Does Deception Impair Experimental Control?

A Review of the Evidence

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Abstract

In an attempt to increase experimental control, many psychologists employ deception. This modus operandi has been a longstanding concern among other psychologists who have suggested that deceptive practices make participants suspicious, prompt them to second-guess experimentersí true intentions, and ultimately affect their behavior. According to these critics, deception impairs or even destroys the experimental control it is meant to achieve. Here we review evidence bearing on this argument. We find important observable methodological costs of deception. We propose a pragmatic and incentive-compatible response that over time is likely both to reduce deception to a last-resort strategy (as recommended by the APA), and to spur methodological innovation.

Introduction

> The use of deception [in experiments] has become more and more extensive.... It is easy to view this problem with alarm, but it is much more difficult to formulate an unambiguous position on the problem.... I am too well aware of the fact that there are good reasons for using deception in many experiments. There are many significant problems that probably cannot be investigated without the use of deception, at least not at the present level of development of our experimental methodology (Kelman, 1967, p. 2).

In his well-known article ìHuman use of human subjects: The problem of deception in social psychology,î Herbert Kelman (1967, p. 2) described his dilemma as a social scientist as that of being caught between the Scylla of the use of deception to study important social behaviors, and the Charybdis of ethical and methodological considerations. He wrote this article in the wake of a public exchange between Baumrind (1964) and Milgram (1964) and in response to the dramatic increase in the use of deception during the 1960s. While the exchange between Baumrind and Milgram focused on the ethical implications of Milgramís research on obedience, Kelman (1967) extended this discussion to the long-term methodological consequences of deceptive practices on participantsí expectations and behavior. The essence of his concern is best expressed in Kelmanís prophecy:

> As we continue to carry out research of this kind, our potential subjects become increasingly distrustful of us, and our future relations with them are likely to be undermined. Thus, we are confronted with the anomalous circumstance that the more research we do, the more difficult and questionable it becomes. (p. 7)

In the American Psychologist, we recently reiterated Kelmanís concern that deception experiments contaminate the participant pool (Ortmann & Hertwig, 1997, 1998). Our comment prompted responses from several researchersóBrˆder (1998), Kimmel (1998), and Korn (1998)ówho defended the use of deception. Three arguments feature prominently in their defense of deception. They are (1) the use of deception has, after an increase in the 1960s and 1970s, declined (e.g., Korn, 1998); (2) ìthe preponderance of evidence suggests that deceived participants do not become resentful about having been fooled by researchers,î and in any case the effects of suspiciousness on research performance ìappear to be negligibleî (Kimmel, 1998, p. 804); and (3) deception is an indispensable tool for achieving experimental controlóat least in some socially significant research areas (Brˆder, 1998; Kimmel, 1998; Korn, 1998). Our comments as well as those by Brˆder, Kimmel, and Korn were based more on assertions and theoretical constructs than on empirical evidence.

It is the main goal of this article to present empirical evidence that bears on these three arguments. Our focus will be on the methodological costs of deception. We do not address the ethical issues that inevitably come with deception. This is not to say that we believe these concerns, laid out eloquently by Baumrind (1964, 1971, 1985; see also Herrera, 1996; Aguinis & Handelsman, 1997), are irrelevant.[1] However, more than 30 years of controversy over the ethical issues have made it clear that this discussion is driven by values, over which people naturally disagree. In contrast, an extensive and systematic review of the methodological costs seems to be feasible but, to the best of our knowledge, has not been attempted recently. Before we begin with our review, we first provide a definition of deception and describe the rationale for its use.

What Is Deception?

Deception is not easily defined. Nevertheless, there seems to be considerable agreement about what definitely ought to count as deception. Such agreement is, for instance, manifest among the group of researchers who studied the prevalence of deception as a research method in (mainly social) psychology. We found a total of 14 studies that analyzed the use of deception in various journals. Examination of their criteria for defining deception reveals that intentional and explicit misrepresentation, that is, provision of misinformation (e.g., about the purpose of the investigation, the identity of researchers and confederates), is unanimously considered to be deception.[2] This view is also shared across disciplinary borders. In the words of economist John Hey (1998), ìthere is a world of difference between not telling subjects things and telling them the wrong things. The latter is deception, the former is notî (p. 397).

Heyís comment, furthermore, indicates what seems indeed to be widespread agreement among researchers: Withholding information does not necessarily constitute deception. In other words, not acquainting participants in advance with all aspects of the research being conducted, such as the hypotheses explored and the full range of experimental conditions, is typically not considered deception. Adair et al. (1985), for instance, reported that ìthe simple failure to disclose the true purpose of the study was not counted as deceptionî (p. 63). Although Baumrind (1979) suggested that ìfull disclosure of everything that could affect a given subjectís decision to participate is a worthy ideaî (p. 1), she also conceded that ìabsence of full disclosure does not constitute intentional deceptionî (Baumrind, 1985, p. 165). Similarly, economists McDaniel and Starmer (1998, p. 406) described some forms of ìeconomy with the truth,î as ìperfectly legitimate,î and

Hey (1998, p. 397) pointed out that ìill-defined experiments,î (i.e., when the experimenter does not inform participants about all features of the experiment) are an important tool.

The demarcation between deception and nondeception blurs when it comes to defining misrepresentation in terms of implicit violations of participantsí default assumptions. One default assumption a participant is likely to have is that experiments start only after an experimenter has clearly indicated its beginning. As a consequence, she might assume that her initial interactions with the experimenter (upon entering the laboratory) are not an object of investigation. Should violations of such expectations be counted as deception? Some of the researchers who assessed the prevalence of deception did not appear to include violations of this assumption (Adair et al., 1985; Nicks et al., 1997); Gross & Fleming, 1982), while others did. Sieber et al. (1995) and Gross and Fleming (1982), for instance, considered participants to be deceived if they were unaware of being research participants at all or were unaware that the study had begun at the time of the manipulation. The fact that some researchers included violations of default assumptions in their definition of deception and others did not, might reflect conceptual disagreement. However, it could alternatively reflect a pragmatic decision on the part of researchers who aimed to quantify the prevalence of deception; violations of default assumptions are much more difficult to identify than provisions of misinformation. We propose that violating default assumptions has the same potential for creating suspicion and distrust as providing false information and should therefore be treated as deception.

To conclude this section, a consensus has emerged across disciplinary boundaries that the intentional provision of misinformation ought to count as deception and that proscription of deception does not entail disclosure of the research hypothesis, the full

range of experimental manipulations, or the like. Common ground has not (yet) been established with respect to the violation of participantsí default assumptions.[3]

The Logic of Deception and Two Degrees of Experiences with Deception

Why deceive? Deception is often justified with the argument that certain aspects of behavior can only be studied if people are caught off guard. If they suspected or knew that some socially undesirable aspects of behavior are being observed (e.g., conformity, prejudices, anti-social behavior), then they would alter their ìnaturalî behavior to look as good as possible to the experimenter or other participants. Consider conformity as an example. If participants knew that an experiment explores the extent to which an individual conforms to erroneous judgments of others, then an individual would be likely to demonstrate that he or she does not give in to social pressure. Therefore, so the argument goes, any experimental investigation of conformity behavior needs to camouflage the purpose of the experiment to achieve experimental control. If not, then ìthe psychologist runs the risk of distorting the reactions of his or her subjects and ultimately limiting the applicability of the research findingsî (Kimmel, 1996, p. 68). Therefore, deception is a methodological necessity in particular for the investigation of socially significant behavior (e.g., Kelman, 1967; Weber & Cook, 1972; Cooper, 1976).

Challenging this logic, critics of deception argue that it is the very use of deception that impairs and ultimately may even destroy experimental control and thus threatens the validity of research findings. Kelman (1967) is not the only one to have proposed this argument. Other researchers in psychology and sociology as well as in experimental economics also have worried that deception contaminates the participant pool. While in sociology it was suggested that a likely outcome of deceptive research is participantsí

future resistance to other research efforts (e.g., Erikson, 1996), psychologists and experimental economists expressed the fear that the expectation of being deceived produces suspicion and second-guessing, and that these psychological reactions rather than the experimenterís scenario and instructions guide, motivate, and ultimately distort experimental behavior. Examples of this concern are listed in Table 1.

