

The Costs of Deception: Evidence from Psychology

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Abstract

Recently, it has been argued that the evidence in social science research suggests that deceiving participants in an experiment does not lead to a significant loss of experimental control. Based on this assessment, experimental economists were counseled to lift their de facto prohibition against deception to capture its potential benefits. To the extent that this recommendation is derived from empirical studies, we argue that it draws on a selective sample of the available evidence. Building on a systematic review of relevant research in psychology, we present two major results: First, the evidence suggests that the experience of having been deceived generates suspicion that in turn is likely to affect the judgment and decision making of a non-negligible number of participants. Second, we find little evidence for the reputational spillover effects that have been hypothesized by a number of authors in psychology and economics (e.g., Kelman, H.C., 1967. *Psychological Bulletin*. 67, 1–11; Davis, D.D. and Holt, C.A., 1993. *Experimental Economics*. Princeton University Press, Princeton). Based on a discussion of the methodological costs and benefits of deception, we conclude that experimental economists' prohibition of deception is a sensible convention that economists should not abandon.

Keywords: experimental economics, deception, reputational spillover effects, experimental control

JEL Classification: C72, C91.

Introduction

Among experimental economists, deception is generally taboo; exceptions can probably be counted on two hands.¹ Davis and Holt (1993, pp. 23–24) gave the following typical rationale for why researchers should 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.

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In response, experimental economists who advocate the use of deception argue that there is little evidence in the literature that deception leads to a loss of experimental control. Hence, they argue that economists' long-standing de facto proscription of deception is unduly restrictive and prevents them from capturing the potentially significant benefits of using it (e.g., and prominently, Bonetti, 1998a, 1998b). Response to this argument has been swift but non-empirical (e.g., Hey, 1998; McDaniel and Starmer, 1998). Whether indeed the use of deception incurs methodological costs, however, is an issue that can be evaluated empirically.

Although the de facto prohibition of deception in experimental economics makes such an evaluation impossible, we can draw on data from psychology where in some areas deception is frequently used (Hertwig and Ortmann, 2002b). Although the research agendas, practices, and participant pool of psychologists and economists diverge significantly (Hertwig and Ortmann, 2001, 2002a), experimenters in both disciplines share areas of common interest such as decision making in an individual and social context, thus inviting a closer look "across the border".

If the use of deception exacts a price, should it be paid? In psychology, answers to this question have been given from a consequentialist or a deontological point of view. Those favoring the latter view argue that the costs of any violation of participants' human rights are prohibitive (e.g., Baumrind, 1964, 1985). This view effectively outlaws the use of deception whatever its potential benefits. In contrast, those favoring the consequentialist view have argued that the costs of violating participants' human rights need to be weighted against the costs of not being able to test theories or to explore experimentally important social policy issues. This view motivates the kind of cost-benefit calculus that the *American Psychological Association* (1992; henceforth, *APA*) has adopted in theory (albeit not in practice²) and that we pursue here by focusing on the methodological costs of deception. This does not mean that we take sides in the debate between consequentialists and deontologists, or that we consider the deontological arguments to be irrelevant. Rather, our view is that agreement on the merits of deception is more likely to be reached over those methodological costs of deception that can be subjected to empirical investigations.

The paper is organized as follows: In Section 1 we provide a definition of deception and sketch the extent of its use in areas such as social psychology. In Section 2 we discuss briefly the structure of the social situation "experiment." Building on this analysis, we review in Section 3 the consequences of the use of deception. In Section 4 we discuss further costs and benefits of deception, and conclude with a policy recommendation.

1. How frequent is the use of deception in (social) psychology?

Before we answer this question, a clarification of what constitutes deception is in order. Let us start with what is not deception. It seems undisputed among both psychologists and economists that it is not feasible, nor desirable, to acquaint participants in advance with all aspects of the research being conducted (e.g., its hypotheses and the full range of experimental conditions). Even Baumrind, whose persistent critique of deceptive practices (e.g., Baumrind, 1964, 1985) made her the doyenne of the "prohibitionists" (Bonetti, 1998b, p. 379) in psychology, suggested that "absence of full disclosure does not constitute intentional deception" (Baumrind, 1985, p. 165).

Psychologists and economists also agree that intentional misrepresentation of the purpose of an experiment, or any aspect of it, constitutes deception. For instance, psychologists Adair et al. (1985) stated: “Deception was defined as the provision of information that actively misled subjects regarding some aspect of the study” (p. 62). Similarly, Nicks et al. (1997) defined deception as “an explicit misstatement of fact” (p. 70). Along these lines, Hey (1998, p. 397) stated succinctly, “There is a world of difference between not telling subjects things and telling them the wrong things. *The latter is deception, the former is not.*”

Or is it? What if not telling participants things leads them to invoke certain default assumptions? One default assumption participants may reasonably have is that the initial interaction with the experimenter (upon entering the laboratory) is not yet part of the experiment. Another plausible default assumption participants may have is that the participants in the experiment are the other participants in the room (see Gross and Fleming, 1982; Sieber et al., 1995, p. 72). Clearly, we are getting into tricky territory quickly. Absence of full disclosure and “economy with the truth” can violate default assumptions and therefore mislead participants. We do not know what effects the violations of default assumptions have. However, we propose that these violations, whether resultant from a sin of omission or a sin of commission, can generate suspicion and induce second-guessing of the research design. Hence violations of default assumptions should be avoided.³

How frequent and widespread is the use of deception in psychology? Though deception is used across a wide range of areas of study such as personality, marketing, and consumer research, it appears to have been used most frequently in social psychology. Take the highest-ranked journal in social psychology, the *Journal of Personality and Social Psychology* (*JPSP*), and its predecessor, the *Journal of Abnormal and Social Psychology* as an illustration.⁴

Between 1921 and 1948, a period for which Nicks et al. (1997) analyzed the percentage of deception studies on a yearly basis, an average of 5% of the articles in *JASP* employed deception each year.⁵ According to Nicks et al., the percentage rose steadily from 9% in 1948 to 51% in 1968, peaked in the 1970s (with 69% in 1975 according to Gross and Fleming, 1982), remained high in the early 1980s (with 50% in 1983; Adair et al., 1985), and declined to 31% 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.

