risk of sending students a distorted ethical message, in presenting deception as the cost of doing business in some areas of psychology. Psychologists are right to point out that the laboratory won't be anyone's first exposure to deception. But that may be beside the point. The pervasiveness of deception outside the lab might simply provide additional reason for not increasing the amount of deception in psychology. And it might also highlight the special risk in attempting to justify deception in the name of scientific inquiry.

If I appear to dwell on this point, it is because I am assuming that while some risks may defy measurement, others probably aren't worth taking. Some risks are inconsistent with broader objectives. For instance, with so much uncertainty about risk, and the harm that might arise from DPEs, the RBA might require that we ascertain whether students might more easily learn about science and the humanities without deception. When student-subjects become psychologists they may face the justification issue in their own experiments.

As psychologists, these former subjects will rely on a trusting public for financial support. This reliance, along with professional norms, may leave the student-subjects with limited resources or incentive for ethical concerns.

Psychologists may therefore be taking unnecessary risks when they do not fully address deception while students can still evaluate their options objectively.

Practical constraints have a way of reducing these and other conflicts to means and ends. Experienced psychologists have yet to resolve the conflicts regarding deception. There may be another special risk in expecting that students will do any better, and in believing that they should have to. Whatever harms deception might involve, empirical or otherwise, the connection to pedagogy and the status as a scientific rite-of-passage for impressionable students are not marks in its favor.

I grant that empirically minded psychologists have reason to remain skeptical of any ethical criticism that seems to be based on allegedly unmeasurable risk. Yet specific problems with these suggested forms of risk would not warrant dismissing the general concern or restricting the matter to empiricism. On the contrary, I hope to prove that according to the terms of RBA, such a dismissal would be unethical and inconsistent.

The strength of any RBA-based argument is determined by its ability to show that all relevant values have been considered. It is also important to be able to show that all ethical considerations within those broad value categories have been addressed. It seems that in this case, a tendency to ignore potential risks could in some ways lead to the

most serious error, the risk of accepting as harmful only what is measurable.

In summary, without being able to answer these questions about risks and harms, the psychologist cannot claim to be able to balance risks with benefits. In terms of justification, one necessary condition seems to be the lack of harm, or the absence of significant risk. But psychologists have yet to demonstrate their ability to fully stipulate what would be meaningful conditions of this kind, and that undermines the possibility of an exemption for deception.

5.8 The Question of Benefit

The other side of the justification argument concerns the benefits of deception. On this issue I will not argue exclusively that psychologists are wrong in what they describe as a "benefit," though there are some cases where this seems obviously true. I will expand my comments to include questions about which benefits are to count in the RBA, and whether this tool and the application are appropriate.

Clarity might seem less important when considering the benefit side of the RBA argument. It might appear that protecting subjects from harm carries more ethical necessity. Yet neither RBA nor utilitarianism support this priority. The RBA requires that real benefits follow from

DPEs. The justification argument relies on the psychologist's ability to weigh these benefits. This means that psychologists have to demonstrate their ability to quantify the relevant benefits *via* RBA.

As with risk, however, there is no science that psychologists might turn to when determining what would qualify as a benefit or who should receive it. Assuming this determination is possible, it will instead require careful attention to the social setting of the experiment, the values that subjects hold, and other factors.

If there are any benefits associated with DPEs, it seems reasonable to assume that they will have to result from some feature of the way the psychologist conducts the experiment or from the subject's participation. But as some methodological critiques applicable to DPEs suggest, these experiments are scientifically bankrupt, and they cannot in principle provide any reliable data. On the terms of these critiques, DPEs are more than a mere waste of time and resources, they are unethical according to the RBA, which requires that psychologists have access to real benefits for quantification.

Particularly susceptible to this line of criticism are DPEs that rely on what psychologists call the *null-hypothesis test.*[15] Critics (*e.g.*, Meehl, 1967) allege that

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[15] The argument that I give in this section reflects claims in my "An Ethical Argument Against Leaving Psychologists to their Statistical Devices" (*Journal of Psychology*, 1996). I

this test too often shores up what are really insignificant or inconclusive experimental data. The problem begins with the traditional approach to confirming a theory. Instead of directly testing a substantive hypothesis that would provide explanatory or predictive power, psychologists test an alternative or null hypothesis. The null is a provisional, or we might say, "sacrificial" hypothesis. It holds that no significant relation exists between the two variables that the substantive theory attempts to explain. The real hypothesis, the one the researcher hopes to confirm, asserts the opposite, that a relation does exist between the variables. But this hypothesis is not tested.

Since it is conceivable that there is *a* relation between nearly any two variables, critics point out that few psychologists could believe what the null hypothesis asserts (Meehl, 1967, p. 109-110). This makes the attempt to disprove the null look dubious. It also undermines the belief that disproving the null will confirm the alternative. What happens, then, is that psychologists wrongly take rejection of the null hypothesis as confirmation of the real hypothesis. They assume, in other words, that data that would appear to rule out a no-relation condition must thereby prove at least a minimal relation.

Of course, proof that one hypothesis is false does not prove that an alternative is true. An appearance of

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thank James Hawthorne for helpful suggestions on that paper.

statistical precision tempts psychologists to overlook the fact that the null-hypothesis test can provide tentative confirmations at best (Bakan, 1967). Psychologists can compound the problem by making claims about the underlying theory on the basis of an apparent confirmation. With the null-hypothesis test, psychologists can draw conclusions about an underlying, substantive theory even when they have only "confirmed" a vacuous or even false substantive theory.

Given these logical and methodological flaws, it may be impossible to assess the validity of experimental findings, and this undermines the possibility of the psychologist's demonstrating real benefits from DPEs (Meehl, 1991). In terms of the RBA, the methodological shortcomings of the null-hypothesis test cast doubt on the psychologist's ability to balance any type of risk with meaningful benefits. This makes it doubtful that the psychologist will be able to meet even minimal conditions under which deception would be justified, according to the psychologist's own utilitarian argument.[16]

Even if we set aside methodological doubts, skepticism should still extend to the claims that subjects are the

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[16] There is, of course, a much older and broader critical view, that psychologists do not appreciate the philosophical limits of experimentation and explanation. In particular, this line of criticism holds that psychologists misrepresent their ability to relate scientific explanation to the human subject (*e.g.*, Deese, 1985; Kline, 1988). Because these methodological critiques are more suggestive than conclusive for the issue of justification, I will not address them here.

primary beneficiaries of DPEs, and that the benefits are tangible. Some of the more specious benefits include a "balanced and interesting summary of relevant knowledge at the time of the participation," "a handout that is carefully edited, clear, simple, and devoid of professional jargon," or "a cheerful and friendly offer to discuss any of the material" (Sieber, 1992, p. 101).

There is also a conceptual difficulty in showing how the subject might derive any benefit from having been deceived. Most potential benefits could be appropriately delivered through participation in non-deceptive experiments, or without the person having to participate as a subject. And in many instances, what are supposed to be benefits are defined in ways that presuppose the value of DPEs, or are presented in ways that would only make sense where deception was literally necessary.

For example, some commentators suggest that during the post-experimental debriefing the psychologist benefits the subjects by teaching them about research (Holmes, 1976a, 1976b; Smith & Richardson, 1983; Marans, 1988), instilling a positive attitude towards experiments and experimenters (Aronson & Carlsmith, 1968; Elms, 1982), and generally repaying them for participating.[17] There is a sense in which it is hard to argue against any proposed educational

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[17] Debriefing plays an obvious (and official) role in the justification, but I will defer attention to such issues until Chapter Seven.

benefits. What could be more obviously justified than a procedure that teaches future psychologists about experimental psychology? Yet the RBA requires that educational benefits, like any others, must be *ethically* relevant to the subject and the RBA.

This means that regardless of how much subjects learn from their participation, psychology professors could provide these educational benefits more efficiently, or at a lower "cost." The psychologist's academic role ensures that educational experiences are neither extraordinary nor something that subjects should have to pay for by being deceived.

Again, most of the questions we might ask about benefits from DPE take a similar direction. We have to ask what makes it is that makes the benefits something that subjects would want, something they could not obtain at a lower cost, and how these benefits relate to the DPE. As with the questions about risk, questions like this about benefit do not suggest that psychologists are unscientific or naive in any intellectual sense. These questions suggest rather that the psychologist's faith in RBA is misplaced.

If psychologists cannot demonstrate the value and prospect of benefits, they are on weak grounds when contending that certain qualifying conditions are met for the purposes of justification. Psychologists might avoid some of these concerns by focusing less on the idea of

balancing these benefits against risks. The possibility that there may be no positive results from DPEs will probably still affect most arguments the psychologists could give.

5.9 Conclusion

In closing, I should note that there are probably as many arguments for deception as there are psychologists who use it. I have examined one prominent argument that asserts that deception is justified by a favorable balance of risks and benefits. There is nothing inherently wrong with this version of utilitarianism or the preference for weighing risks against benefits. We might remember, however, that the RBA that psychologists appeal to is only one tool among many for evaluating deception in psychology experiments, as utilitarianism is only one ethical theory.

If utilitarianism is to be the preferred mode of assessment, and it appears that it will be until psychologists reform their roles, we must continue to ask how well anyone understands the benefit (and risk) associated with DPEs. An understanding of these parameters should precede the decision to apply RBA or any other form of justification. Where we do not understand how participation in DPEs may affect subjects, and how the continuing use of DPES might affect society, we run a real risk of only partially applying the RBA. There could also be an appearance of deciding at the outset to use deception and

attaining "justification" by adjusting the meanings of risk and benefit.

Critics of deception often seem hobbled by the need to show that subjects are actually harmed by deception, or that DPEs represent substantial threats to the stability of Western Society. This is raising the stakes too high. All the critic has to do is to show that deception is unjustified. As I have explained in this chapter, showing that much only requires that we show that psychologists have failed to state acceptable conditions for ethical exemption. Where such exemption is lacking, the psychologist has failed to meet even those conditions that we might grant with qualification.

Chapter Six

The Deception-Debriefing Package

6.1 Overview

"Deception," Alan Kimmel (1979) assures us, "was once accepted without comment, but it is now accompanied by elaborate justifications and extensive debriefing procedures" (p. 634). Kimmel, an experimental psychologist, expresses a view shared by many commentators. He alleges that the post-experimental debriefing sessions that typically conclude deceptive psychology experiments (DPEs) somehow make (or keep) such experiments ethical. Sentiment about debriefing also seems to influence the psychologist's justified-deception argument, as Kimmel's remark suggests.