[Insert Table 1]

The statements in Table 1 differ in one important aspect. Whereas some researchers attribute the contamination of the participant pool to <u>firsthand</u> experience with deception (i.e., participating and being debriefed in deception experiments; see Seeman, 1969), others assume that <u>vicarious</u> experience with deception (e.g., from undergraduate psychology training, campus scuttlebutt, media coverage of psychological research, and the disciplineís reputation more generally) suffices to engender in participants the expectation that they will be deceived (e.g., Orne, 1962; Ring, 1967; Adelson, 1969, Hey, 1991; Davis & Holt, 1993; Ledyard, 1995). This assumption is particularly common in experimental economics where deception is effectively outlawed, with the proscription typically being justified on the basis of the assumption that vicarious experience may make participants distrustful (e.g., see Hey, 1991; Davis & Holt, 1993, in Table 1). In the parlance of economists, participantsí expectation that they will not be deceived (i.e., that experimenters will be honest) is a common good such as air or water that would be depleted quickly if deception were allowed. Economists would fear the reputational spillover effects of deception even if only a few of their tribe practiced it, hence its strict proscription despite calls to relax this rule (e.g., Bonetti, 1998a,b).

We now turn to the first of the three arguments commonly advanced to defend the use of deception, namely, that its use is in decline.

How Prevalent is Deception?

If firsthand experience with deception is necessary to induce suspicion and second-guessing, then the argument that the use of deception has declined since its peak in the 1970s is important because of its implication that it is increasingly unlikely that participants have had firsthand experience with deception. Is there such a decrease? As mentioned, we found 14 studies (see Footnote 2) that analyzed the use of deception across a wide range of journals (deception is by no means confined to social psychology; see, for instance, Toy, Olsen, & Wright, 1989[4]). Here we focus on the results for the high ranked Journal of Personality and Social Psychology (JPSP) (and its predecessor, the Journal of Abnormal and Social Psychology, JASP), for which the most comprehensive and recent figures are available.

[Insert Figure 1]

Between the 1921s and 1948s, a period for which Nicks et al. (1997) analyzed the use of deception on a yearly basis, an average of 4.7% of the articles in JASP employed deception each year. Figure 1 shows the percentage of studies using deception reported in JPSP from 1948 to 1996. According to Nicks et al., the percentage rose steadily from 9% in 1948 to 50.7% in 1968, peaked in the 1970s (with 69% in 1975 according to Gross & Fleming, 1982), remained high in the early 1980s (with 58% in 1983; Adair et al., 1985) and declined to 31.3% in 1994. In an analysis of the same journal, Sieber et al. (1995) found that the percentage of deception studies dropped to 32% in 1986 but was back up to 47% in 1992. Continuing this analysis, Epley and Huff (1998) reported 42% in 1996.

Some of the fluctuations may reflect substantial changes in the applied methods (e.g., the initial upswing in the 1960s), ethical standards, and federal regulation of research; others may reflect different definitions of what constitutes deception (e.g., compare the more inclusive criteria employed by Sieber et al. with the criteria used by Nicks et al.).

Although these figures show a decline compared to the heyday of deception in the late 1960s and 1970s, they also demonstrate that the absolute level is still high: A conservative estimate is that every third study published in JPSP in the 1990s employed it (compared to 4.7% between 1921-1948). For other social psychology journals, such as the Journal of Experimental Social Psychology (JESP), the proportion appears to be even higher (e.g., Gross & Flemming, 1982; Nicks et al., 1997). As a consequence, the number of participants who have had and will have firsthand experience with deception is not at all negligible even today. Moreover, the fact that many psychology departments recruit participants almost exclusively from introductory courses (Sieber & Saks, 1989; Vitelli, 1988) and often require introductory students to participate in more than one study (within one semester) makes it likely that a significant number of first-semester students have firsthand experience with deception. Thus, in our view, we cannot dismiss the concerns about deceptive practices by pointing at its somewhat lower (yet high by some historical standards) level. Clearly, deception is still a relatively widespread practice.[5]

We now turn to the second argument in favor of deception, namely, that participants do not become resentful about having been fooled by researchers, and the effects of suspiciousness on research performance are negligible. In analyzing the empirical evidence, we distinguish between three classes of effects of deception, namely, effects on (1) attitudes and feelings, (2) beliefs and expectations, and (3) experimental performance.

Does Deception Affect Participantsí Attitudes and Feelings?

Does the ìthe preponderance of evidence suggests that deceived participants do not become resentful about having been fooled by researchersî (Kimmel, 1998, p. 804)? In fact, compared to participants in experiments without deception, participants in experiments involving deception reported more enjoyment, less boredom, and stronger belief in the scientific value of the research and in its educational benefit for them (Finney, 1987; Christensen, 1988; Smith & Richardson, 1983). Moreover, Sharpe, Adair, and Roese (1992, pp. 588-589) found that participantsí (post hoc) evaluations of their participation in psychology experiments were not affected by experience with deception, and that introductory psychology students endorsed the use of deception in psychological research. Several other authors also found that participants in experiments involving deception generally found it acceptable (Gerdes, 1979: Korn, 1987; Christensen, 1988) and did not respond negatively during debriefing to mild deception (Toy et al., 1989). Finally, psychology undergraduates rated deceptive methods as more acceptable than did graduate students and faculty (in philosopy and theology; Korn, 1987), and a majority of introductory psychology students reported viewing deception research (here Aschís conformity experiments) as ethically unproblematic and believing that its use is justified by the scientific purpose of the experiment (see also the findings reviewed in Smith & Berard, 1982, and Gerdes, 1979).

Summing up his review of research on the impact of deception on participants, Christensen (1988) concluded: ìThis review ... has consistently revealed that research participants do not perceive that they are harmed and do not seem to mind being misled. In fact, evidence exists suggesting that deception experiments are more enjoyable and

beneficial than nondeception experimentsî (p. 668). As a consequence, ìthe scale seems to be tilted in favor of continuing the use of deception in psychological researchî (p. 664). Along similar lines, Aitkenhead and Dordoy (1985) concluded that ìsubjects seem to be prepared to tolerate a fair degree of physical discomfort, deception and inconsiderate treatment in the interest of researchî (p. 300).

We object to these conclusions. In our view, this collection of findings sheds light on some important properties of current psychological experimentation but, for several reasons, cannot provide the justification for the continued use of deception. To begin with, some of the findings are simply orthogonal to the issue of deception. For instance, we agree with the argument that a study that keeps participants guessing about the purpose of the experiment may be a welcome relief from tedious, self-evident, or otherwise less interesting studies. In our view, the fact that many psychological experiments are of a ìmundane, repetitive natureî (Sharpe et al., 1992, p. 589; see also Coulter, 1986) cannot be used to justify the use of deception; instead, it should spur experimenters to reconsider participantsí incentive structures (see Argyris, 1968; Holmes & Appelbaum, 1970; Hertwig & Ortmann, in press)—that is, make experiments more interesting and provide participants with incentives to perform well in them.

In addition, the findings regarding participantsí attitudes and feelings are not as unequivocal as they were sometimes presented. For an illustration, consider the study by Smith and Richardson (1983), who concluded that ìthose participants who had been deceived evaluated their experience more positively than those who had not participated in deception experimentsî (p. 1075). As Rubin (1985) pointed out, Smith and Richardson (1983) also reported that 20% of the participants in their survey (undergraduate students

in introductory psychology classes) reported they had been in experiments that ìcaused them to feel very nervous, humiliated, excessive physical discomfort, very wrongly deceived and/or very angry,î (p. 1078), and that there is a correlation (albeit not perfect) between those feelings and participation in a deception experiment. Smith and Richardsonís finding is not unique: Oliansky (1991) also observed that participants exhibited severe negative emotions after being deceived during experiments and questioned whether undergraduate participants truthfully report their feelings about deception to experimenters (p. 256). Moreover, Fisher and Fyrberg (1994) reported that the majority of their students believed that participants in various published deception studies must have felt embarrassed, sad, or uncomfortable (see also Finney, 1987, p. 43). In one experiment, Allen (1983) found that only participants who had been deceived during the session ìrated the experiment as worthless, were annoyed with the experiment, and would not recommend the experiment to a friendî (p. 899; see also Straits, Wuebben, & Majka, 1972). Cook, Bean, Calder, Frey, Krovetz, and Reisman (1970, p. 189) found that participants with a history of deception studies considered experiments to be less scientific and less valuable and reported caring less about understanding and following experimental instructions. In addition, Epstein, Suedfeld, and Silverstein (1973) observed that deception is, next to danger to the participant, the most frequently mentioned reason to withdraw from an experiment.