Although some of these fluctuations may reflect different definitions of what constitutes deception (e.g., compare the more inclusive criteria employed by Sieber et al., 1995) with the criteria used by Nicks et al., 1997), a conservative estimate would be that every third study published in *JPSP* in the 1990s employed deception. In other social psychological journals, for instance *Journal of Experimental Social Psychology*, the proportion is even higher (Adair et al., 1985; Nicks et al., 1997). The frequent use of deception in social psychology in recent decades contrasts markedly with its decidedly more selective use in the 1950s and earlier (Adair et al., 1985). Despite the APA ethics guidelines and the fact that institutional review boards must review deception studies, deception has not become the strategy of last resort that it is meant to be.⁶ As we have argued elsewhere (Ortmann and Hertwig, 1997, 1998), the reasons for this may have to do with the fact that psychologists—for all practical purposes being allowed to weigh their own private benefits

of using deception against the public costs—are bound to fall prey to an implicit moral hazard problem.

2. The structure of the social situation “experiment”

To structure our presentation of the empirical evidence, it is useful first to clarify the underlying strategic situation in which both experimenters and participants find themselves. We suggest that the social situation “experiment” has the complicated structure of a multilevel game (e.g., Hausken, 1995a, 1995b). As such it is a complex game that is played inter-group (between experimenters and participants) and intra-group (between participants or between experimenters), and that is complicated through a power hierarchy.⁷ We focus below on the inter-group aspects of this game.

In the interaction between experimenters and participants, the choices available to experimenters are those of an agent squaring off with a principal in a one-sided social dilemma or reputational game (Ortmann and Colander, 1997; Kreps, 1990). Both agent and principal can either contribute their respective assets (honesty for the agent, trust for the principal) or withhold them. Specifically, the agent (experimenter) can choose to deceive participants (the principals) or to be truthful about the setting and purpose of the experiment. The principal (participant), in turn, can choose to trust the agent’s claims (e.g., instructions) or to doubt them. The game-theoretic prediction for this game played once is, depending on the parameterization, clear-cut: The agent will defect (at least with some probability). The principal, anticipating the defection, will doubt the agent’s claims about the experiment’s alleged purpose and nature of the experiment (at least with some probability). The upshot is that even without previous deception experience the incentive structure of the strategic situation is such that participants who enter an experiment are likely to be suspicious. Recent literature on trust, however, suggests that, this prediction may be too pessimistic and that people tend to trust in many one-off situations in which according to canonical game theory they should not (Berg et al., 1995; Ortmann et al., 2000).

The theoretical prediction for indefinitely repeated games is, of course, even more complicated (folk theorem!). Experimental results on public good provision and two-sided and one-sided prisoner’s dilemma games (e.g., Ledyard, 1995; Ortmann and Colander, 1997), however, give us some guidance about what to expect. Specifically, the experimental results suggest that a substantial number of participants react adversely to non-cooperative behavior. For example, Ortmann and Tichy (1999) found that in repeated prisoner’s dilemma games with turnpike matching protocol, 50% of the participants who get “burned” switch to non-cooperative behavior. Since, in every round, participants are matched with a new participant whose prior decision history they do not know, this defection rate is very likely a lower bound. Recent evidence from a once-repeated trust game confirms the conjecture. Dickhaut et al. (1995) observed that participants often give others the benefit of the doubt for the first round. If, however, their trust gets violated, they typically make sure it does not happen a second time. As the title of their manuscript states succinctly, “fool me once, shame on you, fool me twice, shame on me.”

This game-theoretically predicted default suspicion may be amplified—either through direct or indirect experience—because of a mechanism that Davis and Holt (1993, p. 24)

suggested:

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.

Talking to people who directly experienced deception is not the only way to “indirectly” experience it. Such experience can also occur through news reports about spectacular deception studies (e.g., the *International Herald Tribune*, September 18, 1996, on a study of the “The Bellicose Southern Male”⁸), scientific articles documenting deception and participants’ reactions to it (e.g., Oliansky, 1991), and—possibly most important—undergraduate teaching of classic deception studies (e.g., Milgram’s obedience-to-authority studies). Such indirect effects are important because they transform seemingly one-shot principal-agent games into indefinitely repeated games that may have different equilibria.

In light of their game-theoretically predicted participants’ default suspicion, participants’ expectations that they will not be deceived (i.e., honesty on the part of the experimenter) is an important common good. If the experimenter chooses to deceive participants, and is found out or suspected of employing deceptive practices, then this common good might get depleted. The extent of the depletion depends on participants’ responses to direct or indirect experiences with deception: Do they choose to respond in a non-cooperative (e.g., hostile) manner and do their responses generalize beyond the individual experimenter, or her lab? If it generalizes, uncooperative behavior will be directed against the community of experimenters, and thus deception creates externalities.

Our account of the strategic interaction between participants and experimenters suggests that any discussion of deception needs to distinguish the effects of direct experiences (i.e., firsthand experiences with deception) and indirect experiences (i.e., learning about the experiences with deception of others) on both individual experimenters and on the profession. In the following section, we discuss evidence from psychology that allows us to assess the severity of these effects. Throughout we shall distinguish whether deception affects behavior in experiments as well as participants’ feelings, attitudes, beliefs, and expectations. Of course, these categories are not mutually exclusive; indeed, some people argue that affective and cognitive responses are behavioral responses, or at least have the potential to immediately translate into such.