Although the term *debriefing* does not occur in any code of ethics, the American Psychological Association (APA) clearly endorses what I will call a deception-debriefing package. The 1973 version of the APA code of ethics stipulates that

> after the data are collected, ethical practice requires the investigator to provide the participant with a full clarification of the nature of the study and to remove any misconceptions . . . . Where scientific or humane values justify delaying or withholding information, the investigator acquires a special responsibility to assure that there are no

damaging consequences for the participant.

(American Psychological Association, 1973, p. 77)

The current ethics code requires that "any . . . deception that is an integral feature of the design and conduct of the experiment must be explained to participants as soon as is feasible. . . ." (APA, 1992, p. 1609).

I will in this chapter examine this requirement, and some underlying ethical questions concerning debriefing. To clarify the scope of my criticism, I should reiterate that most psychologists do not deceive subjects. Many psychologists do not even use human subjects. Still, enough either use deception or defend it that there is a need for us to re-think the claims surrounding debriefing and the deception-debriefing package. We need especially to examine the presumed ethical qualities of the debriefing component.

Harris (1988) and Sieber (1992) provide detailed accounts of what actually occurs during a debriefing. Such work is valuable in the sense that published research reports rarely include details about the debriefing of subjects. But there is also a need to treat debriefing in the abstract, as least as much as this is possible. I am particularly interested in examining the idea that there is something about a debriefing that affects the ethical quality of DPEs. Not only the APA code of ethics, but some psychologists, suggest that debriefing does this. As we will

see, however, it is less than clear how that is supposed to work.

We will also in this chapter examine the deception-debriefing package in light of our previous discussion of the argument for justified deception. At the most basic level, then, in this chapter we will explore the following questions. How might the debriefing session work? What moral relation does debriefing have to deception? And finally, stating the problem broadly, what bearing does the deception-debriefing package have on the justifiable use of human subjects, or on the public portrayal of that justification?

6.2 Interpreting the Deception-Debriefing Package

One reason for treating such questions in the abstract is that the literature contains few substantive details that might provide answers. We are left to pursue interpretations. But we have regrettably limited evidence from commentators and the code of ethics. Under one interpretation debriefing might exert some kind of ethical force if it could draw on, or apply, fundamental ethical principles like autonomy or honesty. In a similar way, debriefing might exert the ethical force by counteracting or combining with some inherent ethical quality of experimental deception. As two methodological aspects of DPEs, deception and debriefing would presumably be linked, perhaps as

promising and promise-keeping are. Under an interpretation only slightly different, it might be that *deception* is what becomes ethical (or at least more ethical) after a debriefing.

There seems to be some adherence to each of these interpretations. This stands to reason: for the deception-debriefing package to make any sense at all, it would have to satisfy some elements of these interpretations. Yet it is again not clear how it might consistently to do that. In any event, these interpretations seem to describe what commentators and experimental psychologists who deceive and debrief would very nearly have to believe. But rather than clarify the ethical situation, each interpretation unfortunately returns us to questions about what it is about debriefing that enables such a transformation.

Of course, there may be no explicit understanding of the ways or whys of the deception-debriefing package. Debriefing and deception may be imbedded in the experimental culture without any individual being able to give specific reasons for keeping them within the culture. Such a scenario might not leave the experimental psychologists with nothing to go on. Psychologists could rely on an intuitive sense that, for example, where we have deception we should have debriefing, in the name of ethics. Supposing that is the case, we at least have an intuition to examine. We can attempt to show how this intuition might cohere with other

intuitions regarding DPEs and the justified-deception argument. There remains a need to explain, that is, why psychologists and the APA would see (or feel) a need to debrief, or alternatively, why anyone would promote the deception-debriefing package.

6.3 Potential Ethical Bases for the Deception-Debriefing Package

Here is a simplified interpretation that experimental psychologists might offer on the deception-debriefing package. Psychologists incur obligations merely from the fact that they are imposing their objectives on the subjects. Hence, after a deceptive experiment psychologists have an obligation to reassure the subjects that they have been deceived for good reason and that they haven't wasted their time. It is unlikely that psychologists would have access to the subjects at any other time when they might take care of these responsibilities.

During the debriefing psychologists can also answer any questions about the true nature of the research. They can clarify details that might previously have affected the subjects' behavior if the subjects had known of them. This seems to be what the APA is getting at in the excerpt above. There is no denying that subjects do make scientific progress possible, even where their participation results in no scientific benefits. Therefore, it is reasonable to

imagine that psychologists might see in the debriefing a chance to thank the subjects.

Indeed, this show of gratitude would be a reasonable expectation for having served in a *non-deceptive* experiment. The expectation is only stronger where deception is involved. While subjects typically receive academic credit and first-hand knowledge of experimental psychology, their participation might be an imposition nonetheless. The imposition might be acute in deceptive or potentially stressful contexts. Psychologists might interpret a corollary to this scenario of reciprocity. Perhaps the degree of deception determines the degree of obligation to debrief. The more psychologists mislead the subjects, the greater the need to clarify after the experiment. In summary, this interpretation has the psychologist's obligation to debrief resting on a number of ordinary beliefs about social interaction; it suggests that debriefing is the ethical thing to do after deceiving subjects.

A critic of deception, however, is unlikely to be satisfied with this. For one thing, there is more than one way to explicate this intuitive sense of obligation, if that is what this interpretation expresses. Secondly, this interpretation seems to involve several deep-seated, possibly unresolved, assumptions about deception. It may also be making an unwarranted appeal to the idea of a

debriefing as a way to avoid fully cashing out these assumptions. Finally, psychologists thinking along the lines of this interpretation face the challenge of reconciling the ordinary ethical concept of obligation with what actually makes a debriefing necessary.

We will consider a few of these points. But first notice that these issues raise further, more explicit questions. The psychologist might, for example, sense an obligation to share some insights about psychology after the experiment. Yet in what sense could we derive an ethical obligation from this feeling? Do such intuitions provide a basis for alleging that a psychologist who chose not debrief was acting unethically, rather than merely rude or impersonal? Would it make sense, in other words, to criticize a psychologist for not debriefing?

These questions seem to strike a common chord, that debriefing caters to some natural inclination or intuition regarding the proper treatment of another person. The intuitive appeal that such questions seem to reflect may come from deeper notions, for example, that it is better to be lied to and later learn the truth than to remain deceived. Such intuitions are hard in some cases to argue against, but in this application it is hard to reconcile these with what really takes place during and after the DPE.

6.4 Debriefing as Corrective

In an effort to sort out these questions, we can now turn to specific issues associated with a common interpretation of the deception-debriefing package. It seems that, at a minimum, psychologists must present debriefing as some type of corrective or curative. This gives rise to portrayals of debriefing as an ethical response or preventative to the potentially damaging effects and moral prohibitions against deception. Proposed variations on the debriefing theme, like the "dehoaxings," "post-experimental explanations," or "desensitizings" that some psychologists advocate seem to express this view (Holmes, 1976a & 1976b; Marans, 1988).

This view holds that the broadly conceived practice of debriefing remedies something that psychologists have done to subjects through deception. And it is presumably in reference to some obligation that such adaptations to the basic debriefing are justified. In short, deception on this account is supposed to be somewhat wrong. But debriefing and its variants are alleged to play a positive, transforming role for the subjects, psychologists, and the ethical scenario.

This interpretation runs into problems with the moral language it seems to trade on. As the term *obligation* typically applies in ethical, political, and legal discussions, it entails an accompanying right. In this case,

that right seems to be one held by the subjects. If the psychologist has a real obligation to debrief, it is not immediately clear what right the subject might invoke. It does not seem as if the use of debriefing could reflect an intuitively sensed right to self-determination or the truth. The debriefings that we are discussing are contingent on there having been intentional deception. We might suppose that the subject partially forfeits such a right in consenting to participate (an act we consider in the next chapter). But we then have fewer reasons to believe that something is ethical about the psychologist's debriefing afterwards. Here it becomes ethically relevant that subjects consent with the knowledge that deception may occur.

What of the possibility of compensating the subjects or paying them back through a debriefing? Some interpretive options in this direction seem to be ruled out from the start. No psychologist has the power to return the subject's world to the way it was before he or she signed the informed-consent form. The psychologist can, to be sure, turn back the clock in a metaphorical sense to set things right. But establishing debriefing's ethical status will undoubtedly involve more than the expectation that a debriefing is what separates a deceived subject from a non-deceived one (cf. Tesch, 1977).

Psychologists might feel that debriefings offer subjects tangible compensation. But what kind of

compensation, and in response to what specific infraction or wrong? Not only is it unclear that psychologists have anything that might compensate the subjects, it is hard to understand what they are compensating for. It seems that psychologists might want to compensate the subjects for having, temporarily, misled them. But no psychologist explains how a debriefing compensates subjects for having been gullible, albeit willfully so.

Furthermore, as we saw in our discussion of the justified-deception argument, there is the possibility that some DPEs may provide only limited benefits to anyone, psychologists or subjects. A related possibility exists, that the psychologist attempts to define "benefit" so that it will serve easily in the justification of deception. The point of these concerns about compensation and benefit is that psychologists might make a stronger case for the deception-debriefing package if they would offer evidence of the positive features of a DPE. From this evidence, psychologists might further specify which features directly compensate the subjects, and which features are more arbitrary and unrelated to deception, if that is in fact what is being compensated for. Here again the question arises, whether subjects, mainly undergraduate psychology majors, could receive the benefits that we are now grouping under the category of "compensation" without having been deceived.

6.5 Debriefing as Reconciliation

At this point, the psychologist might offer the following, revised interpretation. Debriefing does, retroactively, make the deception ethical, but it does not operate in any metaphysical sense. Debriefing is simply a response based on principal, and it need not rely on any assumptions about there having been harm. The psychologists may sense only a symbolic obligation without undermining the important social function that debriefing serves. After all, psychologists cannot simply turn out the subjects without first "coming clean." The subject expects to be treated with dignity and respect, and debriefing upholds this expectation.

In advocating this interpretation, the psychologist might have to concede a few negative points about deception. First, there seems to be something objectionable about deception. Second, those who deceive seem to incur an obligation not only to confess later, but to smooth over any negative effects. The best way to meet this obligation would perhaps be to offer a sort of apology or attempt at reconciliation. According to this interpretation, debriefing would still perform a transforming function, "curing" the subject of any harmful effects and making the deception and the experimental participation less objectionable, but it would not have to show precisely what it was that psychologists took from subjects or what they returned.