To conclude: We could amass more studies. However, we believe that this would not change the conclusion that the evidence on how deception affects participantsí attitudes and feelings is mixed. This mixed set may indicate that feelings and attitudes concerning deception are a function of factors that have not been controlled for, such as

the type of deception (e.g., Lindsay & Adair, 1990), the experienced consequences of deception (e.g., the extent and type of harm, stress and discomfort experienced; Michaels & Oetting, 1979), the quality of debriefing, and the nature of the experiment (repetitiveness, incentive pay, etc.). For the sake of the argument, let us assume that some participants mind being misled, and others do not. This is disconcerting because there is evidence that performance data of participants with negative and positive experimental histories differ (Holmes & Appelbaum, 1970). Possibly even more important, however, is the argument that having a positive attitude about deception does not mean that participants accept the experimental scenario. It is not contradictory to appreciate the scientific utility of deception in the abstract, but nevertheless anticipate it and attempt not to be fooled.

We now turn to the effects of deception on beliefs and expectations.

Does Deception Affect Beliefs and Expectations?

Do participants in psychology experiments anticipate being deceived? To identify studies that explicitly probed expectation of deception in psychology experiments, we conducted a literature search. For this search and for those reported later, we used the PsycINFO/PsycLIT database, which covers the academic literature in psychology and related disciplines, including sociology and education, in the period between 1887 and July 1999 (at which point our analyses were conducted). We searched for the key word deception in combination with expectation(s) in all titles and abstracts and found a small set of five studies that explicitly investigated participantsí expectations regarding psychology experiments, such as the anticipated truthfulness of information provided. To quantify the findings reported, we calculated effect size measures where possible. The

effect size measure used in Table 2 is eta. Eta is defined as the square root of the proportion of variance accounted for (Rosenthal & Rosnow, 1991), and it is identical to the Pearson product-moment correlation coefficient when df = 1, as is the case when two conditions are compared (as in most cases where we calculated eta). According to Cohenís (1988) classification of effect sizes, a value of eta of .1, .3, and .5 constitutes a small, medium, and large effect size, respectively. Table 2 summarizes the results of the five studies included in our effect size analysis.

[Insert Table 2]

Two of the five studies asked participants to report what their specific expectations would be if they participated in future research (e.g., you will be misled or deceived in some way during the course of the study) and analyzed these expectations as a function of prior experience with deception (Epley & Huff, 1998; Krupat & Garonzik, 1994). The responses suggest that having experienced deception heightened participantsí expectation of being misled, deceived, and not being accurately informed about the experimental purpose. Several of the studies in Table 2 probed participantsí more abstract beliefs, such as the trustworthiness and honesty of psychologists. Here the results were mixed. Cook et al. (1970) reported that previously deceived participants were less inclined to believe experimenters. In other studies, participants with a history of deception differed only slightly or not at all from those who had no experience with deception (Smith & Richardson, 1983; Epley & Huff, 1998).

What do the results in Table 2 tell us about the effects vicarious experience with deception? Could the fact that participants with and without a deception history differ in their expectations be interpreted as meaning that vicarious experience may not suffice to

contaminate participantsí expectations? In fact, Sharpe et al.ís (1992) results reported in Table 2 seem to support this conclusion. These authors examined samples of introductory psychology students who had never participated in a psychology experiment prior to the survey and thus could have had only vicarious (if any) experience with deception. The samples disagreed with the assertion that psychology experiments ìalmost alwaysî involve deception. Sharpe et al. (1992) concluded that ìsubjects entering introductory psychology subject pools have not acquired attitudes from their predecessors or other sources that lead to greater suspicionî (p. 589). Note, however, that participants may have the veridical expectation that deception is used but not necessarily ìalmost always.î

Also, viewed in the context of other results this conclusion appears to be too sanguine. According to Rubin and Mooreís results (1971), for instance, it is not the number of deception experiments in which participants recall having taken part, but the number of psychology courses taken that is most closely related to suspicion. If so, then vicarious experience acquired during undergraduate psychology training (which participants in Sharpe et al.ís samples did not yet have) does matter. In fact, Higbee (1978) observed that students rated psychologists as being less truthful at the end of the semester than at the beginnng (eta = .51), and students with at least five psychology courses rated psychologists as being less truthful than students who had no previous psychology courses (eta = .43). He concluded that ìif psychologists expect the subjects to believe them, perhaps they should get the subjects at the beginning of the semesterî (Higbee, 1978, p. 133).

In sum, the results from this small set of studies suggest that firsthand experience with deception increases participantsí expectations of being deceived without necessarily

affecting their more abstract beliefs about psychologistsí trustworthiness (Epley & Huff, 1998, reported suspicion of experiments and suspicion of psychologists as not significantly correlated; but see also Krupat & Garonzik, 1994, about the framing of relevant questions).

Does Deception Affect Experimental Performance?

One of the most striking illustrations that expectations about deception can strongly affect behavior in an experiment was reported by MacCoun and Kerr (1987): In the middle of a mock jury study, one of the six jurors experienced a genuine epileptic seizure reminiscent of the feigned seizure that served as a manipulation in a classic study by Darley and LatanÈ (1968). MacCoun and Kerr (1987) reported that ìthree of the five subjects spontaneously reported that they had questioned the authenticity of the attackî (p. 199), and that ìthere were indications that prior knowledge of psychological researchóderived primarily from course workówas related to suspicionî (p. 199). While the only person who promptly came to the victimís aid had no prior psychology coursework, ìtwo of the other bystanders reported looking for hidden observers even as they joined in administering aidî (p. 199).

Had MacCoun and Kerrís study been concerned with altruistic behavior, then withholding help due to suspicion would have falsely confirmed the bystander effect (Darley & LatanÈ, 1968). Is this just an isolated example of a situation in which expectations about deception affect behavior and thereby potentially compromise experimental data? We can address this question by looking at three different sets of studies that examine how suspicion of deception affects experimental performance in a

wide range of studies examining judgments, attitudes, incidental learning, and verbal conditioning.

The first type of study we considered compared the experimental behavior (e.g., degree of conformity behavior) of participants who were identified post-experimentally as being either suspicious or unsuspicious of deception. The second set consists of studies that intentionally engendered suspicion of deception and examined experimental behavior as a function of expectations. In the third set of studies, participantsí experimental history (i.e., participation in deception studies) was either recorded or systematically manipulated and their behavior studied as a function of it. We found studies of these three types by searching the PsycINFO database for specific keywords in titles and abstracts (the keywords are listed below). We also included all studies cited in a recent review by Bonetti (1998a), who concluded from his sample of studies that ìdeception does not appear to ëjeopardize future experimentsí or ëcontaminate a subject poolíî (p. 389). Finally, we looked up the studies cited in the articles found using the first two methods and included them if they could be classified into one of the three sets.

Set 1: Post-experimental Identification of Suspicion and Its Effect on Experimental Performance

To find studies that examined the effects of post-experimental identification of suspicion, we searched for deception in combination with suspicion (and its variants, such as ìsuspicious, suspiciousness, suspicionsî). This search uncovered two systematic reviews of the social psychology literature (Stricker, 1967; Stang, 1976) that examined the proportions of participants who are suspicious. The studies Stricker (1967) reviewed,

with one exception (Jones & Jones, 1964), excluded suspicious participants and made no attempt to examine systematically how suspicion affected experimental behavior.[6]

In his review of the literature on social conformity, Stang (1976) found 21 studies that reported the percentage of ìsuspiciousî participants. Interestingly, for the reported period between 1954 and 1973, the percentage of suspicious participants increased as a function of time, that is, more recent studies observed more suspicious participants (r = .76), and about one-third of the studies reported that more than 50% of the participants were suspicious. Note that the positive correlation between year of study and percentage of suspicious participants tracks the increase of the use of deception in social psychology during that time (see Figure 1).

Out of the 21 studies, Stang (p. 363) specifically cited 9 that systematically compared the behavior of suspicious and unsuspicious participants. Typically, this classification was performed on the basis of post-experimental interviews in which participants responded to questions such as ìDo you feel this experiment was deceptive (involved lying) in any way?î (Geller & Endler 1973, p. 49). In addition to those nine studies referenced by Stang, our search turned up another five studies that examined behavior as a function of suspicion, all of which were also concerned with conformity behavior. It is probably no coincidence that researchers studying conformity have been particularly concerned with the possible repercussions of suspicion. According to Gross and Fleming (1982), researchers in this area rely heavily on deception, with 96.7% of studies in the area of compliance and conformity having used deception.

[Insert Table 3]

As shown in Table 3, in 10 of the 14 studies (71%) identified by Stang and our additional search, suspicious participants conformed less than unsuspicious participants. For 9 of the 10 studies (in which the necessary information was given) we calculated an effect size measure (eta, or r); the reduction in conformity due to suspicion was of medium to large effect size. In four studies (Chipman, 1966; Willis & Willis, 1970; Endler et al., 1972; Wiesenthal et al., 1973), suspicion did not significantly change the amount of conformity behavior. No study reported that suspicion produced greater conformity.