3. Consequences of the use of deception: Empirical evidence

3.1. What are the effects of direct experiences with deception?

3.1.1. Affective and cognitive responses. Several authors have concluded that during or after the conclusion of deception experiments, participants display negative emotions. Fisher and Fyrberg (1994), among others, reported that the majority of their students believed that participants in various published deception studies must have felt more embarrassed, sad, or uncomfortable than their non-deceived counterparts. Studying conformity behavior, Finney (1987) found that deceived participants believed less in the value of the research, reported

less confidence in their judgment, and more tension during the experiment.⁹ Allen (1983), in prisoner's dilemma games, found that participants who had been deceived during the session "rated the experiment as worthless, were annoyed with the experiment, and would not recommend the experiment to a friend" (p. 899; see also Straits et al., 1972). Oliansky (1991) observed that both participants and research assistants acting as confederates exhibited severe negative emotions.¹⁰

Offering a contrast to these reports of negative affective and cognitive responses, Christensen (1988) summed up his review of research on the impact of deception on participants as follows: "This 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 non-deception experiments" (p. 668).¹¹ Interestingly, Finney (1987) also observed that those who were deceived in an experiment reported less boredom with it than those who were not deceived (despite the fact that the former experienced other negative emotions). Noting that the records of participants' reactions to deception often draw on surveys or people who imagine taking part in experiments and then report their imagined reactions, Aitkenhead and Dordoy (1985) designed an experiment that crossed active and passive (role-playing) participants and examined the effects of minor forms of deception.¹² They reported that, in contrast to the imagined reactions of role-playing participants, "[active] subjects have, on the whole, positive reactions to the research, and are generally tolerant towards being deceived, being caused physical discomfort (even pain), and being treated inconsiderately by the experimenter" (p. 303).

The seemingly conflicting evidence prompts the following five observations and comments: First, the fact that deceived participants experienced less boredom in psychological experiments that suffer from a "mundane, repetitive nature" (Sharpe et al., 1992, p. 589; see also Coulter, 1986) cannot justify the use of deception. Rather, such a state of affairs should spur experimenters to make experiments more interesting and provide participants with incentives to perform well in them (e.g., Argyris, 1968; Hertwig and Ortmann, 2001). Second, deception (or the suspicion thereof) may in fact make a tedious and boring study less so by triggering curiosity. However, such responses—regardless of whether they are affectively negative or positive—undermine economists' saliency tenet (Smith, 1982), especially if the pay-off function is flat. In other words, even if one accepted Christensen's (1988) conclusion that participants enjoy deception experiments and believe to benefit from them, one would still face the danger that such affective and cognitive responses are likely to inject noise into the experimental data.

Third, the findings regarding participants' apparently positive feelings and attitudes are not as clear-cut as they were sometimes presented. For an illustration, consider the Smith and Richardson (1983) study, prominently featured by Christensen (1988, p. 668). They concluded that "those participants who had been deceived evaluated their experience more positively than those who had not participated in deception experiments" (p. 1075). As Rubin (1985) pointed out, however, the same authors reported that 20% of the participants in their survey (undergraduate students in introductory psychology classes) reported they had been in experiments that "caused them to feel very nervous, humiliated, excessive physical discomfort, very wrongly deceived and/or very angry" (p. 1078). These observations are

relevant because there was a correlation (albeit not perfect) between those feelings and participation in a deception experiment.

Fourth, deception as used in Aitkenhead and Dordoy (1985) is not deception as used in Asch (1956) or Finney (1987), which is not deception as used in Milgram (1963, 1964) or Oliansky (1991). In other words, whether deception lies within a participant's "comfort zone" (Gerdes, 1979), and is considered acceptable by a participant, is a function of, for instance, such as the nature and severity of deception, the methods of debriefing, and the recruitment mode. Fifth, and possibly most important for the present purpose, even if people had positive emotions, it would not preclude the possibility of them becoming suspicious, arguably an emotional-cognitive response. As Kelman (1967) put it, widespread deception is likely to lead to a situation in which participants "may not know the exact purpose of the particular experiment in which they are participating, but at least they know, typically, that it is *not* what the experimenter says it is" (p. 6). Before we turn to the behavioral consequences of such suspicion, we first examine how prevalent it is among participants in psychology experiments.

3.1.2. The prevalence of suspicion. Do participants assume that the purpose of an experiment is not what the experimenter says it is? To get a more systematic idea of the prevalence of suspicion among experimental participants in psychology, we conducted a literature search in PsycINFO/PsycLIT. This database covers the academic literature in psychology from 1887 to July 1999 (when our searches were conducted). Across the different searches that we will report below, we hunted for specific keywords in titles and abstracts. In addition, we included all psychology studies cited in Bonetti (1998b) who concluded from his sample that "deception does not appear to jeopardize future experiments or contaminate the subject pool" (p. 389). Finally, we looked up studies cited in the articles found and included them if they seemed relevant.