Indeed, the psychologist might allege, we typically think in these vague terms when referring to ordinary apologies. There we rarely cite metaphysics or overly complex ethical theories. On the contrary, if when I try to apologize, you point out that nothing can literally undo what I have done, we lose an important facet of moral life. By the same token, a debriefing does not have psychologists trying to reverse time or duck the fact that they set actions into motion that led to stress or a violation of rights. Instead, debriefing serves as a special form of restitution, perhaps similar to Affirmative Action hiring programs, civil court settlements, and again, ordinary apologies.

Within these examples of restitution, there remains a common idea of compensating for inflicted wrongs within parameters that both sides agree on. There is also a presumption of justice or fairness. There is a problem, however, with this interpretation, and it issues from what the psychologist is apparently wanting to assume. The problematic linkage between deception and debriefing remain. For instance, the psychologist contends that deception is justified, in part because it isn't harmful and also because it makes scientific benefits or progress possible. This is the by now familiar Strong Case for justified deception:

> a potential subject is in far less physical danger during virtually any kind of research

> participation than in driving across town to an experimental session, or in spending the research hour playing tennis instead. Psychologically, as researchers have often pointed out to institutional review boards, the principle danger to the typical subject is boredom. The individual is at much greater psychological risk in deciding to get married, to have a baby, or to enroll as a college student, all activities typically entered without informed consent, than in participating in practically any academic research study ever carried out by a social scientist. (Elms, 1982, p. 237; cf. Reynolds, 1972)

If for the purposes of this discussion we suppose that psychologists can measure and compare harms as this passage implies, the interrelation between the arguments for deception and debriefing still causes problems.

On one hand, the more psychologists claim that debriefing is a necessary part of an ethical experiment, and talk about obligations or restitution, the more suspicion they create about their claims that deception is harmless. Unless *something* is wrong with deception, why view debriefing as response or restitution for it? If psychologists haven't identified or even caused any harm, there should be nothing for debriefing to respond to. There should be nothing for the psychologist to mend. Far from

being necessary, debriefing would be either superogatory or superfluous in an experiment that involved truly innocuous deception.

On the other hand, psychologists would have an ethical basis for response or restitution if deception presented even minimal harm, but only at the cost of reopening unanswered questions about whether debriefing is the most promising option. Take one question, for example: if psychologists know that deception harms subjects, or even poses potential risks, wouldn't psychologists have to explain why they keep deceiving? How, that is, can we still think in terms of restitution or what psychologists owe the subjects? After the first debriefing or the first case of potential harm, might it make sense to think about restricting or rejecting additional deception, instead of continuing the deception-apology cycle?

The idea that in each case of deception the debriefing adds a soothing, ethical flourish can lead to tough questions about whether psychologists really see any need to address the ethics or the obligation. It might appear instead that psychologists are content to leave this as an ongoing problem for the next researcher to deal with, if anyone needs to. Thus, psychologists who debrief do not have to clarify the ethical dilemma of deception. But to the extent that deception remains an open question, its

uncertain ethical status will infect the supposed moral grounds of debriefing.

There is another problem with viewing debriefing as an apology. In the various forms of apologies or restitutions both people have to strike a bargain. Their acceptance of the apology rests on agreement about their relationship and the way that it might move forward in a spirit of reassessed behavior or attitudes towards what was done. The apology and the relationship hold so long as there is that sense of fair play and openness about whatever was done. There are other ethical and social criteria for apologies and blame (Tavuchis, 1991, examines these, for those interested); the key here is that ordinary apologies seem to work only if the person who would rectify something admits to the full extent of any damage or misunderstanding. In addition, apologies presuppose that neither side misconstrues the difference between confessing to deception and having been honest all along.

It is in that difference that the view of debriefing-as-restitution encounters problems once more. It is one thing for psychologists to admit to deception. It is another for them to confess only after the experiment, when the revelation can no longer jeopardize scientific interests. The psychologists may use a debriefing to apologize or confess, when the deception was hardly necessary. Debriefings may end each deceptive experiment on a positive

note, but this does not change the fact that psychologists rarely devise an experiment only to realize at the end of the planning stage that they will be forced to deceive. The need for debriefing, if one exits, arises from value judgments that psychologists make about the balance of interests associated with the deception long before the experiment begins. It is the psychologist's ultimate control over this situation that makes the debriefing look like a poor substitute for the apologies that we give in most human interaction for accidents or surprises. In contrast, debriefing is the culmination of the orchestrated deception that may have enabled the study in the first place.

The psychologist asks for absolution for something done intentionally, recognized beforehand as an act that falls outside of ordinary moral conduct. The psychologist does this under the assumption that he or she will determine when, where, and how the apology would come out. Under that interpretation, the debriefing ensures that psychologists never have to relinquish the control that allows for deception. Psychologists determine the extent of the deception, the terms of the apology, and ultimately the nature of the relationship with the subject. Most important, debriefings appear to give psychologists discretion in defining what is ethical and what should be accepted as such by the subject.

To summarize the points of this section, there does not appear to be a consistent ethical value in debriefing, nor does debriefing seem to transform any negative features that deception might have. Although it would perhaps be preferable to deal exclusively with questions about debriefing, the recurrent problems with deception are unavoidable in this context. And unfortunately for those who advocate the deception-debriefing package, those problems are not made to look any better (though they may look no worse either) by debriefing.

6.6. Debriefing as Public Relations

Even if the tradition of debriefing lacks a strong ethical base, psychologists might see some practical reasons to continue the tradition. There seems to be a strong possibility that debriefing can be more useful than ethical. In the literature on debriefing there is more discussion of debriefing's practical side than its ethical qualities. In other words, there is more attention to what debriefing can do for psychologists than what it might do for subjects. In part this may reflect uncertainties about the ethics of deception and debriefing. It seems possible as well that the emphasis on self-interest, and the positive claims made about debriefing are reflections of the experimental model that we discussed in chapter three. As one commentator notes,

> [T]he 1960s and 1970s saw experimenters alter their practice to include the debriefing of most subjects. Specifically, they listened to subjects in order to assess the effectiveness of the experiment's deceptions and to judge any distress that might have been caused. Experimenters then explained the deceptions that had been perpetrated and attempted to remove and distress that remained. But such debriefing, although seemingly more humane than failing to dehoax subjects, was incapable of doing what critics [of deception] called for: namely, empowering subjects. For after eliciting subject perceptions, contemporary experimenters cannot change their past behavior or the experiment. Instead, they continue the debriefing by imposing a purportedly scientific reality on the individuals who have just volunteered their (mistaken) view of the research. Rather than being empowered, the experimental subject receives another lesson in the importance of looking to the experimenter for the correct view of things. (Harris, 1988, p. 206)

I am not going to argue here that there is something objectionable about the power differential that Harris describes in this passage. At least there does not seem to be anything inherently wrong with this differential. But

there is the potential that psychologists might use the debriefing for purposes that are socially irresponsible or at least ethically questionable. This is a special concern where the debriefing becomes a public-relations affair, meant to instill what the psychologist sees as the appropriate attitude towards DPEs and experimental psychology.

For example, in any DPE the psychologist runs the risk of generating bad feelings in the subject, or as the APA phrases it, "the likelihood of any serious resentment toward the investigator, the institution, or to the conduct of future research" (APA, 1973, p. 78). Psychologists have obvious practical reasons to be concerned about such feelings. One frequently raised concern suggests that subjects with a negative attitude towards deception might spread the word to other potential subjects (Fillenbaum & Fry, 1970; Reynolds, 1982). Aware that this would diminish the subject pool, one commentator sees a solution in a carefully staged debriefing.

> [To the subject:] I hope you have learned something and that this gives you a better idea about experiments and how they are conducted. So will you promise not to say anything about the experiment? Okay, promise? Another reason it would be bad if people talked about the experiment is that if people knew about it ahead of time it

> would spoil it for them and they wouldn't have the same experience that you did. If anybody asks you about the experiment, just tell them that it was an experiment on group discussion and that you were asked not to say anything about it. (Mills, 1976, p. 9)

Psychologists might also use the debriefing to explain to skeptical subjects the importance of their having been deceived.

> The experimenter must attempt to utilize the cooperativeness of the subject as well as the good will which he (hopefully) has built up during the postexperimental interview. . . . The experimenter should carefully and vividly explain the disastrous problems presented to science and to himself personally if he unwittingly were to report erroneous data. (Aronson & Carlsmith, 1968, p. 73)

And it is not only the subjects who are might gain the proper perspective from a debriefing. Greenberg and Folger (1988) prescribe debriefing to help psychologists feel better about what they have just done. "The benefits of scrupulously ethical treatment," they maintain, "are derived by both the subject (who is made to feel good and is given an educational experience) and the experimenter (whose

conscience is cleared of guilt for having been deceptive)" (p. 144).

Finally, there are cases where psychologists, after observing subjects act in ways that the psychologists think might be embarrassing or threatening, conduct a debriefing to instill in the subject the *correct* self-perception. Deception critic Diana Baumrind's (1964) phrase, "inflicted insight," refers to the potential that debriefings have of revealing to subjects truths about themselves that could diminish self-image or confidence. An experiment might, for example, leave subjects thinking that they are intellectually inferior. Against this, the debriefing is supposed to reassure subjects. As with the other examples, however, this shows how what might be proposed under good intentions can lead to the psychologist's continued manipulation of the subject.

The idea of using debriefing as an attitude-adjustment session, or for other researcher-centered objectives, deserves special attention. The ethical status of debriefing appears more murky when psychologists are able to present it in the interests of subjects, only to use it for their own benefit, amid claims of ethical treatment or educational benefit. Psychologists who debrief on the assumption that they have a right (or obligation?) to instill a certain disposition towards themselves are taking more liberties

than their academic positions warrant, and certainly more than an informed consent form might grant.

One risk from debriefings that function as public-relations opportunities is that psychologists might not be able to objectively re-orient the subject to the needs of research and the justification of deception. Rather, the psychologist may distort important ethical considerations, perhaps even leading subjects to see their own dignity as an impediment to science, even to feel guilty about having been the cause of all of this public relations and propaganda effort. Debriefings might end up teaching student-subjects that ethics amounts to a constraint on research, and that is a questionable lesson for the psychologist-educator to provide.

There are attitudes that psychologists simply shouldn't tamper with, and that the debriefing should not focus on. Under the best intentions, there is so far not a strong enough argument to allow psychologists to "transform" the subjects. A subject who enters the laboratory with a negative view towards dishonesty should not leave believing that there is no real ethical or social problem in advancing important research objectives by lying to, or concealing details from others. Psychologists would especially abuse their privilege by asking that the subject include his or her friends in the category of those who must now be

manipulated, so that they do not disturb the course of future research.