To conclude, in the conformity studies reviewed here, almost half of the participants (46%) believed that the experiment involved lying in some way. Moreover, suspicion influenced participantsí experimental performance. Compared to unsuspicious participants, suspicious participants conformed less in over two-thirds of the studies analyzed. The reduction of suspicion was of medium to large effect size.

Set 2: Ex-Ante Manipulation of Suspicion and Its Effect on Experimental Performance

In the previous set of studies, suspicion of deception was recorded after participants performed the experimental task. Alternatively, experimenters can engender participantsí suspicion from the outset and study their subsequent performance as a function of it. To find such studies, we used the search term deception in combination with prebriefing, or forewarning. We found eight studies. Note that the issue with which we are concerned here, namely, the effect of experimentally induced suspicion, was not the explicit focus of all studies. Participantsí knowledge and thus suspicion ranged from relatively neutral forewarning about experimental procedures in general (e.g., Allen, 1983, p. 901: ìin a few experiments it is necessary for experimenters to deceive subjects concerning some

elements of the experimentî) to confederate tip-offs (e.g., Levy, 1967) and disclosure that deception would occur during the experiment (e.g., Finney, 1987).

[Insert Table 4]

Table 4 summarizes how these instructions affected behavior. The results are mixed, with some studies finding no effect and others large effects. Nevertheless, a trend is discernable. When participants received detailed tip-offs about the true purpose of the experiment (e.g., Levy, 1967; Turner & Simons, 1974), were explicitly told that they would be deceived (Finney, 1987), or explicitly acknowledged awareness of experimental manipulation (Golding & Lichtenstein, 1970), suspicion altered experimental performance (albeit not necessarily on all dependent measures). In contrast, when participants were merely informed that some kind of deception might happen (Allen, 1983; Finney, 1987; Wiener & Erker, 1986) or were told the purpose of the study (without indicating the possibility of deception, Gallo et al, 1973) then their performance did not differ from that of control participants not given this information (but see Spinner et al., 1977).

There are two interpretations of these results. First, one could conclude that specific and certain knowledge about deception alters experimental performance, whereas general and uncertain anticipation of deception does not. Finney (1987), however, had another suggestion for why uncertain anticipation (ìmay be deceived,î see Table 4) did not seem to differ from the control condition (with no information). In his view, ìone might speculate that this information merely reaffirms subjectsí prior belief that deception may occur in an experiment and, therefore, causes no change in their anticipationî (p. 45) compared to the control condition that has the same prior belief.

That (at least some) participants have such strong prior beliefs became painfully clear in the study by Gallo et al. (1973), who reported that even in a condition in which

> subjects were told point blank that they were in a conformity experiment, quite a few speculated in the open-end question that the experiment was <u>really</u> a study of distance perception and that the material about being a conformity experiment was put in to throw them off the track! This type of response indicates that psychologists are disbelieved even in those rare situations in which they are caught telling the truth. (p. 106)

At the same time, even without information more than half of the participants in Gallo et al. (1973) expressed suspicion.

In sum: The evidence suggests that specific and certain knowledge about deception can alter experimental performance. The effect of general and uncertain anticipation of deception is less clear.

<u>Set 3: Experimental History and Its Effect on Experimental Performance</u>

Yet another way to explore the effects of suspicion is to study how participantsí experimental history affects experimental performance. To find studies that explored this question, we used the search term <u>deception</u> in combination with <u>experimental</u> <u>history</u> and found nine studies. Table 5 summarizes their findings.

[Insert Table 5]

In brief, the results suggest that firsthand experience with deception or manipulation affects performance, while mere disclosure of the possibility of deception in psychological experiments does not (Cook & Perrin, 1971; Christensen, 1977, Experiments 1 and 2). Second, Silverman et al. (1970) observed that the experience with deception appears to

make people more apprehensive of evaluation. Third, the studies by Fillenbaum (1966) and Fillenbaum and Frey (1970) caution that not all suspicious participants act upon their suspicion. Fourth, different dependent variables seem to be differentially affected by the experience with deception. In Cook and Perrinís (1971) research, incidental-learning data differed as a function of experimental history, but attitude data did not (but see Experiment 2 in Cook et al., 1970). Finally, the extent to which previous deception experience transfers to other experiments may depend on the similarity between the past and present experimental situation (Brock & Becker, 1966; Cook et al., 1970).

Page and Scheidt (1971) reported a dramatic example involving the ìweapons effect,î which illustrates how past experience with laboratory deception can distort behavior so extremely that it elicits a phenomenon that ìcannot be generalized to nonlaboratory situationsî (p. 304).[7] Page and Scheidt were able to replicate the weapons effect in only one out of three of their experiments, and only in a group of participants who had taken part in a deception experiment within the previous month; participants unfamiliar with psychological experimentation never exhibited the effect. Turner and Simons (1974; see also Simons & Turner, 1976) challenged Page and Scheidtís results, and based on them Turner, Simons, Berkowitz, and Frodi (1977) came to the opposite conclusion: ìPerhaps the failures to replicate the weapons effect occurred because the researchers used subjects who were not naÔve about deception or who were very apprehensive about the impression they might createî (p. 369). Although Page and Scheidt (1971) and Turner et al. (1977) disagree over the issue of how experience with deception alters experimental performance, they agree that is does have this potential.

Turner and Simons (1974) concluded: ìApparently, unless subjects are naive, the effects of important independent variables may be obscuredî (p. 347).

Other Behavioral Evidence

There is still another category of studies that have sometimes been interpreted as reflecting behavioral effects of the use of deception. Based on both systematic and incidental observations in deception experiments, it has been reported that participants who have privileged knowledge during an experiment (e.g., about the fact that deception is being used; e.g., Levy, 1967; Altemeyer, 1971, Newberry, 1973; Taylor & Shepperd, 1996) tend not to admit to having had it when later questioned, and that they sometimes disclose information to other participants although they earlier agreed to withhold it (e.g., Wuebben, 1967; Lichtenstein, 1970; Lipton & Garza, 1978; but see Aronson, 1966). Although these results surely undermine the assumption that participants are cooperative (e.g., Orne, 1962; Fillenbaum, 1966; Brˆder, 1998) and the hope that reputational spill-over effects can be limited, they do not necessarily imply that experimentersí duplicity causes participants to sabotage psychological research (as proposed by Schultz, 1969). To draw this conclusion we would need to know the baseline of cooperative behavior among participants. Unfortunately, results from psychology give us little guidance.

Here results from experimental economics can help. Elsewhere, we have modeled the structure of the social situation ìexperimentî game-theoretically (Ortmann & Hertwig, 2000). From this perspective, the situation often involves intragroup games among participants in experiments and intragroup games among experimental economists that are similar to social dilemma games. Furthermore, the social situation experiment always involves a classic principal-agent game, in which the agent (i.e., experimenter) can choose

either to deceive the principal (i.e., participant) or to be truthful about the setting of the experiment. The principal (the participant) in turn can choose to trust the experimenter or to doubt the experimenter. For all these games, experimental economists have compiled massive experimental evidence (see Ledyard, 1995, for a review of the evidence on social dilemma games, and Ortmann and Colander, 1997, for a primer and brief review of the evidence on principal-agent games). While the findings suggest that people are too cooperative according to game-theoretic predictions for one-off and finitely repeated games (under complete information), they also demonstrate that people more often than not are uncooperative. Particularly relevant for the present context are recent results by Dickhaut, Hubbard, and McCabe (1995), whose manuscript title ìTrust, reciprocity and interpersonal history: Fool me once, shame on you, fool me twice, shame on meî is self-explanatory.

Although it only rarely makes it to the public domain, another category of evidence is researchersí unprompted observations. One example of such an unprompted observation is MacCoun and Kerrís (1987) report (described earlier). Is their observation just a rare exception or the tip of an iceberg? We do not know. We are, however, surprised by how many of our colleagues have made unprompted observations, ranging from comments on participantsí distrust about the promised performance-contingent payment to their distrust of crucial parameters in gambles to their conviction that some noise outside of the laboratory is systematically related to the current experiment. These unprompted observations illustrate that there are myriad ways in which suspicion can seep into an experiment, and that it may not only directly affect experimental performance but

also indirectly, for instance, by causing participants to doubt the promise of performance-contingent financial incentives.