In the first search, we entered the keyword "deception" in combination with "suspicion" (and its variants such as "suspicious", "suspiciousness", "suspicious"). This search uncovered two systematic reviews of the social psychology literature (Stricker, 1967; Stang, 1976) that examined the proportions of participants who are suspicious. In a sample of 88 deception studies from four leading social psychology journals, Stricker (1967) found that only 16 studies attempted to quantify the degree of participants' suspicion of deception. In this subset, the median percentage of "suspicious" participants was 4%, with a range from 0% to 23%. Stricker et al. (1969, p. 345) later suggested that "it seems very likely that the overall rate of actual suspicion in these 16 studies was seriously underestimated" due to the inadequacy of the suspicion criteria used. Using different criteria, later studies found that indeed typically between one third and two thirds of the participants harbored suspicions.¹³

To quantify the prevalence of suspicion, however, one not only has to deal with the problem of operationalizing suspicion but also with participants not admitting to be suspicious. Relatedly, researchers found that participants were reluctant to admit that they had prior information about the experiment. Newberry (1973), for instance, reported two experiments in which participants received information about the experiment from a confederate and were later asked by the experimenters if they had prior information. The proportion of the participants who lied about having possessed prior information varied from approximately

.8 to .3 in various conditions. Taylor and Shepperd (1996) described an experiment in which they used deception to study the effectiveness of conventional debriefing procedures for detecting suspicion of deception among research participants. Regardless of the experimenter's explicit instructions to the participants that they not communicate while he left the room on a pretext, the authors found that participants did communicate with each other and found out that deception was involved in the experiment—a discovery they did not reveal during debriefing. Taylor and Shepperd (1996) concluded that “our observation suggests that participants may fail to supply, and may even withhold, information that is crucial to evaluating whether the procedures proved a valid test of the hypothesis” (p. 887).

Stang (1976) deserves a special mention here. After an exhaustive search of the conformity literature, he identified 21 studies that reported the percentage of participants who were classified as suspicious.¹⁴ Interestingly, Stang found a dramatic increase of suspicion, especially in the second half of the 1960s, that seems to track closely the dramatic increase of deception experiments during that period and the possibly associated increase in participant sophistication and decrease in participant trust.¹⁵ Specifically, Fig. 1 in Stang (1976, p. 354) suggests that until the mid-1960s 1 out of 10 participants reported being suspicious, with that number shooting up to between 40% and 60% for the period 1967 to 1973, the last year reported in his Fig. 1.¹⁶ Note that, for example, in *JPSP* and its predecessor, *JASP*, the percentage of deception studies tripled from 16% in 1961 to 47% in 1971, with similar upward swings also documented for other journals (see Adair et al., 1985).

Stang (1976, p. 360) claimed that participants in conformity experiments who report suspicions commonly get excluded from the data analysis.¹⁷ For instance, in Stang's own 1976 study, 20% of participants were excluded based on self-reported suspicions. The problem is, as we demonstrated above, it is quite possible that this number systematically underestimates the true degree of suspiciousness since participants often may not reveal their suspicions or knowledge truthfully (e.g., Newberry, 1973; Oliansky, 1991; Taylor and Shepperd, 1996; see also Argyris, 1968; Schultz, 1969). Does this non-cooperative behavior also manifest itself in participants' experimental performance?

3.1.3. Experimental performance. In general, there are two strategies to analyze the effects of suspicion on experimental performance. Experimenters can engender participants' suspicion from the outset and study their subsequent performance as a function of it. Alternatively, experimenters can record suspicion after participants concluded the experimental task. In what follows we explore both strategies.

To find studies that induced suspicion, we conducted another search following the procedure outlined earlier. Specifically, we used the search term “deception” in combination with “prebriefing” or “forewarning”. We found eight studies that included those key terms (Levy, 1967; Golding and Lichtenstein, 1970; Gallo et al., 1973; Turner and Simons, 1974; Spinner et al., 1977; Allen, 1983; Wiener and Erker, 1986; Finney, 1987). Methods used to induce suspicion ranged from relatively neutral forewarning about experimental procedures in general (e.g., Allen, 1983, p. 901: “in a few experiments it is necessary for experimenters to deceive subjects concerning some elements of the experiment”) to confederate tip-offs (e.g., Levy, 1967) and disclosure that deception would occur during the experiment (e.g., Finney, 1987).

To quantify the studies' findings, we calculated effect size measures where possible.¹⁸ The results, once again, were mixed. For some of the eight studies we found small or no effects (e.g., Gallo et al., 1973; Allen, 1983) and for some we found medium and large effects (e.g., Levy, 1967; Turner and Simons, 1974; Spinner et al., 1977).¹⁹ Despite this mixed picture, a trend is discernible: When participants received detailed tip-offs about the true purpose of the experiment (e.g., Levy, 1967; Turner and Simons, 1974), were explicitly told that they would be deceived (Finney, 1987), or explicitly acknowledged awareness of experimental manipulation (Golding and Lichtenstein, 1970), suspicion altered experimental performance across a wide range of dependent variables measured in social-psychological research. In contrast, when participants were merely informed that some kind of deception might happen (Allen, 1983; Finney, 1987; Wiener and Erker, 1986) or were told the purpose of the study without an indication of the possibility of deception (Gallo et al., 1973), then their performance did not differ from that of control participants who had not been given this information (but see Spinner et al., 1977).

There are two interpretations of this picture. One could conclude that specific and certain expectations about deception alter experimental performance, whereas general and uncertain anticipation of deception does not. Finney (1987), however, had another suggestion for why uncertain anticipation ("may be deceived") did not seem to differ from the control condition (with no information). In his view, "one might speculate that this [general and uncertain] information merely reaffirms subjects' prior belief that deception may occur in an experiment and, therefore, causes no change in their anticipation" (p. 45). If indeed this general uncertain information only reaffirms prior beliefs, it is not surprising that the experimental and the control groups (who share the same priors) do not differ.