6.7 Conclusion

In conclusion, while I have in previous chapters tried to narrow the focus of my criticism to the specific interaction that occurs in the laboratory, we see now a need to expand the attention to the social context of DPEs. There are troubling aspects of the deception-debriefing package that force a wider concern. We can now see one way that psychologists might use the deception-debriefing package to distort the public's view of experimental psychology, and DPEs in particular. I have so far avoided the suggestion that psychologists might stop conducting DPEs. Here I will also note that like DPEs, debriefings do not seem, in principle, unjustifiable. But there are reasons to wonder what a successful justification argument might look like. We know some of the barriers it will have to overcome, and it seems difficult to construct a plausible argument that could do this.

I will end this chapter, therefore, on a partially open verdict. There seem to be reasonable grounds for *wanting* psychologists to debrief. I am not suggesting that psychologists should stop debriefing subjects. Nevertheless, the application of the deception-debriefing package should proceed only after psychologists have answered reasonable

questions about their understanding of the ethical situation. And as we have seen in this chapter, these questions might also address the ways that psychologists might avoid turning their debriefings into public-relations sessions. Debriefings probably do have an ethical basis, provided other aspects of the DPE are clarified. If psychologists can sort out some of the ethical problems associated with deception, they will in that way at least preclude the need for public relations, even if the idea of debriefing remains on uncertain terms.

Chapter Seven

Autonomy and Deceptive Psychology Experiments

7.1 Overview

Commentators on deceptive psychology experiments (DPEs) sometimes mention the autonomy of the human subjects. Some commentators speak of a broad range of autonomy-related ethical concerns. They would evaluate DPEs according to the subject's "relative power and legitimacy" for example (Kelman, 1972), apparently wanting to highlight problems that result from the subject's subordinate role. The psychologist's code of ethics, along with the Federal guidelines on human-subject research, emphasize the principle of respect for autonomy (American Psychological Association, 1992; *Belmont Report*, 1978). But few discussions adequately show how autonomy concerns apply to DPEs.

There is often not a clear distinction, for instance, between the way that we might interpret autonomy in the context of ethical discussion over DPEs. In particular, we can distinguish between autonomy concerns that apply to *participation* in a DPE and those that concern the *consent* to participate. In this final essay I will offer an attempt at clarification, by way of two interpretations of autonomy. In the course of this chapter I will consider three questions that focus on the relationship between autonomy and DPEs. My primary aim is to show how an emphasis on autonomy concerns,

according to one interpretation, would support an ethical case against DPEs.

7.2 The Autonomy-Infringement Issue

One general form of autonomy-based arguments against DPEs would focus on what happens to a subject's autonomy during a DPE. The critic might especially want to know if participation unduly infringes on the subject's autonomy. There is a clear appeal in this approach. Deception of any kind usually presents an ethical problem, if only because it is so effective in subverting the listener's autonomy. We have seen that human-subject research is equally contentious, especially where researchers design intentional autonomy-infringement into the experiment. The contrived setting of a DPE thus becomes problematic on both counts. The DPE seems to openly involve deception aimed at restricting the subject's autonomy.

An ethical system like Kant's, one that apparently rejects lies and emphasizes autonomy, would support such a critique. But critics could appeal to any theoretical position that makes autonomy inherently valuable (see, *e.g.* Berlin, 1969; Dworkin, 1988) and views deception as autonomy-infringing (Hill, 1991; Kupfer, 1990). For instance, it is not difficult to envision libertarian or utilitarian principles also supporting an argument against DPEs (Bok, 1995; Gauthier, 1993).

Yet regardless of its theoretical backing, arguments that develop this two-pronged approach may not sway psychologists. As a field, psychology grants that autonomy is valuable, that deception is problematic, and that DPEs raise thorny ethical issues. But individual psychologists might challenge the moral assumptions about deception and autonomy.

A psychologist might ask tough questions about the critic's position. Say that deception does infringe on autonomy, viewed here as a person's capacity to judge and respond to an environment. This, the psychologist might claim, only shows that deception requires some argument for justification. As we have seen, psychologists have a justification argument that they contend can accommodate autonomy concerns. This justification argument, we know, is mainly a utilitarian case, and it rests on the results of laboratory experience.

The most salient feature of that experience is that subjects tend not to act naturally if they know too much about their own participation. We return once more to the claim about practical necessity. Psychologists contend that they could not conduct some research honestly. As one introductory text explains, "if a person guesses that the study is about verbal ability, he or she may become as talkative as a sportscaster at the superbowl" (Wade & Tavris, 1987, p. 70; Greenberg & Folger, 1988). This

recurrent defense of DPEs takes on only a slightly different character when responding to autonomy concerns.

Psychologists may stay within the confines of the traditional justification argument and contest the use of what they see as pejorative terminology. They may question the wording specifically because we are discussing autonomy; psychologists will most certainly question the charge of autonomy-infringement. This time psychologists could claim that, like the medical research that relies on placebos or randomized clinical trials, DPEs rely on mis-representations and omissions. But in neither case, the psychologist might argue, does there need to be any references to *lying*. Subjects, psychologists point out, are not surprised to learn that they have been misled or misinformed. As an audience does not expect veracity from a magic act or Hollywood special effects, neither do subjects who consent to DPEs.

The psychologist's code of ethics may have such analogies in mind. The code does not discuss "lying." Along with defenders of DPEs, the code of ethics refers to "misinforming," and the giving of "misdescriptions," rather than autonomy-infringement (American Psychological Association, 1992; Milgram, 1977; Sieber, 1992). And as we have seen at various points in our discussion, it might be less important what we call it, when the fact is that subjects expect something like deception and autonomy-

infringement. Recall that the subjects expect something that works like lying, even in *non-deceptive experiments* (Reynolds, 1979).

Suppose that the psychologist is wiling to accept that experimental control achieved through deception equates with autonomy-infringement. It seems then that the psychologist has only to show that not all autonomy-infringement is unethical. To argue this case, the psychologist might point to examples like the physician who invokes concern for autonomy when deceiving a disease-stricken patient about mortality rates.[18] An autonomous person might also submit to hypnosis to stop smoking, trading one form of autonomy-infringement for another.

It should not prove too difficult for the psychologist to infuse the justified-deception argument with a relatively non-technical understanding of autonomy. The psychologist could grant, for instance, that as long as such benevolent deception avoids paternalism, the autonomy-infringement it makes possible might be permissible or even obligatory. In addition, the psychologist's autonomy-infringement, unlike the physician's, is consensual and temporary. Not only that, defenders of DPEs contend that no subject in a DPE has ever been harmed (*e.g.*, Elms, 1982). Finally, since it seems that psychologists can in most cases avoid the charge of

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[18] The example is Christman's (1991); VanDeveer (1986) gives additional examples.

paternalism, deception should be as least as permissible in the DPE as it is elsewhere.

I have strong reservations about this line of thinking. It is not something we should mistake for moral analysis. It remains to be seen, for example, how psychologists can respect and deceive subjects. And critics can rightly ask for specific details, as we saw in Chapter Five, regarding the empirical or utilitarian grounds that justify deception. It might also be useful if psychologists were more forthcoming about the specific nature of the autonomy-infringement and how this really compares to something like the diseased-patient example.

But for all this, the psychologist could still evade these issues merely by pointing out that they are not immediately tied to autonomy-concerns. And since it does seem that the DPE involves, at worst, consensual autonomy-infringement, I suggest that the critic will have to adopt another approach to show why DPEs are unethical. Initially, the prospect of making a persuasive case against DPEs on the grounds that deception necessarily involves a negative kind of autonomy-infringement do not look good. The negative view highlights questionable features of DPEs, but these are probably features that psychologists are well aware of.

7.3 The Personhood Issue

A less familiar approach to autonomy concerns might shift the focus from the psychologist to the subject. More specifically, this approach would center on the possibility of the subject's giving voluntary consent. To show the plausibility of this approach, we might first interpret the experimental situation as one where subjects consent with the knowledge that deception and a loss of autonomy will result. This much follows from the preceding discussion, and it concedes to the psychologist several claims from the justified-deception argument.

I have in mind a question about personhood, that would ask if someone can ethically agree to deliberate autonomy-infringement. Is it permissible for subjects to agree to participate, knowing that autonomy-infringement will occur? In this case, I think we have a stronger, negative answer, though we will see how we might have to go beyond the typical approach to this question to support our position.

First, we should note that Kant's ethics again provides obvious critical resources. Critics may look to the role that autonomy plays in respect for self and others. Since subjects can know only the most general details about the psychologist's ends, the respect-for-persons version of the Categorical Imperative might apply. Mindful of Kant's definition of immorality as *heteronomous* action, critics might also argue that participation will lead to reduced

capacity for autonomous, moral deliberation. We saw this much in Chapter Four.

Still from within the critic's position, we might read in Kantian duties to oneself a strong, autonomy-preserving stance.[19] To supplement this, we can note where J. S. Mill also makes the capacity to direct one's life a necessary entitlement of mature, rational persons. He allowed autonomy-restriction (referring to "liberty" instead) only to prevent harm to others or to prevent contractual servitude. More recent commentators (*e.g.*, Young, 1982) provide non-partisan views of autonomous personhood that would support our goal of illuminating the consent issue.

In medical ethics, autonomy serves as a fundamental principle or human entitlement (Childress, 1990); one influential theory uses autonomy to define physician-patient relationships and informed consent (Beauchamp & Childress, 1994; cf. Dworkin, 1988). Even a sampling of these views might support a negative answer to the personhood question. It would seem, that is, that subjects cannot ethically agree to participate. They cannot, if only because doing so would be an abandonment of their autonomy.

Once more, however, the autonomy concerns we have raised may initiate rather than conclude the criticism. Psychologists and subjects may grant that consent leads to

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[19] See Kant's *Groundwork*, *Metaphysics of Morals*, and especially the *Lectures on Ethics* (1963, pp. 116-125).

decreased autonomy. They may not, however, see anything reprehensible about such willful consent to autonomy-infringement. There may be critics who would argue that subjects should not *want* to abandon their autonomy, but each year thousands of competent and rational subjects find good reasons to do just that. It seems that critics who cling to the negative portrayal are, in effect, deciding what subjects should value. Subjects may volunteer under the reasonable impression that, valuable as autonomy is, sustaining a coherent value structure is a part of personhood too.