Conclusion

We analyzed three sets of empirical studies that explored the effects of suspicion of deception on participantsí behavior. There is strong evidence that in research on conformity, participantsí suspicion of deception compromises the results obtained (see Table 3). In addition, there is also evidence that the specific expectations of deception (e.g., after being forewarned or prebriefed) can systematically changes experimental performance (see Table 4). Finally, we found evidence that participantsí experimental history (i.e., having firsthand experienced deception in past experiments) also affects behavior (see Table 5). The results from all three lines of research converge to the same conclusion: More often than not, but not always experience with deception affects behavior (e.g., judgments, attitudes, and measures of incidental learning and verbal conditioning).

Why did some studies not observe biasing effects of deception? One reason may simply be that experimental scenarios differ in the extent to which they elicit participantsí suspicion and second-guessing, and that dependent variables differ in the extent to which they provide room for biasing effects of suspicion. A second reason relates to the question of determining participantsí prior expectations (see Gallo et al, 1973; Finney, 1987) in the baseline group. All studies reported in Tables 4 and 5 explored the effects of deception by comparing different groups of participants (e.g., those with and without the instruction that deception may occur). In doing so, they assumed that one group is the baseline group and effects of deception exist to the extent that the experimental

performance of participants in the treatment groups differ from that in the baseline group. If, however, even in the baseline group participants have prior expectations of being deceived (e.g., due to introductory psychology classes), then performance differences between control group and experimental group will be diminished and thus the effects of deception underestimated.[8]

Most of the studies included in our analysis date back to the decade between the mid-1960s and the mid-1970s. This is no coincidence. Silverman (1978, p. 405) referred to this period as the ìmost self-critical decadeî of psychology, during which much research was devoted to investigating the ìthreats to validity that reside in Ö the interaction between the experimenter and the subjectî (Rosenthal & Rosnow, 1991, p. 110). Silverman (1978) argued that the revelations of this decade nevertheless ìseem to have had minimal effects on our traditions and practicesî (p. 405). Are these revelations obsolete today? We think the answer is no. For one, although deception is lower than during the 1970s, our review demonstrated that it is still frequently used. To the extent that participantsí expectations and degree of suspicion reflect this current research practice, the threat of data contamination remains. Moreover, studies published in the 1980s and 1990s generally support the conclusions based on this earlier research (e.g., MacCoun & Kerr, 1987; Krupat & Garonzik, 1994; Epley & Huff, 1998).

DeceptionóAn Indispensable Tool?

The most common argument for using deception suggests that it is indispensable for the study of those facets of behavior that are of great social importance, and for which alternative research methods are either unavailable or would produce invalid data. By this argument, the costs of <u>not</u> conducting such research (e.g., on conformity, obedience,

racial stereotypes, bystander effect, and aggression) outweigh the costs of using deception (e.g., Trice, 1986).

How Indispensable is Indispensable?

According to the American Psychological Association (APA) rules of conduct, ìpsychologists do not conduct a study involving deception unless they have determined that the use of deceptive techniques is justified by the studyís prospective scientific, educational, or applied value and that equally effective alternative procedures that do not use deception are not feasibleî (APA, 1992, p. 1609). In other words, the APA rules allow that deception may be indispensable under certain circumstances but treat it as a last-resort strategy to be used only if its benefits justify its use and if no alternatives are feasible.

How effective is this rule in regulating the use of deception, that is, to make sure that deception is indeed employed only as a last resort? Not effective at all. To arrive at this conclusion, it suffices to browse through recent issues of leading journals in social and experimental psychology. Participants are routinely misled to believe that their decisions in games and gambles will determine their final payoffs, that assignment of roles in an experiment will be determined by chance, that they will be paired up with an another person, that the feedback they receive will be veridical, or that information they provide will be made public and so on. These are just a few arbitrary examples. But none of these false claims seemed indispensable.

In addition, even in research traditions in which deception has been considered to be indispensable, alternative research techniques are often available. This follows from Gross and Flemingís (1982) review of 1188 journal articles in leading social psychology journals

(between 1959 and 1979). This review analyzed the prevalence of deception in 24 research areas in social psychology (e.g., conformity, altruism, impression formation, attitude change). The authors observed a wide variation in how often deception was used in different areas. Researchers in about half of the areas used deception in half or less than half of all studies. That is, in those areas alternatives must have always been available. Even in research areas such as conformity and compliance, which according to Gross and Fleming (1982) used deception in 96.7% of all studies, alternatives very likely are available. Note that according to Stricker et al. (1969) one-fifth of all conformity studies published in four leading social psychology journals in 1964 did not use deception.

In our view, the frequency of deception studies would likely decrease substantially if deception were truly used only as a last-resort strategy. But there are still other ways of reducing the frequency of deception studies, one of which has been inadvertently proposed by critics of role-playing studies who prefer to use deception. Such critics have suggested that role playing could be used to help generate experimental hypotheses (e.g., Freedman, 1969), which opens up the interesting possibility of using deception in the context of theory justification but not in the context of discovery. This idea is consistent with Wallstenís suggestion (personal communication) that the decision about the use of deception should not depend on whether a particular study can be conducted or a particular topic investigated. Rather, the question ought to be whether the theory under consideration can be investigated without the use of deception. This approach of linking methodological choices to theoretical progress rather than to sheer feasibility strikes us as being desirable.

Are There Alternatives to Deception?

Contrary to the claim that few (if any) alternatives to deception exist (e.g., Miller, 1972; Krupat & Garonzik, 1994), a number of alternatives have been proposed. In his review of alternatives to deception, Geller (1982) described three typesónaturalistic observation or experimentation, deception-free laboratory experiments, and simulations (e.g., field simulations, role-playing)óand provided illustrative examples of each type. In addition, Rubin (1973) listed several nonexperimental substitutes for deception, such as naturalistic observation, social surveys, and archival data; within the realm of experiments in social psychology, he identified four ìhonest strategies,î which are not deceptive and involve little or no invasion of privacy (see Rubin, 1973, for actual studies using these strategies). Moreover, the implementation of confidentiality and anonymity can also be used as alternatives to deception. There is evidence that the promise of confidentiality and the implementation of anonymity can be effective in dealing with the problem of social desirability responses (e.g., Esposito, Agard, & Rosnow, 1984; Ceci & Peters, 1984; Hoffman, McCabe, & Smith, 1996). The dilemma is that these two methods are only effective to the extent that participants trust the experimenters, and thus the use of deception may compromise these alternative techniques.

Alternatives to deception are not lacking. What is lacking, however, are incentives to develop, evaluate, and employ them. As Baumrind (1971) put it:

> Many of the investigators who choose to use Machiavellian means in experimental settings are brilliant and creative methodologists. The likelihood is that if such men knew that in order to investigate experimentally an area in which they were interested they would have to revise their research strategy, they would Ö be

capable of inventing new experimental methods that were well controlled as well as humane. (p. 893)

Indeed, given that the alleged last-resort strategy of deception is widely accepted, why bother develop an experimental methodology that would allow us to do without?

When Are Alternatives Equally Effective?

In evaluating alternative methods, the bone of contention is the definition of what are equally effective alternatives. The controversy over the use of role playing illustrates this crucial discussion. The critics of role playing have proposed that deception studies are the standard against which role playing studies should be compared, and that the data should be considered valid to the extent that they replicate those observed in deception studies (e.g., Freedman, 1969; Miller, 1972). What is the validity of data obtained from role playing studies by this standard? Greenberg and Folger (1988; see also Geller, 1982; Greenwood, 1983) reviewed a representative sample of studies, including role-playing replications of classic deception experiments on conformity, compliance, obedience, and aggression and found mixed results. Whereas some studies provided a ìdramatic demonstration of agreement between role-playing and deceptionî (Greenberg & Folger, 1988, p. 54; e.g., Mixonís 1972 and Gellerís 1978 replication of Milgramís obedience research), other studies suggested that role-playing participants reproduced simple main behavioral effects but not the complicated patterns of interaction observed among deceived participants.

For those who share the assumption that deception studies provide the ìtrueî results, these results invalidated role playing as a research alternative to deception (e.g., Miller, 1972). However, for those who assume that the results of deception studies are

compromised by methodological flaws (e.g., participantsí suspicion), deception studies do not deserve to be considered the norm or baseline of human behavior (e.g., Forward, Canter, & Kirsch, 1976; Diener & Crandall, 1978; Greenwood, 1983). According to Greenwood (1983), for instance, role-playing studies should be evaluated relative to experiments, with or without deception, in which the ìexperimental manipulation created the intended subject definitions of the situationî (p. 245). In this view, both role playing and deception ought to be subjected to evaluation, and deception studies do not have a superior epistemological status.