An alternative, though imperfect way to assess participants' suspicion is to ask them after the conclusion of the experimental task (but before the debriefing) whether they had any suspicion (e.g., Asch, 1956). Our search turned up 14 studies that investigated behavior as a function of suspicion. All were concerned with conformity behavior. In 10 studies, suspicious participants conformed less than unsuspecting participants. For 9 of these 10 studies (those in which the necessary information was given) we could calculate effect size measures (*eta* or biserial correlation *r*; see footnote 18): the reduction in conformity due to suspicion was of medium to large effect size (in increasing order of effect, Adair, 1972; Stang, 1976; Endler and Hartley, 1973; Ettinger et al., 1971; Geller et al., 1973; Rubin and Moore, 1971; Stricker et al., 1967; Geller and Endler, 1973; Glinski et al., 1970). In 4 of the 14 studies, suspicion did not significantly change the amount of conformity behavior (Chipman, 1966; Willis and Willis, 1970; Endler et al., 1972; Wiesenthal et al., 1973). No study reported that suspicion produced greater conformity.

To conclude, the systematic samples that we investigated suggest that suspicion has the potential of altering participants' behavior in psychology and, for that matter, economics experiments. Take conformity experiments as an example: To the extent that conformity experiments explore people's compliance with social pressure, and to the extent that social norms have been identified as an important control variable in a variety of experimental settings (e.g., Hoffman et al., 1996), using deception clearly has the potential to inject systematic error variance in the experimental data.

3.2. *Do participants' direct experiences with deception studies spill over to future experiments?*

3.2.1. Affective and cognitive responses. To assess systematically the impact of the use of deception on participants' generalized expectations (e.g., regarding the anticipated truthfulness of information provided), we conducted yet another literature search. This time we searched for the keyword "deception" in combination with "expectation(s)" in all titles and abstracts and found—quite to our surprise—only five relevant studies. Among these studies, the size of the effect of direct experience with deception on people's expectations ranged from small (Smith and Richardson, 1983; Sharpe et al., 1992) to medium-large (Epley and Huff, 1998; Krupat and Garonzik, 1994). It is interesting to note that the most recent, and in our view most careful, studies—those by Epley and Huff and by Krupat and Garonzik—have larger effect sizes than the earlier investigations. In what follows, we briefly describe these two studies in more detail.

Epley and Huff (1998) studied experimentally how deception impacts the suspicion of participants. They gave participants a reading comprehension task plus fabricated performance feedback. Then, at the end of this first session, participants were given either a full debriefing that explained the deceptive nature of the experiment or partial debriefing that omitted information about the use of deception. Participants' reactions to the experiment were measured through a questionnaire immediately after the debriefing. Specifically, participants responded to the following statement: "As a participant in future research, I will not be suspicious of any information presented by the experimenter." Epley and Huff found that those participants who were told that they had been deceived anticipated being more suspicious of future experiments. However, judging from their answers to the statement "psychologists are trustworthy and honest", their suspicions did not appear to extend to the profession in general. The former result is important in light of the APA requirement that experimenters who employ deception must debrief participants afterward. Clearly, this well-meaning imperative has the potential to backfire.²⁰

In their Study 1, Krupat and Garonzik (1994) asked 255 psychology students from introductory-, intermediate-, and advanced-level courses to answer an 11-item questionnaire. The introduction to all items was identical: "If you were asked to participate in a psychology study, would you expect that . . ." The key item continued "you will be misled or deceived in some way during the course of the study." Controlling for psychology courses taken and experimental experience as participants, Krupat and Garonzik observed that a single encounter with deception suffices to make students expect that they will be misled or deceived in other studies. In contrast, psychology courses taken and experimental experience as participants had no significant impact.

The results by Krupat and Garonzik (1994) and Epley and Huff (1998) seem to contradict earlier results by Sharpe et al. (1992). These authors included one item in their questionnaire that referred to deception, "Experiments in psychology almost always involve deception." They observed that their three participant samples disagreed with this statement. This finding, however, may simply be due to the fact that two of their participant samples had never participated in a psychological study prior to the survey, and only 29% of the participants in the third had encountered at least one deception experiment. In fact, only 12% of the

experiments in which those 29% of the participants took part involved deception, and “relatively mild forms of deception” (p. 588) to boot. In addition, psychology experiments by no means “almost always” involve deception. Thus, participants may have had the veridical expectation that deception is used but not necessarily “almost always”.

To conclude, the results suggest that direct experience with deception appears to increase participants’ expectations of being deceived in future experiments. This is consistent with the work of Cook et al. (1970, p. 189), who found that participants with a history of deception studies were more suspicious of the truthfulness of experimenters. At the same time, direct experience with deception does not seem to affect participants’ beliefs about psychologists’ trustworthiness in general.

3.2.2. *Experimental performance.* To study systematically whether or not direct experience with deception experiments translates into behavioral responses in future experiments, we searched the PsycINFO/PsycLIT database using the term “deception” in combination with “experimental history”. We found a total of nine studies: Brock and Becker (1966), Fillenbaum (1966), Cook et al. (1970), Fillenbaum and Frey (1970), Silverman et al. (1970), Cook and Perrin (1971), Page and Scheidt (1971), Christensen (1977), and Gruder et al. (1977).

In brief, the results obtained in those studies suggest that direct experience with deception or manipulation affects performance in future experiments, whereas mere disclosure of the possibility of deception in psychological experiments does not (Cook and Perrin, 1971; Christensen, 1977, Experiments 1 and 2). Second, Silverman et al. (1970) observed that experience with deception appears to make people more apprehensive of evaluation (i.e., participants are concerned about being observed and judged, and thus will develop hypotheses about how to win positive evaluations and avoid negative ones). 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. For instance, in Cook and Perrin’s (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).²¹ Finally, the extent to which previous deception experience transfers to other experiments may depend on the similarity between past and present experimental situations (Brock and Becker, 1966; Cook et al., 1970).