We also should not underestimate the moral relevance of a subject's desire to consent in the interest of advancing science. This interest might take priority over all but the most basic autonomy concerns. It doesn't matter that the subjects may not grasp their potential role, or that psychology experiments may not qualify their ambitions. The point is that rational persons should retain the option of reassessing their autonomy, and adjusting it to their preferences (and *vice-versa*).

This would let subjects consent while anticipating deception and autonomy-infringement. Not making such a contract might in some cases be irrational, as the example of the "Ulysses contract" is supposed to show. Fearful of what he might do if he heard the Sirens, Ulysses ordered his

men to tie him to the mast of his ship. He then gave instructions to disregard any later orders to set him free.

There are limits to the usefulness of this analogy. But advocates of liberal consent regulations can apply similar reasoning to research. Take mental patients, for example. Although they may be incompetent in other respects, mental patients could understand enough about the research to autonomously consent to participate. With appropriate safeguards, they should be allowed to participate out of respect for their autonomy.[20] Indeed, it might violate their autonomy to prohibit the patients from participating.

There might still not be a close enough fit between these instances of willful autonomy-infringement. For better or worse, the Kantian is unlikely to be moved by claims about what subjects think about their own autonomy. Perhaps a compromise on the ethical status of consenting to autonomy-infringement might center on the psychologist's need for methodological control and the subject's need for self-control. Viewing consent as autonomy *transfer*, rather than infringement, might lead to such a compromise. By consenting, subjects might transfer a pre-determined degree of autonomy to the psychologist. The subject could set the level of self-control and the amount of information advantage (*i.e.*, methodological control) given the psychologist.

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[20] Culver and Gert (1982) discuss such cases in-depth.

Being somewhat analogous to a situation where a patient consents to anesthesia, this transfer would leave subjects responsible for rationally assessing the likely effects on their autonomy. The amount of information subjects would want for their assessment would determine the extent of the psychologist's responsibilities. These responsibilities would ensure that consenting subjects had enough information to evaluate participation and the transfer.

Specifically, subjects might need to know about potential forms of deception, problems in assigning risks and benefit, the likelihood that any one DPE will advance scientific knowledge, and so on. The amount of information that subjects would receive would be limited on one end by the acknowledgement that some information would disqualify them for participation. At the other end, subjects could request an "information waiver," allowing them to forfeit most or all discussion of the participation.[21]

We should keep in mind that there is surely the potential that talk of transfer differs only in terminology from talk of infringement and exploitation. In addition, there is no reason to suspect that the transfer model would fully answer the worries about degradation and disrespect that the personhood question expresses, or that we considered in previous chapters. But with subjects

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[21] Ost (1984) and Strasser (1986) debate the application of information waivers in medical research.

establishing the degree of risk (to cite one parameter) in their own transfer of autonomy, the consent might avoid the charge of exploitation and self-exploitation.

To digress for a moment, we might note that some commentators suggest that researcher and subject interact as "co-explorers" (*e.g.*, Ramsey, 1970; Veatch, 1988), possibly designing and conducting the (medical) research as partners. This is an idea in the right direction, though its applicability to DPEs is questionable. For one thing, the concern with DPEs and autonomy has never been one of merely getting subjects more involved. A genuine transfer might, however, increase involvement according to the subject's terms. An improved transfer model might also avoid an overly conservative position that would undermine the very respect and self-control that it seeks to preserve.

7.4 The Practical-Reasoning Issue

The personhood issue surrounded questions about whether consent is *permissible*. A related question asks if subjects can adequately assess the offer to participate. This question, I will call it the practical-reasoning question, asks if autonomous consent is *possible*. At issue is the subject's ability to objectively factor such things as the costs and benefits of participation (and non-participation) into the decision to participate (or refuse). This question does not dwell on what happens to autonomy during the

experiment. It also relies more on a basic tenet of autonomy than ethical theory.

For that reason, we will add a few details to our conception of autonomy. We will assume that, for example,

> my beliefs are mine only to the degree that they are formed and revised according to a critical assessment of relevant evidence and argument; my conduct is mine only insofar as it is explained by the free exercise of practical reason, and this in turn is understood as the disengaged assessment of reasons for action and the choices one makes as a result of that assessment. (Callan, 1994, p. 35)

This capacity of continual self-assessment and control in turn grounds the informed consent doctrine. Prospective subjects need to know what their options are. Ideally, they should know why they feel the way they do, at least as much as these feelings affect the reasoning about those options. No prospective subject has to understand all aspects of the offer to participate or the technical details of DPEs. Likewise, the subject's deliberation does not have to be fully rational, as long as the level of rationality (or irrationality) is acknowledged by the subject. (The personhood question touched on a similar idea.)

Although no option should be persuasive enough to block cognitive access to the others, neither autonomy nor informed consent requires that any belief be independent of

external influences. We have to view the decision to consent not as a test of intellect, but an exercise of personally defined values. As one commentator notes, "how I come to have the desires and values I have is in large part not up to me, but whether I continue to act on them and fail to revise them, *i.e.*, make them autonomous, is up to me" (Christman, 1991, p. 21).

Drawing on implications within this beefed-up conception of autonomy and informed consent, the practical reasoning question asks if influences might alter the beliefs of prospective subjects. It asks if these influences might be strong enough that a subject might consent without realizing the role of the influences on the decision. The assumption here is that strong or insidious influences might deny the prospective subjects full cognitive autonomy; influences could make participation look overly positive, even compelling.

Another example from medical ethics can put this question into sharper focus. Some critics allege that prisoners exist at such a social and economic disadvantage that a researcher's offer of money, privileges, or reduced sentences in return for participation is one no prisoner could refuse. The mere offer may seem to rule out all rational reasons *not* to consent. Prisoners might consent to potentially life-threatening experiments that a rational, non-prisoner would refuse. Even where researchers do not

intend to coerce the prisoner, the offer itself can infringe on autonomy if becomes compelling rather than appealing.

As critics of DPEs, we might try to show that DPEs present a similar situation, looking for some influence associated with the DPE, or perhaps the general presentation of claims about participation. We would be looking for influences that could incline prospective subjects towards consenting to DPEs, when in their considered, rational judgment they would ordinarily refuse.[22] We might imagine a hypothetical subject, an undergraduate considering the offer to participate for course credit. Suppose that this subject has ethical qualms, perhaps vague reservations about the idea of justified, prearranged deception.

There are other reasons not to consent, but ethical claims against deception are real enough. Assuming that these reservations coalesce into a skeptical attitude towards DPEs, the critic has only to show that cognitive access to this attitude is affected by the offer to participate or by some feature of the setting. The idea is that the ethical reservations that might otherwise turn the subject away from participation may be weakened or distorted by influences surrounding the offer to participate. In that case, the consent is questionable.

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[22] Beauchamp and Childress (1994) discuss the prisoner-consent case. Two recent papers (Castro, 1995; Cocking & Oakley, 1994) discuss the problems in drawing subjects from poor or politically oppressed groups.

It is important to distinguish between types of influences and their relevance to autonomy. Threats to autonomous deliberation might come from academic requirements. Our hypothetical subject may have the option of participating for course credit. This might mean that a subject's future partly "depends on being able to please professors" (Hauerwas, 1986, p. 120). This suggests, however, an acknowledged influence on autonomy. Students who know that participation is part of a course on Introductory Psychology may tacitly consent when they enroll. Likewise, the extreme view that sees academics and scientists indoctrinating a permissive, consenting attitude into subjects is getting at a much broader issue than autonomous deliberation.

Other influences within the institutional setting are powerful, but nonetheless difficult to cash out in terms of autonomous consent. So while we should not underestimate these influences, or view allegedly tacit consent too lightly, it is better to look for subtle, yet pervasive, influences. These influences on the subject's practical reasoning may come from the packaging or the portrayal of the options. The subject may not be aware of the influences on her decision to consent or refuse because she attends more to the overall message. As Haworth (1986) notes in his critique of business advertising, a persuasive message can

> make false or questionable claims which the recipient is in no position to identify as being false or questionable. This can occur, for example, when the information the person would require in order to ascertain that the claims are false or questionable has been censored or communicated in a distorted or highly slanted manner. . . ." (p. 198; cf. Lippke, 1989)

Two candidates for questionable, persuasive claims applicable to our discussion are the positive portrayal of experimental psychology, including DPEs, and the implicit claim that psychologists can balance scientific and ethical concerns for the public good.

As people unreceptive to Freudianism may still assimilate psychoanalytic terms (*e.g.*, "repression") into their speech, prospective subjects might passively assimilate such claims. The assimilation might be complete enough to leave the subject with no apparent reason *not* to participate, when in fact her reservations have simply been silenced. In his discussion of community-identification and autonomy, Mason (1992) uses the metaphor of "engulfment" as I am using "assimilation." That metaphor is apt: psychologists need not brainwash the subject to make her view her moral reservations as inappropriate challenges to scientific objectives and the vague idea of *Psychology in the Common Cause* (Bugelski, 1989).

The generally positive claims about DPEs and the innocuousness of the deception may also leave the subject with a limited capacity to assess reasons against consenting. For instance, psychologists openly promote the importance of deception and DPEs.

> Often, temporary deception provides the only feasible means of obtaining valuable data about various aspects of human behavior. . . [W]hen used with caution, temporary deception provides us with an important, and necessary, tool for adding to our store of knowledge about human behavior. Another and somewhat different benefit conferred by this procedure involves the increased insight and understanding often gained by research participation in investigations employing temporary deception. (Baron, 1981, p. 9)

In the areas of psychology that rely on deception, the level of enthusiasm (and defensiveness) sometimes sounds ideological. Critics of deception become threats to scientific progress and occupational security (see, e.g., McGuire, 1969). Psychologists might tell hesitant subjects how they already justify deception in risk-benefit terms, and make it clear that significant numbers of subjects refusing to participate would, like further ethical restrictions, impede the search for knowledge (Krout, 1954; Christensen, 1988).

There is also the potential for strong influence from psychologists who package the chance to participate and the role that participation plays in the larger scientific project as beneficial to subject and society. As we just discussed, to some psychologists one function of post-experimental debriefings is to reemphasize to the just-deceived subject *how* beneficial DPEs are (Holmes, 1976a, 1976b). Another psychologist recommends that post-experimental debriefings be used, among other things, for "dehoaxing . . . desensitizing . . . and restoring [the subject's] confidence in science. . ." (Sieber, 1983, p. 1).