This discussion is crucial because the assessment of alternative methods depends critically on the standards of comparison. In our view, two points ought to be kept in mind in this discussion: First, many results in social and cognitive psychology (and in other areas of psychology) are highly susceptible to even minor variations in experimental methods (e.g., in instructions and representation of information). Thus, replication of main effects and complicated patterns of interaction obtained using deceptive methods is a very strict criterion by which to judge nondeceptive methods. Second, to know exactly how strict this criterion is, we would need to know the base rate of replication across deception studies. Exact replication of empirical results is not known to be a frequent event (e.g., Meehl, 1978; Rosenthal, 1990). Therefore, even deception studies might not be able to meet the high standard imposed on nondeceptive studies.

Conclusions

We have discussed three arguments often advanced to justify the (continued) use of deception in psychology. These arguments are, first, that the use of deception has declined; second, that the effects of participant suspicion on behavioral performance

appear to be negligible; and, third, that deception is indispensable to the study of socially significant behavior. We found that although deception is less frequently used than in its heyday, its absolute prevalence in the 1990s is still high (e.g., more than 30% of articles in JPSP employed deception) and substantially higher than in the 1940s and 1950s. In addition, we found that suspicion and prior experience with deception can alter experimental performance in conformity, verbal conditioning, aggressiveness, and incidental-learning studies. In the samples of studies we examined, the size of these distorting effects is of medium to large size. Finally, although they are less developed and less often evaluated, alternative methods to deception exist. Although the empirical data are sometimes mixed, we conclude that the evidence we reviewed is not consistent with the three arguments advanced to justify deception.

There are at least three possible responses to these findings. The first is to continue doing business as usual. However, if the use of deception is based on the three arguments above, then the methodological costs documented here seriously undermine this approach. The second response is to adopt the policy of experimental economists, namely, to outlaw the use of deception. We believe that this strategy has important benefits—namely, increasing participantsí trust in experimental scenarios, thus reducing suspicion and second-guessing of the true purpose of the experiment, and ultimately, increasing experimental control and replicability. However, the empirical evidence we amassed here is probably insufficient to convince researchers to abandon a widely used and powerful research tool. Specifically, we did not find clear evidence for the prediction that vicarious experience with deception would lead to widespread reputational spillover effects. While vicarious experience in the form of undergraduate training appears to be a source of

suspicion, it is not clear that undergraduates enter college expecting to be deceived in psychological experiments.

We therefore advocate a third response, namely, to implement rules of conduct that effectively make deception a last-resort strategy (as currently stipulated by the APA guidelines). Even a cursory glance at contemporary deception studies reveals that deception is not treated as a last resort (recall that every third study in JPSP uses deception). In Kelmanís (1968) view, in the 1960s deception was used ìnot as a last resort, but as a matter of course.î Although the APA rules of conduct are considerably stricter now than in the 1960s, and have successfully reduced the severity of deceptive techniques, the rules have not changed the somewhat cavalier manner in which deception is used in psychology. In the words of a colleague, himself an eminent social psychologist, deception is still treated as a ìfirst-resort strategy.î

Why are the APA rules of conduct not more effective? The rules clearly stipulate that psychologists should not conduct studies involving deception unless they have determined that it is justified by its anticipated utility. This is a nice theoretical argument. Unfortunately, it also seems to be difficult to implement. The key problem is that the cost-benefit analysis is typically done by the experimenters themselves. Notwithstanding the mediating role of institutional review boards (which tend to focus on the ethical rather than the methodological consequences of deception) this practice leaves the assessment of private benefits (e.g., relative quick publication, see Adelson, 1969) and public costs (contamination of the participant pool) to an interested party (the experimenter)—a classic moral hazard problem.

We therefore propose a pragmatic solution, made possible by recent technological advances, that would give both defenders and critics of deception the opportunity to discuss on a case-by-case basis whether deception is necessary. Specifically, we propose that experimenters about to perform deception studies post their experimental designs on an APA website for a specified time period, thus giving those opposed to deception a chance to suggest workable alternatives.[9] Such a procedure might spur a spirited case-by-case debate about the necessity of deception and, ultimately, methodological innovation. In addition, such a website will over time build up a list of successful alternatives and examples of experiments in which they were used so that experimenters considering deception can easily ìbrowseî through alternatives.

We expect that such a pragmatic approach, in combination with the proposal to limit experimental deception to studies that test theories (while using other methods in the context of discovery), can dramatically reduce the use of deception. In addition, we propose that the deception studies that remain be designed to also enable an empirical assessment of the methodological necessity (e.g., by running a deception and a nondeception condition using the same experimental design; see Vinacke, 1954, whose respective proposal has rarely been realized) and the methodological consequences of deception (see studies in Table 5). This practice would give rise to a database that would eventually enable psychologists—defenders and critics of deception alike—to precisely assess its methodological costs.

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Footnotes

1 We note that ethical questions are not divorced from methodological ones. For example, the moral thing to do with deceived participants is to debrief them as quickly as possible after completing the experiment. Each time this moral imperative (which the APA has endorsed) is met, however, the general impression that psychologists commonly deceive is strengthened (Tesch, 1977).

2 They are Stricker (1967), Seeman (1969), Carlson (1971), Menges (1973), Levenson, Gray, and Ingram (1976), Krupat (1977), McNamara and Woods (1977), Gross and Flemming (1982), Adair, Duschenko, and Lindsay (1985), Vitelli (1988), Toy, Olsen, and Wright (1989), Sieber, Iannuzzo, and Rodriguez (1995), Nicks, Korn, and Mainieri (1997), and Eply and Huff (1998). Adair et al. (1985), for instance, defined deception as ìthe provision of information that actively misled subjects regarding some aspect of the studyî (p. 62). Nicks et al. (1997) defined deception as an ìexplicit misstatement of factî (p. 70), and Menges (1973) described deception as instances where ìthe subject is given misleading or erroneous informationî (p. 1032).

3 There is a completely different approach to the definitional issue. While most define deception on the basis of the experimenterís behavior (e.g., intentionally providing false information), one could define it alternatively on the basis of how participants perceive the experimenterís behavior. According to such a definition, deception would have occurred if participants, after being completely debriefed, had perceived themselves as being misled. Such an approach defines deception empirically and post hoc rather than on the basis of abstract principles.

4 To test the often-heard belief that deception is almost exclusively used in social psychology experiments, we conducted a small-scale survey in which we asked researchers in the field of behavioral decision making how often they used deception in their research practice (Hertwig & Ortmann, 1999). A total of 26 researchers responded; the sample encompassed well-established as well as young researchers. They estimated that, on average, they used deception in 1.7 out of 10 experiments. It was particularly interesting to observe that the researchersí opinions about the use of deception varied widely within one field. While 60% percent of the researchers stated that they never use it, 20% use it in half or more of their experiments. One of those who strictly opposed deception argued: ìI think that people who use deception often end up deceiving themselves. People see through the deception in many cases, and in others, they don't understand the task. The idea that ëtrueí behavior is found only with deception is a silly idea. The E and the S must communicate, and I think the best way to do that is to be VERY clear in giving instructions.î

5 Although deception is still widely used, ethical guidelines for research have become stricter (for a short history of ìten commandments of the APAî see Rosnow & Rosenthal, 1997, chapter 6), and as a consequence, the profession has been able to prevent drastic instances of deception. Rosnow and Rosenthal (1997), for instance, concluded that, ìmany of the seminal studies that were conducted then would be impossible today (e.g., Milgramís obedience studies)î (p. 114). We agree with this conclusion, notwithstanding evidence that emotional distress caused by less severe deception practice can still be substantial (e.g., Oliansky, 1991).

6 Stricker (1967) surveyed all articles using deception that were published in four leading social psychology journals in 1964. Of the 88 deception studies, only 16 attempted to quantify the degree of participantsí suspicion of deception. In this subset, the median percentage of ìsuspiciousî participants was 4%, with a range from 0% to 23%. Stricker, Messick, and Jackson (1969) later suggested that ìit seems very likely that the overall rate of actual suspicion in these 16 studies was seriously underestimatedî (p. 345) due to the inadequacy of the suspicion criteria used. Using different criteria, later studies found much larger numbers of suspicious participants (see Table 3).

7 The ìweapons effectî (originally reported by Berkowitz & Le Page, 1967) suggests that weapons might stimulate aggression by classical conditioning processes resulting from learned associations between aggressive acts and weapons.