To illustrate how past experience with laboratory deception can distort behavior in future experiments so extremely that it elicits a phenomenon that “cannot be generalized to nonlaboratory situations” (p. 304), consider Page and Scheidt’s (1971) studies of the “weapons effect”.²² Page and Scheidt were able to replicate the weapons effect in only one of their three experiments, and only in a group of participants who had taken part in a deception experiment within the previous month. In contrast, participants unfamiliar with psychological experimentation never exhibited the effect. Turner and Simons (1974; see also Simons and Turner, 1976) challenged Page and Scheidt’s results, and based on them Turner et al. (1977) came to the opposite conclusion: “Perhaps the failures to replicate the weapons effect occurred because the researchers used subjects who were not naive about deception or who were very apprehensive about the impression they might create”

(p. 369). Interestingly, although Page and Scheidt (1971) and Turner et al. (1977) disagreed on how experience with deception alters experimental performance, they agreed that it does have this potential. Turner and Simons (1974) concluded: "Apparently, unless subjects are naive, the effects of important independent variables may be obscured" (p. 347).

To conclude, past experience with deception can, but does not always, translate into non-cooperative behavior in future experiments. To what extent it does translate seems to depend on variables such as the similarity between previous and future experiments, and on the degree to which evaluation-apprehension interferes with people's behavior in experiments.

3.3. *Do participants' indirect experiences with deception spill over to future experiments?*

We did not find a single study that investigated the impact of participants' indirect experiences on individual labs. Indeed, theoretically such effects are not likely to occur: It requires an individual experimenter acquiring a reputation for his deception experiments, a participant having heard about this experimenter's practices through a textbook, campus scuttlebutt, or the like, and the coincidence that the participant will take part in such an experiment. To the extent that many experimenters in psychology seem to restrict themselves to first-semester students, as we will see presently, this scenario is not likely to happen. Therefore, we focus here on how participants' indirect knowledge of deception studies might spill over to other experiments.

3.3.1. *Affective and cognitive responses.* The evidence in this category is somewhat limited. The few available studies, however, indicate that indirect experience may matter. According to Rubin and Moore's 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 indirect experience acquired during undergraduate psychology training matters. In fact, Higbee (1978) observed that students rated psychologists as being less truthful at the end of the semester than at the beginning ($\eta^2 = .51$), and students with at least five psychology courses rated psychologists as being less truthful than students who had no previous psychology course ($\eta^2 = .43$). Based on these findings, Higbee (1978) concluded that "if psychologists expect the subjects to believe them, perhaps they should get the subjects at the beginning of the semester" (p. 133)—a refinement of the advice that Silverman et al. (1970) gave.

3.3.2. *Experimental performance.* With the exception of one dramatic report, we did not find systematic attempts to investigate how indirect experience affects behavior. MacCoun and Kerr (1987) presented the case of a participant experiencing a genuine epileptic seizure and other participants' reactions to it. Three of the five other participants (all of them introductory psychology students) reported that they questioned the authenticity of the seizure and that they believed it to be a charade perpetrated by the experimenter and the victim. MacCoun and Kerr reported that "there were indications that prior knowledge of

psychological research—derived primarily from course work—was related to suspicion. The most suspicious subject (...) had begun to study psychology in high school. Another subject recalled reading about the Milgram (1963) obedience studies in a psychology textbook” (p. 199). It is interesting to note that had MacCoun and Kerr’s study been concerned with altruistic behavior, then withholding help due to suspicious would have falsely confirmed the “bystander effect” (Darley and Latané, 1968).

4. Discussion

The purpose of this paper was to assess *empirically* the methodological costs of deception. Are the proponents of deception in experiments right when they argue that “there is little significant effect of deception on subject behavior or on later groups of experimental subjects” (Bonetti, 1998a, p. 413; see also Kimmel, 1998)? In contrast to this view, from a systematic review of the available evidence we conclude that direct experience with deception has the potential to elicit affective and cognitive reactions that interfere with experimental control. Specifically, direct experience with deception can generate suspicion, which, in turn, can alter behavioral responses. The rich research on the effect of suspicion on conformity behavior illustrates this link. In addition, direct experience with deception can spill over into future experiments by eliciting the expectation of being deceived again. Based on these findings, it seems fair to conclude that *direct* experience with deception has the potential to alter the experimental performance of those participants who harbor suspicions, and in addition, to generate suspicion and second-guessing in future experiments.

We agree with Bonetti, however, that there is no clear and convincing evidence that *indirect* experience with deception matters, as authors in psychology (Kelman, 1967) and in economics (Davis and Holt, 1993) have hypothesized. We believe that there are two reasons for the lack of evidence. First, indirect experiences (e.g., via campus scuttlebutt or media exposure) are simply difficult to measure and quantify, as one needs to identify and compare participants with and without indirect experience. This difficulty may explain why few attempts have been made to study the effect of indirect experience. If so, then the lack of such studies merely suggests that the issue is difficult to investigate, but not that indirect experience of deception comes without methodological costs.

A second reason for the lack of evidence regarding the consequences of indirect experiences with deception may be that they may not exist in psychology. This does not mean, however, that indirect experiences do not exact a price. Rather, psychologists have apparently tried hard to avoid paying the price by relying on students from introductory courses who may still be naive. In a relatively recent review, Sieber and Saks (1989; see also Vitelli, 1988) documented the extent to which psychologists depend on such students. Specifically, they reported responses of U.S. psychology departments with participant pools. In their sample of 242 departments, they found that 93.4% recruited from introductory courses.²³ For comparison, consider Schultz’s (1969, Table 1) summary of the composition of the human subjects pool (in studies published in three APA journals) published 20 years earlier. He reported that on average less than 40% of human participants were from introductory psychology courses. Albeit speculative, it is plausible that psychology’s peculiar institutional arrangements (to recruit participants mostly from introductory courses) is the result

of an evolutionary process—a process driven by attempts to minimize the contaminating effects of deception on the participant pool (and by the desire to guarantee “free” access to participants). If true, psychologists may have taken the advice of Silverman et al. (1970) to heart “that the practice of using the same subjects repeatedly be curtailed, and whenever administratively possible, subjects who have been deceived and debriefed be excluded from further participation” (p. 211).