Notice that subjects who did not hear views like this first-hand could still see scientific research enjoying some status, and occupying a traditional role in higher education. That status might exert influence by way of the Official and institutional stance on DPEs that prospective subjects are likely to hear before psychologists make the offer to participate. In that case, the difficulties that face critics of DPEs would face individual subjects with ethical qualms.

Where psychologists can make a successful utilitarian claim about personal and scientific benefits, an offer to participate becomes a *de facto* offer to facilitate progress. A refusal to participate could challenge progress. Our prospective subject may feel pressure to conform ethical

concerns to the presumption in favor of progress-through-deception.[23]

Admittedly, this critical answer to the practical reasoning question suffers from the lack of empirical evidence that plagues any attribution of influence or motivation. The most secure case for autonomy-infringement would show the influences on and the causal link to the subject's decision. Ultimately, the nature of the question may rule out appeals to empiricism, which means that it may be all but impossible to find evidence supporting either side.

This casts a dim light on the some research into subject attitudes about deception, informed consent, and participation (Christensen, 1988; Muskowitz, 1995). Psychologists note that subjects are not especially concerned with deception, and that whatever ethical reservations they have do not transfer to attitudes about DPEs. Yet this research only reveals what subjects explicitly think; what we really need to know is how subjects decide and what influences them. And neither psychology or philosophy are well positioned to pursue these issues in a non-question-begging way.

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[23] My comments on the relation between psychology and society are intentionally selective. More in-depth perspectives on this relation vary from optimism and theoretical concern to alarmism and calls for reduced funding or other restrictions. See, *e.g.*, Deese (1985), Kline (1988), and Sarason (1981).

In the absence of evidence that will convince those on either side of the practical reasoning question, we might remain at least skeptical of the subject's ability to autonomously consent. That skeptical position leaves the practical reasoning question largely unanswered. Still, as was the case with the previous two questions, it may be sufficient to arouse interest in the practical reasoning question, and advance our understanding of autonomy and DPEs. This, when taken in conjunction with the issues that we have addressed in previous chapters, should provide a marked advance over the level of criticism that DPEs currently receive.

Chapter 8

Epilogue

Once, while I waited in line for a bank teller, I noticed that a woman holding a video camera was walking among the lines at the other tellers. She continued for a few seconds, alternating her attention between those customers who were talking with tellers and those who were, as I was, still waiting. Her photo-shoot did not last long; the bank's security guard soon appeared. He walked towards her with obvious intentions, and told her that she would have to either stop filming or leave.

The response that this woman gave when told to shut off her camera remains fresh in my mind. She claimed something to the effect that her actions were acceptable, since she was a psychologist. "This is what I do," she said, "I watch people. With that, the guard moved even closer. I would guess that he had harsh words about the woman's scientific ambitions, because he lowered his voice beyond the hearing of those of us who were watching this spectacle. In any event, the woman quickly left (taking her young children with her), and those of us still in line went back to staring straight ahead.

I would not go so far as to suggest that this scene could serve as a unifying theme for these essays. Still, this episode frequently comes to mind when I read yet another version of the Strong Case for justified deception.

While it would be wrong to make much of this incident (or to attribute similarity to other "psychologist"), I have tried to show that there is nothing far-fetched or alarmist in questioning the social role, practices, and above all the pervasiveness of the DPEs.

It would be wrong to miss the import of this episode. As I interpret it, this episode shows the tendency that psychologists have, and that society reinforces, towards excusing what would otherwise be serious actions on the basis of perceived social acceptance of psychology. This interpretation would be accurate whether or not the woman was an academic psychologist. It would hold whether she ever performed a laboratory experiment. Indeed, the situation might appear worse if interpreted it as showing how even non-psychologists might try to disguise and justify their behavior. What is it about psychologists, that is, that gives non-psychologists such impressions?

The woman's defense seemed to me to suggest this: an ordinary citizen perhaps would not have taken such liberties, but a psychologist might have extraordinary motives and special privileges. Most of us do feel entitled to record the posture and movements of strangers in the bank lobby. But there is apparently some ethical determinant in the title of psychologist. In several ways, this episode sums up this collection of essays, since we have been

discussing little that we could not classify as motive or privilege.

We have seen that there are certain things that psychologists feel justified in doing to other people. It is easy to lose sight of this fact: ethical debate over DPEs centers on deception. Despite psychologist's claims about deception being the rule in society, it is still true that the act of deceiving another person generally earns some degree of moral blame. We are asked by the Strong Case to make an exception, based on position, privilege, and motivations. By the same token, there are things that people will do in the name of psychology, or on the promises or assumptions of psychological research, which they would not ordinarily do. This is one reason that two of the chapters tried to highlight the relevance of the subject's actions as well.

It might clarify my position if I mention what I have not tried to do with these essays. I have not tried to discuss the psychologist's privilege directly. Instead, I have concentrated on one category of acts, the intentional deception, or the intentional submittal to deception, of another person. I have not tried to show, or even suggest, that deception or DPEs are threats to a stable, healthy society. I have even left open-ended most of my claims about DPEs being harmful to any of the participants.

What I have tried to do is force upon psychologists and those who support them basic questions about values. If I have shown anything, I hope to have demonstrated the reasons that we might want to know what values separate those who advocate DPEs from those who criticize them. To that extent, I have until now hardly given mention to potential criticisms to my own views. Perhaps I cannot avoid those any longer.

One criticism might sound this way. A psychologist might see in these essays a naive, narrow view of the ethical problem. My attack on the ethics of experimental deception might appear fatally uninformed about the nature of experimental psychology. I might have missed all the good reasons that psychologists can put forth as justification for deception. The critic might say that I should accompany psychologists as they conduct their experiments.

Presumably this would enable me or any other critic to better understand the problem. I might then offer a better argument, or at any rate a different one. It might also satisfy such critics if I stayed more within the lines of the debate, carefully explaining which types of deception most bother me. After all, there is clearly a difference in degree between overt attempts at manipulation and unintentionally leaving some details out of an explanation.

At different stages of the writing of these essays I have heard variations of this criticism, from teachers,

students of psychology, and journal editors. None so far has made a convincing defense, and I'm not sure that my position has changed. One thing that undercuts this criticism is that I am quite willing to plead no contest to the charge of being misinformed about experimental psychology. In truth, I am not terribly eager to learn more regarding experimentation. No doubt there are compelling reasons to deceive in the laboratory, and there are probably subtle methods of doing and explaining this.

But that doesn't seem surprising. Psychologists are not the only ones who can cite compelling reasons to deceive. The ease of which we can find compelling reasons make deception a continuing ethical problem, not just in psychology, but anywhere in public and private life. Thus, it seems odd to suggest that more exposure to deception would somehow improve my perspective. Just as I don't deny the enormous advantages that deception might provide, I grant the possibility of offering elaborate arguments for securing such advantages in each case.

What I have tried to do with these essays is ask if in *this* case psychologists have justified their deception. And if they have, on what grounds is the psychologist's special brand of deception justified, and according to which theory? It doesn't seem obviously true that additional exposure to the nuances of experimental deception will provide different answers to such questions. In addition, if there are

technical details and distinctions that I am unaware of, I have wonder if these are not missing from the arguments that would defend deception as well. It is hard to see why psychologists would also leave such details out of their arguments.

There is also a notion that a critic should not make claims about what psychologists do, at least not from an insulated and naive position within philosophy. Perhaps critics who think as I do about DPEs should learn more about the uses and benefits of deception. Presumably, if I assisted during an experiment, I would "learn" what the psychologist knows. I would discover, that is, that (a) no subjects are visibly or seriously harmed by deception, and (b) deception is a valuable tool of scientific inquiry.

Without attempting to speak for all critics, I have to respond that I have never doubted either claim. Therefore, "getting my hands a bit dirty" is probably not the best way to test the claims that I am making. (Can I argue that if psychologists would simply stop performing experiments for a while, they would see my view?) Again, nothing in these essays suggests that deception seriously harms subjects, or that deception cannot provide important access information that would otherwise remain hidden.

I treat the dirty-hands response more fully in the following Appendix, so my comments will be brief here. Admittedly, I would raise issues of my own credibility if I

presented myself as having anything beyond casual acquaintance with the history or theory of experimental psychology. But I never claim to be an authority on experimental psychology or even deceptive experiments. A more crucial issue is the amount of technical knowledge is required for ethical criticism. How might empirical observation or additional reading improve the results of my project?

For my part, I have little interest in what happens when a psychologist deceives and how this improves the scientific standing of the claims about the experiment. More to my ethical concern is what happens when psychologists conceive an experiment, and decide to incorporate deception. That *decision* is in my view unethical and unjustified; I leave others the task of documenting what is right or wrong with deceiving specific subjects in specific conditions.

We might make an analogy to the abortion debate. A critic of abortion need not have performed one, or been a patient herself. Female commentators sometime allege that male commentators cannot fully grasp the ethical issue, and should not try to offer arguments one way or another. But surely there is a minimum of technical or personal knowledge required. A woman who undergoes an abortion brings a different approach to the problem. There is no reason to believe that her arguments will have a special logical force that others cannot approximate. Nor is there any reason that

arguments have to dwell on the details of what happens, rather than the results and the arguments put forth about the ethical relevance of those details.

Another suggestion that I have frequently heard has to do with taxonomy. Defenders of DPEs might say, for instance, that it would be more beneficial to delineate the kinds of deception possible in the DPE, and criticize only the "bad" forms of deception. This selective approach to the issue has been in fashion since the mid-1960s, in my view with limited success. I choose instead to look at the nature of deception, and set aside the question of which deception is acceptable.

I base my decision on my interest in the idea of justifying deception, whatever its form, rather than any specific feelings about one form or another of deception. The problem in assuming that one form of deception has qualities that another lacks is that this kind of differentiation requires a substantive ethical position in itself. There is a need to elaborate such a position, but I am not prepared to give it here. In the end, it may disappoint critics that I leave open questions about various forms of deception. But I have tried to address questions that are important no matter how well we understand, for example, what distinguishes misdescription from misinformation. To the extent that I have accomplished that, I have taken a very general look at a specific occupance of

deception. My claims are not meant to apply to all forms of deception, or to deny that there are forms that are less objectionable. But taxonomies aside, psychologists know when they are deceiving, and I have addressed the arguments that they give to justify these instances.

Appendix

Medical Ethicists in the Psychology Department?