8 Also participantsí motives may contribute to the mixed results. In research on the ìsocial psychology of the psychological experimentî (see Rosenthal & Rosnow, 1991, chapter 6), various motivations that participants may possess have been proposed. According to Rosenthal and Rosnow (1991), evaluation apprehension, that is, the motivation to ìlook goodî is ìlikely to emerge as the predominant motivation of many subjectsî (p. 118). However, how to put oneís best foot forward is not a simple question, that is, this is a single motivation that may be satisfied in myriad ways. For instance, does one make a favorable impression by succumbing to suspected deception and manipulation, thus cooperating with the experimenter; by ìfiguring outî the deception and thwarting it; or by trying to appear smart, efficient, and competent regardless of the research question?

9 To make such an approach work, a few rules of the game may need to be spelled out: For instance, to provide an incentive for critics of deception to come up with alternative designs, they should receive credit for it in the published articles. There is incentive for defenders of deception as well: By making key aspects of a planned experiment public, these researchers would be ìgoing on recordî first with an idea, preempting others following a similar line.

Figure Captions

Figure 1. The proportion of studies employing deception in the Journal of Personality and Social Psychology and its predecessor Journal of Abnormal and Social Psychology between 1948 and 1996. Note that if more than one number per year was available, the average proportion was calculated (Source: Levenson et al., 1976; McNamara & Woods, 1977, Gross & Flemming, 1982; Adair et al. 1985; Sieber et al. 1995; Nicks et al. 1997; and Eply & Huff, 1998).

Table 1. A sample of conclusions from psychologists and experimental economists regarding the negative effects of deception.

Orne (1962, pp. 778-779). [The use of deception] on the part of psychologists is so widely known in the college population that even if a psychologist is honest with the subject, more often than not he will be distrusted. As one subject pithily put it, ìPsychologists always lie!î This bit of paranoia has some support in reality.Ring (1967, p. 118). What is the perceptive student to think, finally, of a field where the most renowned researchers apparently get their kicks from practicing sometimes unnecessary and frequently crass deceptions on their unsuspecting subjects? The short-run gains may be considerable, but it does not appear chimerical to suggest that the ultimate price of deception experiments may be the creation of extremely mistrustful and hostile subject pools. It would be ironic indeed if, by their very style of research, social psychologists were to put themselves out of business.Argyris (1968, p. 187). Many experiments have been reported where it was crucial to deceive the students. [Ö.] One result that has occurred is that students now come to experiments expecting to be tricked. The initial romance and challenge of being subjects has left them and they are now beginning to behave like lower level employees in companies. Their big challenge is to guess the deception (beat the management). If one likes the experimenter, then he cooperates. If he does not, he may enjoy botching the works with such great skill that the experimenter is not aware of this behavior.Adelson (1969, p. 220). When the campus population learns, as it can hardly fail to do, about the common tendency of psychologists to deceive, so that all kinds of unanticipated, unknown expectations enter the experimental situation, the subject aiming to ìpsychî the experimenterís ìpsychingî of him, subject and experimenter entangled in a web of mutual suspicion, mutual deception.Seeman (1969, pp.

1025-1026). When a subject has once participated in a study using deception he is no longer a naive subject but a sophisticated subject who brings to subsequent studies a variety of personal theories and hypotheses that guide the behavior of the subject quite as decisively as theories and hypotheses guide the behavior of an experimenter. In view of the frequency with which deception is used in research we may soon be reaching a point where we no longer have naive subjects, but only naive experimenters. It is an ironic fact that the use of deception, which is intended to control the experimental environment, may serve only to contaminate it. Hey (1991, pp. 21, 119, 173, 225). I feel that it is crucially important that economics experiments actually do what they say they do and that subjects believe this. I would not like to see experiments in economics degenerate to the state witnessed in some areas of experimental psychology where it is common knowledge that the experimenters says one thing and do another. [Subjects] believing what the experimenters tells them...seems to me to be of paramount importance: once subjects start to distrust the experimenter, then the tight control that is needed is lost.Davis and Holt (1993, pp. 23-24). The researcher should...be careful to avoid deceiving participants. Most economists are very concerned about developing and maintaining a reputation among the student population for honesty in order to ensure that subject actions are motivated by the induced monetary rewards rather than by psychological reactions to suspected manipulation. Subjects may suspect deception if it is present. Moreover, even if subjects fail to detect deception within a session, it may jeopardize future experiments if the subjects ever find out that they were deceived and report this information to their friends. Ledyard (1995, p. 134). It is believed by many undergraduates that psychologists are intentionally deceptive in most experiments. If undergraduates believe the same about economics, we have lost control. It

is for this reason that modern experimental economists have been carefully nurturing a reputation for absolute honesty in all their experiments. Table 2. Effects of participantsí history with deception experimenters on beliefs and expectations.

StudyConditionsQuestionsResults (Effect Sizea)Cook et al., 1970Participants completed a questionnaire on social psychology experiments after a history of five versus no deception experimentsQuestions concerning global attitudes about experimenters, experiments, and about cooperation in experimentsParticipants with deception history believed experimenters less (p < .10)Smith & Richardson, 1983Participants, who had participated in psychology experiments, responded to a questionnaire that also included a postexperimental trustworthiness rating of psychologists ìBased on your experiences in psychology experiments, how trustworthy do you feel psychologists are?îThose participants who had been in deception experiments tended to perceive psychologists as less trustworthy than did nondeceived participants (eta = .09).Sharpe, Adair, & Roese, 1992Three groups of students received a questionnaire that included an item relating to the use of deception: a 1970 and 1989 sample of introductory psychology students who had not participated in any psychology experiment prior to the survey; and a 1990 sample of students who had participated in an average of 3.5 psychology experiments. On average, merely 12% of the studies in which they participated involved any deceptionìExperiments in psychology almost always involve deception or ëtrickingí the subject in some way.îThe 1989 pre- and 1990 postparticipation samples more strongly agreed with this statement (M = 1.5, 1.45) than the 1970 sample (M = 1.1; eta = .14). All three samples, however, fell on the ìdisagreeî side of the scaleEpley & Huff, 1998After the experimental session, half of the participants were given a full debriefing regarding false performance feedback, whereas the other half were

not debriefed regarding deception. Immediately after and in a 3-month follow-up, participants responded to various questions, including one probing their expectations ìAs a participant in future research, I will not be suspicious of any information presented by the experimenter.î ìPsychologists are trustworthy and honest.îAfter being debriefed, participants reported more future suspicion than those participants who did not know that they were participating in a deception experiment (eta = .36). The suspicion was maintained in the 3-month follow-up after the original deception experience. The item probing suspicion about psychologistsí honesty did not show an effect, and it did not significantly correlate with the first item Krupat & Garonzik, 1994 Psychology students were recruited from introductory, intermediate, and advanced level courses (n = 255). As a function of exposure to experimental deception, participants were then grouped into two categories: no exposure vs. one or more.If you were asked to participate in a psychology study, would you expect that: ìYou will be misled or deceived in some way during the course of the study.î ìThe purpose of the study would be clearly and accurately explained to you at the beginning of the study.îWith previous exposure to deception, participants were more likely to expect to be misled/ deceived (eta = .41), and that the purpose of the study would not be accurately explained (eta = .22) a Effect sizes calculated (Rosenthal & Rosnow, 1991) when sufficient information was available (search words: ìDeceptionî and ìexpectationsî).

Table 3. The effects of suspicion on experimental performance.

AuthorsProportion of Suspicious ParticipantsExperimental Performance of Suspicious Participants (Effect Sizeb)Allen, 196630 out of 120 (25%) Less conformity: On a maximum score of 100% conformity, unsuspicious participants scored on average 26% and suspicious participants 12%Stricker, Messick, & Jackson, 196738.6% (averaged across sex and

suspicion about various aspects of the experiment; Table 1) Less conformity: r = .49 (Table 4), r = .33 (Table 5); averaged across sex and measures of conformity Glinski, Glinski, & Slatin, 1970Sessions 1 and 2: 42 out of 55 (76%)Less conformity: r = .89 (Session 1), r = .86 (Session 2) Ettinger, Marino, Endler, Geller & Natziuk, 197115 out of 40 (38%)Less conformity: eta = .33Endler, Wiesenthal & Geller, 1972No dataNo difference in conformity (no ìsignificantî main effect)Endler & Hartley, 197314 out of 40 (35%)Less conformity: eta = .31Geller & Endler, 197328 out of 54 (52%)ìOnce subjects become suspicious, their conformity sharply decreasesî (p. 52): eta = .6Geller, Endler, & Wiesenthal, 197321 out of 61 (34%) Less conformity: eta = .33Wiesenthal, Endler & Geller, 197396 out of 116 (83)No difference in conformity (non-significant t-test)Chipman, 1966a19 out of 68 (28%)No significant difference in conformityWillis & Willis, 1970a54.2% Little to no effectRubin & Moore, 1971a95 out of 142 (67%) were either medium or highly suspiciousLess conformity: r = -.42Adair, 1972a38 out of 86 (44%)Less conformity: eta = .21Stang, 1976a13 of of 65 (20%)Less conformity: eta = .3; ìësignificantí treatment effects on conformity only when suspicious Ss were removed from the analysesî (p. 353)a Obtained from our literature search (search words: ìdeceptionî and ìsuspicionî and its variants); articles with no index stem from Stangís (1976) review.

b Effect sizes calculated (eta, biserial correlation r) when sufficient information was available.