To conclude, we agree with previous observations that there is little evidence that indirect experience with deception has negative effects. However, we suggest that those effects might very well become manifest if recruitment practices in psychology did not curtail them, or alternatively, if a discipline such as experimental economics, which does not recruit students primarily from introductory classes as experimental participants, began using deception.

4.1. To deceive or not to deceive?

We believe that anti-prohibitionists such as Bonetti (1998a) are to be applauded for reflecting on the costs and benefits of deeply entrenched methodological practices. Like Bonetti, we believe that to put our practices in perspective, it is useful to look at methodological practices across disciplines (Hertwig and Ortmann, 2001, 2002a). Based on our own extensive and systematic review of studies on the consequences of deception conducted in psychology, however, we disagree with Bonetti’s (1998a) conclusion, based on his “brief review of the available evidence” (p. 384), that “the experimental evidence directly undercuts the basis of attempts to proscribe deception” (p. 389). We believe that his conclusion is the result of a less extensive and systematic review of the evidence.

According to Bonetti (1998a) the most important benefit of deception is “the way in which the attention of the subjects can be effectively distracted, thus ensuring that the behavior which is measured is more natural and spontaneous and less affected and contrived” (p. 386). In other words, anti-prohibitionists argue that certain aspects of human behavior can only be studied if people are caught off guard. Bonetti’s argument echoes that of advocates of deception in psychological experimentation who argue that, for instance, in investigations of socially undesirable aspects of behavior the experimenter needs to camouflage the purpose of the experiment to achieve experimental control. If not, the “psychologist runs the risk of distorting the reactions of his or her subjects and ultimately limiting the applicability of the research findings” (Kimmel, 1996, p. 68). This argument, as persuasive as it may look, ignores the empirical evidence that we have accumulated above and that, to our mind clearly, indicates that the direct experience of deception has the potential to change experimental performance. In other words, the very use of deception can impair, and even destroy, the experimental control it is meant to achieve. Moreover, its use exacts another cost, which needs to be taken into account when trying to strike a balance between the costs and benefits of deception, namely the slowing down of methodological innovation. This argument was well expressed three decades ago by Baumrind (1971, p. 893):

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 . . . be capable of inventing new experimental methods that were well controlled as well as humane.

Indeed, given that the alleged last-resort strategy of deception is widely accepted in psychology, why bother developing an experimental methodology that would allow psychologists to do without? Hence, we argue that if economists gave up their de facto prohibition against deception, they would discourage the development of alternatives to deception. In this context, it is noteworthy that Bardsley (2000) recently provided a non-deception replication of Weimann (1994), which Bonetti (1998b) paraded as a prime example of an economic investigation that “necessarily required deception” (p. 387). Bardsley’s innovative design demonstrates that this statement is untenable.²⁴ In fact, we conjecture that there is no theory in economics that could not be tested without deception.

4.2. Conclusion

Using evidence from psychology we have attempted to assess empirically and systematically the methodological costs of deception. The review of the available evidence suggests that the direct experience of deception and the suspicion of deception carry with them the potential of provoking significant affective, behavioral, and cognitive responses. To the extent that these responses are bound to introduce random as well as systematic error variance in the data, they impair, and possibly destroy, experimental control. In light of this danger and additional costs such as selection biases in the participant pool, and the slowing down of methodological innovation (if deception were to be accepted among economists), we conclude that the prohibition of deception is a sensible convention that economists should not abandon.

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Notes

1. Bonetti (1998b) and Hey (1998) enumerated the half-dozen studies in economics that come to mind quickly. In a recent survey, we found that a representative sample of experimental economists estimated that, on average, they use deception in 0.17 of 10 experiments (for details see Hertwig and Ortmann, 2001).
2. The APA guidelines admonish researchers to employ deception as a strategy of last resort only. However, the high rate of deception experiments in areas such as social psychology suggests that deception is not treated as such. In many areas of psychology, deception is indeed considered an inconsequential transgression that “unrepentant deceivers” (as one psychologist called himself in a communication to one of us) rationalize with

the elegance of a research design and that others (to cite an example that one of us encountered recently as a referee) are allowed to rationalize with, for instance, pragmatic consideration such as poorly programmed experiments. In Hertwig and Ortmann (2002b)—written for a psychology audience and a companion paper of sorts—we propose an incentive-compatible mechanism designed to reduce the frequency of deception experiments in psychology.