Overview

Philosophers often serve as consultants or advisors in hospitals and medical-research institutes. There they carry the title of clinical or medical "ethicist." The ethicist is supposed to help physicians and patients come to terms with the special moral problems that arise in medical situations. Some see the medical ethicist as a positive development in the Socratic tradition of public criticism and reform. By working in the hospital (or merely writing about what goes on), ethicists can reduce the complexity of moral problems, and eliminate confusion and misunderstanding. Academic philosopher R. M. Hare (1977) offers this view, and not surprisingly, contends that ethicists can reveal to physicians aspects of moral language or morality itself.

This provides some precedent for the idea that philosophy might play a role in assessing experiments in psychology and other human-subject sciences. Given optimistic views on the role of the medical ethicist, the idea of a social-science ethicist might seem a natural extension. Surely there is confused thinking in social science, along with ethical problems that scientists are not willing or able to solve. Why not, therefore, suppose that a philosopher can isolate ethical aspects of something like

the deceptive-experiment problem that psychologists overlook?

Critics suggest that philosophers might be better suited elsewhere. What some of these critics have to say about medical ethicists would apply to prospective social-science ethicists. The critical comments or warnings would also raise questions about the foregoing essays. I will in this Appendix consider the critics of medical ethicists; in particular, I will examine the ways that the medical ethicist and the social-science ethicist might overcome many of the most common criticisms.

To begin with, we might state the criticisms in general terms so that we can show where they might apply to the social-science ethicist. Most important, critics allege that medical ethicists are naive or self-serving, wrongly seeing themselves privy to some wisdom that the medical staff lacks. To critics, the title "ethicist" hints at a kind of expertise (or pretention) that physicians with years of experience in critical care medicine are especially suspicious of.[24] The specialized knowledge that ethicists claim to have does not in any case seem appropriate to the

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[24] Caplan (1992b) highlights problems with the ethics-expert approach. I will not have a lot to say about this issue in this dissertation. This is a more likely problem in medical ethics. This may be because of the comparatively long history of medical ethics. It could be that unlike medical ethicists (and business ethicists), social-science ethicists haven't developed their own discipline to the point that they could convince anyone that they are "experts" yet.

demands of medical-ethics issues. In the worst scenario, the ethicist would be able to do little more than raise abstract questions that are irrelevant and possibly unanswerable.

Skepticism About the Ethicist's Resources

We could summarize the critic's position as it would apply to social science by saying that ethicists are supposed to have too much academic training and too little technical, hands-on training.[25] Academic training in particular is supposed to be of little use in the hospital. We might reasonably wonder whether this training would be of any more use in the laboratory.

It seems that the ethics that philosophers discuss on campus is too far removed from the ethics that the rest of the world uses. Academic ethics is alleged to be too dependent on reason, logic, and methodological (almost scientific) approaches to ethical problem-solving. In what is probably an oversimplification, the critic sees academic ethics involving a naive attempt to redefine problems and rationally apply a favored ethical theory or set of principles (see esp. Noble, 1982).

Applied to the hospital setting, this leads to ethical "consultations" that produce predictable and thus not very useful solutions. Critics portray the ethicist as someone

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[25] Gorovitz (1986) and Jamieson (1988) list criticisms directed at ethicist. I only touch on those that have some bearing on the role of a social-science ethicist.

expecting real-world problems to resemble the thought-experiments and arguments that abound in the artificial world of academic discussion. Yet when medical dilemmas inevitably violate this ethicist's sense of closure, as of course they will, the ethicist may choose to endlessly debate the meanings of terms (*e.g.*, are we talking about "killing" or "letting die" here?). Perhaps the ethicist will dismisses the problem altogether (as "nonsense" or "muddle-headedness").

In either case, critics think that physicians have neither the luxury nor the inclination to adopt such a course. One critic complains that "wherever the action is in medical ethics, the action is mostly talk" (Kass, 1990, p. 6). Physicians, it seems, actually have to solve ethical problems. It is easy enough to imagine a psychologist growing impatient, in a similar way, with my claims about DPEs.

The school of "anti-theorists" says some especially negative things about the academic treatment of ethics, and, by implication, the ethicist's prospects. In some instances the anti-theorist position would hold that academic ethics is so anemic that would-be ethicists have nothing of substance to apply to real life-or-death issues. One anti-theorist asks, "if there is such a thing as the truth about the subject matter of ethics . . . why is there any expectation that it should be simple?" (Williams, 1985,

p. 17). Another speaks directly to medical ethicists, reminding "those who make a living talking and writing about ethics" that "ethics never came in flavors of deontology and consequentialism . . . . autonomy and utility are not intrinsic properties of ethical problems. . . ." It seems that ethicists who forget this trade "the world of genuine moral experience for the world of moral fiction, a simplified, hypothetical creation suited less for practical difficulties than for intellectual convenience" (Elliot, 1992, p. 29).

As if that weren't bad enough, medical ethicists are also attacked from the other direction. Some critics fault ethicists for *not* emphasizing ethical theory enough (*e.g.*, Clouser & Gert, 1990; Lustig, 1993; Dubose *et al.*, 1994). They cite "principlism" as a key failing in the way the prevailing methods of medical ethicists. Briefly, the principlist supposedly identifies ethical problems through the application of one or more principles, such as autonomy, and proposes solutions that would restore the principle to its rightful value. Instead of appealing to an ethical theory as they should, the ethicist focuses on one or two values or principles, and (in the critic's view) fails to situate these within the broader medical and moral context. In light of this line of criticism, I should probably concede that my discussion on autonomy in the previous chapters comes dangerously close to principlist reasoning.

At the very least, my account of Kantian ethics likely offers the kind of application that anti-theorists worry about.

Still other critics (*e.g.*, Holmes, 1990; Kass, 1990) seem to think that even with the best ethical theory and the correct application, the medical ethicist will still fail. Here references to the "real-world" abound. In a criticism that translates readily into something a social-science ethicist might hear, critics contend that the real-world of medicine is more complicated than the world the ethicist is used to. Presumably, this is because ethicists are not used to integrating values or methods from outside of the academy into their ethical deliberation. In our context, a psychologist might allege that an ethicist is not in a position of knowledge, at least not one sufficient to support a criticism of a whole category of experiments.

This line of criticism has close ties to the attacks on theory and methodology; as it turns out, this school of critics, call them the "eclectics," most distrusts ethicists' orientation to ethical problem-solving. According to the eclectics, who are interestingly enough mostly practicing ethicists, a useful ethicist will combine feeling, interpretation, and narrative with standbys like analysis and abstract reasoning. Others eclectics want philosophers to learn from ordinary life experiences before

joining the ranks of the true (*i.e.*, truly useful) ethicists.

As I mentioned in my response to this criticism in the last chapter, here too it is never clear what, precisely, prospective ethicists should learn. Recommendations range from the obvious to the trivial. The most basic recommendation holds that medical ethicists need to recognize that life is more complicated than the academic approach to morality indicates. Stuart Hampshire (1983) devotes an entire book to the idea that ethics and life make for a challenging combination, one that perhaps academic ethics is not ready for. His work is often cited in commentary on the medical ethicists position.

Also cited frequently are the works of critics who want more emphasis on ethical reflection, and of course, less analysis (*e.g.*, Kass, 1990; Williams, 1985). It is not often noted that the hospital, or the laboratory, are set somewhat apart from what constitutes the real world for most of us, but the criticism builds on an assumption that what is ethical in one context will perhaps not be ethical (or as ethical) in another, so this detail may be unimportant.

Diamond (1982) and Nussbaum (1989) suggest that imagination and reflecting on great literature would reinvigorate academic ethics. They never mention hospitals or ethicists, but critics see in their work further evidence that the medical ethicist is missing something important

about the real world. Finally, those who take broad swipes at academic ethics, such as MacIntyre (1984) and Kass (1990), see in the concept of virtue an emphasis on character and practical deliberation that current attempts to find easy or even "correct" solutions miss. Theirs is only a subtle twist on the anti-theory position, but its force derives from the assumption that virtue is part of real-world moral existence, or that it should be.

One school of critics breaks with this progression, and argues that none of this will salvage the medical ethicist's role unless medical knowledge is added to it. What ethicists need most of all, these critics claim, is experience in a hospital (*e.g.*, Callahan, 1973; LaPuma & Toulmin, 1989). Ethicists should learn, apparently, what dialysis involves before pontificating about scarce medical resources. Ethicists should understand how patients view their own roles in the medical situation, in addition to learning how physicians understand these roles. Once more, critics seem to wish that ethicists would be as willing to get their hands dirty as they are to refine theories and counter-examples.

As I pointed out in the last chapter, recommendations to this effect have an undeniably practical side. It is hard to argue against the position that, "if all members of the consultation service are expert in their own disciplines and clinically astute, requesting physicians are likely to pay

attention to their recommendations" (LaPuma & Toulmin, 1989, p. 1110). As someone who has experienced some measure of inattention or outright rejection, I can see the import of such criticisms.

Implications for the Social-Science Ethicist

Hence, while much of this criticism seems to attack a caricature of the responsible ethicist, there are hints at significant obstacles that a medical ethicist might face. If this is so, in spite of the humanistic tradition that medicine and ethics share, we can expect the social-science ethicist to face similar obstacles. Indeed, the challenge from social scientists might be greater.

Social science has for most of this century modelled itself on positivistic lines. Though it may be hard today to find a researcher who openly advocates a positivist methodology, the positivist attitude does persist in the treatment of human subjects as objects of controlled observation. Add to this the 20th-century aversion that psychologists and others sometimes show towards philosophy (most notably the classical behaviorists), and there is clear justification for considering how well the social-science ethicist might cope with the criticisms I have just listed.[26]

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[26] It is interesting to note that business ethicists have long been challenged with many of the same criticisms. They have nonetheless thrived, in part, by taking such criticisms

Methods

The social-science ethicist may find it especially hard to dodge the criticisms regarding the inapplicability of academic ethics. Granting some obvious truth to this criticism, I think that the key is to keep this criticism in perspective. Researchers have traditionally been quite selective in deciding what they can and cannot do. We have seen in Chapter Three that the psychologist's code of ethics is based on suggestions from the field, not Mill's *On Liberty* or Rawls' Maximin principle. With little regard for theoretical coherence, researchers can justify an action according to one value-base (*e.g.*, beneficence) and justify another action on a different, competing value-base (*e.g.*, freedom of inquiry). There is no reason to think that only psychologists define the Ethical in such selective terms.