Table 4. The effects of anticipation of deception on experimental performance.

AuthorsResearch TopicManipulationBehavioral Effects (Effect Sizea) Levy, 1967Verbal conditioningTwo groups of participants: Fully informed (tipped-off by a confederate) and uninformedGroups differed in the level of performance (eta = .41) but no significant differences in the shape of the acquisition curveGolding & & Lichten-stein, 1970Valins effect (effect of bogus heart rate feedback on preferences)Three groups of participants: NaÔve, suspicious (by being told in a conversation with a confederate that that they would be tricked), and completely informed about the deception by a confederateNo ìsignificantî differences in the Valin effect as a function of prior knowledge. However, participants who admitted awareness of experimental manipulation in a postexperimental questionnaire did not show the Valins effect, while those whose who either were not aware or did not admit their awareness showed a substantial effect (r = -.48)Gallo, Smith, & Mumford, 1973Conformity behaviorThree groups of participants: Complete, partial, or no information about the purpose of the experiment (the information did not reveal that deception was used) No significant effect (eta = .13)Turner & Simons, 1974Aggression (weapons effect)Three groups of participants: No information, informed that some deception might be involved (by a confederate tip-off), or informed that ìthe weapons were probably part of the procedure to influence their behaviorî (p. 342)ìIncreased levels of Ö subject sophistication led to decreased numbers of shocks administered by subjects to their frustratorsî (p. 341; eta = .43) Spinner, Adair, & Barnes, 1977 Incidental learningAt the end of the first part of an experiment, designed to arouse suspicion, participants were told: ìSometimes experiments require that a subject be deceived initiallyî (p. 546). Based on an ìawareness questionnaireî administered at the end of the second part of the experiment, participants were classified

into three groups as a function of their suspicion and anticipation of other tasksThose who were suspicious and intended to prepare for some other task scored higher than those who did not prepare and/or were not suspicious (eta = .46)Allen, 1983 Cooperativeness in a Prisonerís Dilemma gameTwo groups of participants: ìneutral forewarningî (i.e., ìin a few experiments it is necessary for experimenters to deceive subjects concerning some elements of the experimentî) vs. no forewarningNo significant effect Finney, 1987 Conformity behaviorThree groups of participants either were instructed that they ìmay be deceived,î ìwill be deceived,î or they did not receive any consent informationThe number of conformity judgments in the ìwill be deceived groupî (4.1) was significantly higher than in the ìno consentî group (1.9), but the results in the latter group did not differ from those in the ìmay be deceived groupî (2.3)Wiener & Erker, 1986Attribution of responsibility and evaluation of culpabilityTwo groups: Standard informed consent, and prebriefing group (i.e., participants were alerted to the possibility that they might be intentionally misinformed)No significant effects for sentencing judgment, verdicts, and attribution judgments; significant differences in the attribution processa Effect sizes calculated when sufficient information was available (search words: ìdeceptionî and ìprebriefing,î ìforewarning,î or ìinformed consentî).

Table 5. The effects of experimental history on participantsí performance.

AuthorsResearch TopicManipulationBehavioral Effects (Effect Sizeb) Brock & Becker, 1966Compliance behaviorStudents participated in two consecutive experiments, the debriefed experiment and the test experiment. Participants were assigned to three groups: No debriefing, partial debriefing, and complete debriefing. For half of the participants, the test experiment included an element from the debriefing experience; for the other half the common element was omitted.Complete debriefing reduced compliance behavior in the test experiment (10%) but only when the debriefing situation and the test experiment was explicitly similar; no reduction in the no and partial debriefing conditions (50% and 50%)Fillenbaum, 1966Incidental learningExperiment 1. Performance on an incidental-learning task after an earlier task that did or did not involve deception. Experiment 2. Same procedure as in Experiment 1 with minor changes.Experiment 1. Altough participants who experienced deception did somewhat better on the incidental-learning task, the difference was ìnot very large and far from significantî (p. 534, r = .1). Difference was larger if one compared participants who reported themselves to be suspicious to those who did not. Experiment 2. Participants with deception experience did better on the incidental-learning task (r = .27). As in Experiment 1, difference was larger if one compared participants who reported themselves to be suspicious to those who did not.Cook, Bean, Calder, Frey, Krovetz, & Reisman, 1970Attitude-change experimentsExperiment 1. Experimentally naÔve participants took part in one of five attitude-change experiments. Experiment 2. Participants were assigned into one of three groups in Experiment 1, which was or was not linked to Experiment 2 (by a common cue). The three groups were no deception, experience of deception, knowledge

of deception.Experiment 1. Attitude data did not significantly differ as a function of experimental history. Experimental history, however, affected global attitudes: Participants with deception experiences believed the experimenter less, considered experiments to be less scientific and less valuable, and reported caring less about understanding and following instructions Experiment 2. Attitude was affected by the deception variable and the presence of the cues (eta = .34). Without a cue, experience of deception biased the data (compared to knowledge of deception). With a cue, learning about deception but not experiencing it biased the dataa Fillenbaum & Frey, 1970Incidential learningStudents were given the critical incidental-learning task immediately after a prior and revealed deception on another task. Students were categorized as ìtrustfulî or ìsuspiciousî participants. Suspicious participants scored higher on the incidental-learning task than trustful participants (eta = .31)Silverman, Shulman, & Wiesenthal, 1970Various dependent variablesExperiment 1 involved either deception and debriefing or a memory study without deception. In Experiment 2, all participants were given tests measuring compliance of demands , persuasibility, sentence completion, and a personality test.ìSignificant differences between deception and nondeception conditions were observed with all four of the tests usedî (p. 209). Eta equaled .25, .26, and .29 for the compliance of demands, persuasibility, andsentence completion test, respectively. Overall, ìthe deception experience sensitized subjects to possible ulterior purposes of experiments, increasing evaluation apprehensionî (p. 209) Cook & Perrin, 1971Attitude change, incidental learning Experiment 1. Participants were assigned to one of three deception conditions: no-deception, experience of deception, and knowledge of deception. Experiment 2. Attitude-change and incidental-learning measures were obtained

(participants did or did not learn that also this experiment involved subsequent deception; we ignore this manipulation here). The attitude data (unlike in Cook et al, 1970) did not discriminate between conditions. The incidental-learning measure showed that prior experience but not prior learning of deception produced greater incidental learning (r = .3), and ìexperiencing deception produced the strongest evidence of absolute biasî (p. 215). A measure of general suspiciousness (ìhow truthful are psychology experimentersî) but not of particular suspiciousness (concerning the relationship of both experiments) showed a main effect on incidental learning (eta = .29)Page & Scheidt, 1971Aggressive-ness (weapons effect)Experiment 3. Two groups of participants: NaÔve participants who took part in a psychological experiment for the first time and sophisticated participants who took part in a deception experiment within the last month.The weapon effect was obtained for the sophisticated but not for the naÔve participants (eta = .32). ìWhat appeared to be aggressive behavior to the original investigators seems to have been a sham or an artifactî (p. 315). Christensen, 1977Verbal conditioningExperiment 1. Four experimental groups, including one group in which an active attempt was made to manipulate their behavior. Then they were debriefed and went through the verbal conditioning procedure. Experiment 2. Three experimental groups, including one prior manipulation group and one nonmanipulation group Experiment 1. Conditioning did not occur for the group that experienced prior manipulation and deception, but it did occur for the group that was only told that experiments may involve active manipulation of their behavior Experiment 2. Unlike in the control and nonmanipulation groups, ìsubjects given a manipulative experimental experience do not exhibit verbal conditioningî (p. 397) Gruder, Stumpfhauser, & Wyer, 1977Performance on an intelligence testParticipants

received randomly determined feedback about their performance on an intelligence test. Half of them were debriefed about this deception while the other half were not. Then they worked on a parallel form of the test a week later.Participants who had been debriefed improved more in the parallel form than those who had not been debriefed (eta = .3)a Bias being defined as the difference to the no deception group.

b Effect sizes calculated when sufficient information was available.