3. Admittedly, this statement is difficult to operationalize. In the words of one referee, “How will we know what range of default assumptions might be entertained by a given subject pool.” One sensible approach to assessing default assumptions would be the use of appropriately constructed pilot sessions.
4. We found 14 studies that analyzed the frequency of deception across a wide range of journals and areas of study (e.g., Toy et al., 1989). We use *JASP* and *JPSP* for illustration because the data for these journals are the most comprehensive.
5. We call a study (an experiment) that involved deceptive methods a “deception study” (“deception experiment”). This does not necessarily mean that the effects of deception were studied, as the shorthand expression might suggest.
6. Although deception is still widely used, ethical guidelines for research have become stricter (for a short history of the “ten commandments of the APA” see Rosnow and Rosenthal, 1997, Chapter 6). As a consequence, the profession has succeeded in reducing the severity of deceptive methods used. Rosnow and Rosenthal (1997), for instance, concluded that “many of the seminal studies that were conducted then would be impossible today (e.g., Milgram’s obedience studies)” (p. 114). We agree with this conclusion, notwithstanding evidence that emotional distress caused by less severe deception practices can be substantial (e.g., Oliansky, 1991, or Asch, 1956).
7. The relationship between participants and experimenters “has some of the characteristics of a superior-subordinate one Perhaps the only other such one-sided relationships are those of parent and child, physician and patient, or drill sergeant and trainee” (Schultz, 1969, p. 221). Similarly, Argyris (1968) argued that “[subjects] are now beginning to behave like lower level employees in companies” (p. 187). Such a power hierarchy may invite psychological reactions per se that may have little to do with the experimental scenario or the reputation of the lab among the student population. It is likely that these problems are more prominent if participation is not voluntary. According to an analysis of recruiting practices in psychology, “[o]nly 11 percent of departments have a subject pool that is voluntary in the strictest sense, that is, there are no penalties for nonparticipation, no grades for participation, and no alternatives to participation. . . . most (departments) are not entirely in conformance with the APA ethical guidelines” (Sieber and Saks, 1989, p. 1057).
8. In an attempt to compare stress- and aggression-related hormone surges for southern and northern white males, the authors of the study instructed confederates to bump into participants and call them insulting names. A couple of years after the study came out, one of us, while taking in the spectacular vista of the Monhegan Island coast (Maine, USA), overheard one hiker tell another about that very experiment.
9. Following Asch (1956), conformity experiments typically place a participant in a judgment or decision situation (e.g., the line-length comparison task) together with other “participants.” These other participants, however, are confederates of the experimenter and are instructed to make incorrect claims. The purpose of such an experimental scenario is to study compliance with social pressure and adherence to social norms.
10. Oliansky related his experiences as a confederate/research assistant whose job it was to trick participants into believing that they could affect another person’s emotional well-being. In actuality, the experiment was designed to test how participants’ feelings were affected by their perception of their ability to help another person in emotional distress. Oliansky discussed his own doubts and guilt as a confederate. He also discussed the angry responses of the significant number of participants who felt duped and the responses of others who questioned the alleged purpose of the experiment from the beginning.
11. Curiously, Christensen’s review did not contain his own earlier article in which he concluded that “subjects who perceive that their behavior is being manipulated will tend to resist this influence and exhibit behavior reflective of the so-called negative subject” (Christensen, 1977, p. 399).
12. Aitkenhead and Dordoy (1985) had participants solve five-letter anagrams. Deceived participants were told that the aim of the experiment was to discover how quickly anagrams could be solved when participants were either relatively stressed or relatively unstressed. The high-stress condition required participants to solve three

- difficult anagrams while holding an ice cube; the low-stress condition required participants to solve three easy anagrams (no ice cube). Finney's (1987) experiment was a conformity experiment that exposed participants to incorrect line-length judgments of a group of confederates. The Aitkenhead and Dordoy participants were not paid; Finney's participants were offered extra credit points.
13. These numbers resulted from post-experiment interviews. Relevant studies include: Glinski et al. (1970), Ettinger et al. (1971), Endler and Hartley (1973), Geller and Endler (1973), Geller et al. (1973), Wiesensthal et al. (1973), Willis and Willis (1970), Rubin and Moore (1971), Adair (1972), and Stang (1976).
 14. Typically, the classification is done on the basis of post-experimental interviews that prompt participants to answer questions such as "Do you feel this experiment was deceptive (involved lying) in any way?" (Geller and Endler, 1973, p. 49).
 15. Stang (1976, p. 355) mentioned other, in our view less plausible, explanations for the increase in suspicion, namely decreased quality of the cover story and measurement artifacts. On average, experimental designs tend to become more rather than less sophisticated over time.
 16. Stang (1976, p. 354) observed that the percentage of suspicious participants correlated strongly with the year in which the studies were published ($r = .76$).
 17. Conformity experiments, however, are by no means the only areas of research where suspicious participants (self-reported) get excluded (e.g., Sagarin et al., 1998).
 18. The effect size measure we used is *eta*. It is defined as the square root of the proportion of variance accounted for (Rosenthal and Rosnow, 1991), and is identical to the Pearson product-moment correlation coefficient when *F* has only a single degree of freedom in the numerator, as is the case when two conditions are compared (which happened in most cases where we calculated *eta*). According to Cohen's (1988) classification of effect sizes, a value of *eta* of .1, .3, and .5 constitutes a small, medium, and large effect size, respectively.
 19. All effect size computations of this kind (including those reported below) and more detailed discussion of the manipulations and behavioral effects of each study reported here may be found in Hertwig and Ortmann (2002b; see especially Tables 2–5). See <http://home.cerge-ei.cz/Ortmann/recentWPs.html>.
 20. Toy et al. (1989) reported that debriefing happens rarely in marketing research and conjectured that experimenters in marketing are thus trying to avoid this problem.
 21. Incidental learning is an experimental paradigm used to investigate learning without intent.
 22. The weapons effect (originally reported by Berkowitz and LePage, 1967) describes the observation that the mere sight of guns can facilitate aggressive thoughts and behavior (e.g., in terms of number of shocks that a subject inflicts on a confederate subject). Berkowitz (1974) explained this finding in terms of a classical conditioning process: Weapons are associated with aggressive stimuli through their frequent pairing with aggressive acts in real or fictional life. Thus, when an aroused and uninhibited person is exposed to a weapon, it might elicit the responses that have frequently been associated with guns, that is, aggression-facilitating reactions.
 23. This does not mean that 93% of their participants are from introductory courses, as 35% of the responding departments also recruit from other lower division courses (Sieber and Saks, 1989, p. 1057).
 24. In Bardsley's (2000) procedure participants are told that some of the public good scenarios in the experiment would be "fictitious". He showed that under certain conditions it is a dominant strategy for participants to treat these scenarios as "real". His approach allowed him to study how participants react to specific patterns of contributions by others—the major goal of Weimann (1994), who used deception to achieve this goal.

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