On the contrary, researchers across the social sciences can do this, if for no other reason, because they are out to prescribe only a very narrow range of behavior. Anyone who would make claims as an ethicist would need to either acknowledge this or show a way around it. Sometimes social scientists need only to prescribe behavior within a sub-discipline, as when the psychologist's code of ethics divides concerns between the experimental and therapeutic sub-disciplines. The social sciences may at times seem to

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for granted, and carving out their own discipline, sometimes in apparent isolation from actual business practice and business-school curriculum.

offer a systematic account of morality or moral personhood.[27] But it is unlikely that these accounts are systematic in the prescriptive sense that the ethicist is used to finding in the works of Aristotle, for instance.

Thus the social-science ethicist will have to devise an application of ethical theory or principles that can accommodate, rather than exclude, scenarios involving fragmented value systems. Academic ethics of the sort that critics dismiss, has not traditionally seen this as necessary or explored ways that it might be done. Yet this only means that making sense of the ethical systems at work in social science will be difficult for someone used to presupposing an ethical theory and a world that seems to await it. In other words, critics may underestimate the ethicist's strengths if they assume that ethicists will not see the error in such a presupposition.

On that note, we might bear in mind that ethicists are not unique in placing too much confidence in methods traditionally used in their field of ethical deliberation. It is not just ethicists who summarily reject a range of values (*e.g.*, as expressed in the appeal to scientific progress), either because they see internal conflicts, or because they seem not to fit ethical positions elsewhere that are difficult to give up. Such lapses in judgment are

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[27] Socio- and evolutionary-ethics come to mind, to the extent that they qualify as social sciences.

probably more universal than the critic wants to admit. On one hand, ethicists too are suspectable to these lapses. On the other hand, academics-turned-ethicists seem well qualified to identify flawed reasoning, especially their own.

It is hard to argue with the charge that much applied ethics would be better "applied" in journals or classrooms, that is, left as thought experiment and not applied at all. But we have seen reason to be as critical towards the ethical positions that social scientists advocate. These positions, after all, are more likely to find their way into formal codes of ethics than anything that academic philosophers debate. Again, the point is that there is more than enough faulty reasoning and bias to go around. An ethicist can help clarify and refine ethical thinking wherever it occurs. Social science research seems a fertile ground for an application of this admittedly vague objective.

Ultimately, we may need no stronger argument for the role of ethicists than the fact that social-science involves contentious philosophical issues. There is a need to restrain those, whether in philosophy, science, or administration, who would attempt to reduce the task of ethical assessment to inflated or unsound claims about, for example, the ease of weighing risks and benefits.

This view, the Socratic one, suggests that we need to temper the idea that academic training makes for insufficient preparation. No matter where ethical deliberation takes place, it can benefit from some emphasis on a broad range of values. Perhaps ethicists in some areas need to supplement ethical theory with reflection or caring, for instance. But it seems equally true that the ethicist's success will depend on his or her ability to understand the human relationships at issue. And if we are focusing on social-science research, the issues that arise do not seem to be directly tied to the level of care that psychologists provide, or the amount of great literature that the ethicist has read. This is not to denigrate the value of such resources; the point is that they can be important to the ethicist's understanding of ethical issues without having an immediate effect on his or her ability to understand a particular set of ethical problems.

The notion that eclectic and "real-world" ethical positions are more sound than those that emphasize only a narrow range of values (utilitarian concerns, for instance) rests on a misconception about ethical deliberation. It is not I think so difficult to clarify the challenge. The ethicist who would assess social science research needs to do what most anyone engaged in ethical deliberation has to do. Ethicists, whether in medicine or science, will have to determine which values are relevant, which are under-

represented, and which are most appropriate to the given problem.

Admittedly, all this would be made much easier if social-science ethicists had the theoretical backing that medical ethicists have. It would help if ethicists made the construction of such a theory first priority, and devoted less attention to social science issues. I say this because until social scientists and ethicists can devise a theory of social science ethics, there will continue to be (warranted) skepticism over the ethicist's role. Social scientists now have rough heuristics telling them what values they are supposed to uphold, but the thinking in this area has been dominated by medical ethics models (see esp. *The Belmont Report*), and as we have seen, the comparison between the two fields is not without its problems.[28]

The Need for Theory

A theory of social-science ethics would have to comprise several ideas. It would, for example, require an inter-disciplinary model of the researcher-subject relationship that could also represent the relationship between social science and society at large. The theory would have to take some position on the nature of persons, as well as the nature of scientific inquiry. There are

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[28] My thinking on this point has been influenced by Machan & Den Uyl (1987).

regrettably few theories of medical ethics; a few might provide excellent starting points for the social-science ethicist.

Some of the better theories prescribe the appropriate relationship between researcher and subject (*e.g.*, Culver & Gert, 1982; Veatch, 1987), but these are usually silent on the overall, social effects of research. Since many areas of social science involve large numbers of subjects and ordinary citizens, ethicists would have to adapt medical-ethics models before they would be of any real use in social science. We cannot assume, in short, that the same kind of concerns from an experiment in medicine will apply in a psychology experiment.

The theory that a social-science ethicist would need might also emphasize the methodological orientation that the various social sciences adopt. This too would involve a departure from medical-ethics models, since the view of the medical-research subject is quite different from the view of the behavioristic, passive subject of a psychology experiment.

Finally, this theory would have to take some steps towards definitions that stalemate debate over issues like deception. What counts as a resolution to an ethical problem? What is harm or benefit, and how do these values span the sub-disciplines in social science? These are not issues easily resolved, but an ethicist might find it all

the more difficult to resolve selected problems in social science without the benefit of a systematic theory of social-science ethics.

Technical Competence or Technical Awareness?

The ethicist will likely label certain practices unethical. He or she will also attempt to show why specific instances of research should not be conducted. It is easy to see, therefore, how someone in the role of ethicist might operate in a situation where he or she becomes, to say the least, unwelcome. Even with a comprehensive theory of social-science ethics, the ethicist might still be unwelcome and unproductive in the laboratory. The combination of challenges I have already listed, not to mention the current lack of clarity over this potentially adversarial role, raises the practical problem of how the ethicist is supposed to get anyone to listen. Recall that an analogous problem is seen by some critics to undermine the medical ethicist's chances of success.

Critics of prevailing views towards medical-ethics suggest that the ethicist has to gain technical knowledge to be an accepted member of the team. Instead of debating the meaning of "autonomy" or "harm," it seems that ethicists are supposed to immerse themselves in methodological and technical details. In the case of social science this would probably translate into a need for the ethicist to learn the

history of the particular science, its philosophical roots, and most important, its actual practice. That is, the social-science ethicist might need to understand how researchers work, what decisions go into performing an experiment, and what explanatory or predictive value the various methods promise. It might even, if critics of medical ethics are correct, help if the social-science ethicist can observe the actual work of science before turning to assessment.

It is impossible to make claims about the degree of technical knowledge that an ethicist needs without thereby making relatively strong claims about the ethicist's role. Critics of medical ethicists rarely explain what the ethicist's proper role is. But there are two issues. First, how can the ethicist make a valuable contribution? Second, how can the ethicist get social scientists (or physicians) to listen? It is easy to fuse these two points, as critics who think that physicians will only pay attention to ethicists who display a high level of technical knowledge do. For our purposes, we can consider how the criticism regarding technical knowledge might apply to the social-science ethicist by first getting clearer on the ethicist's role.

I suggest that the successful ethicist will perform in a comparatively limited role. There is no reason that competent ethical deliberation should require the ethicist

to become a full-fledged member of the scientific staff. There may be political or institutional grounds for wanting the ethicist to act as a graduate student learning social science; however, the ability to arrive at sound conclusions regarding the practice of human research is not linked to his or her ability to fit into the preconceived notion that researchers might have of an ethicist.

My resistance to the critic's suggestion issues from a skepticism over the amount of technical knowledge that is required to challenge the ethical quality of human experimentation. That is not to say that the problems that the social scientist faces do not require a great deal of expertise. But our understanding of philosophy of science and ethics is not developed to the point that ethicists or researchers can offer broad recommendations about the level of technical knowledge that an *ethicist* would need to have (any more than there is a standard regarding how much ethical theory a researcher needs to know).

Knowledge of social science is certainly not going to diminish the integrity of the ethicist's conclusions. But exposure to social science may not be prerequisite to an adequate understanding of ethical problems that arise. Just as an ethicist might offer a sound argument regarding abortion without ever having witnessed the procedure, a social-science ethicist might detect questionable uses of human subjects without having conducting a survey or

experiment. An ethicist can avoid being naive about primary methods and assumptions in social science without becoming a junior partner of the research team. Most important, it is not clear how increased technical knowledge would benefit subjects.

The criticism that ethicists want only to talk while others actually conduct experiments misses the point. Why believe that ethicists should do anything but talk, and what should they talk about other than analysis of concepts and interpretations of principles or theories? Researchers and subjects deserve at least tentative solutions from philosophers who claim some insight into applied ethics. To the psychologist, it may sound trite or even insensitive if ethicists couch these solutions in jargon of beneficence or autonomy. Still, the ethicists who overlook the depth of such concepts in favor of learning about experimental design would be disregarding the best aspects of their training.

This point returns us to a fairly basic issue concerning any ethical situation: the success of those deliberating will be proportional to their access to relevant details, not just about the ethical problem, but about the intellectual and cultural resources they have at their disposal. Hence, no matter how well the ethicist understands social science, he or she will still be expected to propose solutions that rest on philosophical analysis. Solutions that try instead to cater to the social-

scientist's knowledge base will not provide anything that isn't already available.

Some Concluding Remarks on the Extent of My Own Expertise

Let me close this Appendix with a concession. My recommendation of a limited role would leave the ethicist in a precarious (and possibly unpopular) position. I'm not sure, however, that this is a kind of precariousness that ethicists of any stripe want to avoid. Some commentators (*e.g.*, Caplan, 1992a) worry about academic philosophers' not being on sure footing in the hospital, after a career that previously included only presenting papers and teaching. Yet the role of any ethicist, medical or social-science, *should* be a precarious one. The role itself should force the ethicist to see the difference between an academic conference and a dispute over an informed-consent form (though it is hard to imagine anyone would need that lesson).

More important, the ethicist's role should also provide an opportunity for enhancement of both areas, the academic and the applied. Perhaps in the end the role of ethicist should be at least as precarious as the practice of human-subject research. My guess is that those who fail to see the risks of applying academic ethics to the welfare of subjects and society have missed the precariousness of limiting ethics to the academy as well. Ultimately, I am willing to

work within the precariousness that is indicated by what I have said about DPEs and what I have just said about the ethicist's role.

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