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**DELINEATING DECEPTION
IN EXPERIMENTAL ECONOMICS:
RESEARCHERS' AND SUBJECTS' VIEWS**

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Delineating deception in experimental economics: Researchers' and subjects' views

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Abstract

I report the results of a large survey of experimental subjects and researchers concerning the use of deception. I conclude that members of these two groups largely agree on the extent to which various specific techniques are deceptive. I identify the main dimensions that determine this judgment. I also find that the attitude towards deception among subjects tends to be more favorable than among researchers, although even the latter do not readily conform with the common view that deception is taboo in experimental economics.

Keywords:

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1 Introduction

Avoiding deception is often believed to be a hallmark of experimental economics (Davis and Holt 1993, p. 24). Roth (2001), among others, remarks that “[economists] almost never use deception”. This is supposedly duly enforced by editors and reviewers. For instance, Gächter (2009) asserts that “experiments [...] which use deception are normally not publishable in any economics journal.” Wilson and Isaac’s (2007) version is even stronger: “In economics all deception is forbidden. Reviewers are quite adamant on the point and a paper with any deception will be rejected”.

It is further quite widely believed that such a ban is reasonable, to avoid reputational spillover effects (Hertwig and Ortmann, 2001). Allowing for deception would soon make subjects distrustful and compromise experimental control for any economist (I shall refer to this idea as “reputational argument against deception”). Davis and Holt (1993) maintain that “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.” (pp. 23, 24)

Deception would thus be portrayed as hitchhiking on the public’s trust in experimental economists’ professional reputations, which is justly frowned upon and punished by journal rejections.

These assertions are not easy to verify for several reasons. First, there is no agreed-upon definition of deception in experimental economics. Second, the global reputational Armageddon envisioned here can only happen once, so investigating it is difficult. Third, because deception impedes publication in economics, it is a challenge to investigate the impact of deception on reputation even on a local scale. Fourth, as I have experienced myself during the course of this project, deception is a touchy issue – in trying to chart its boundaries (which inevitably involves determining whether specific papers should be labeled “deceptive”), one does not necessarily make friends.

All this is unfortunate because to the extent that potentially deceptive experimental techniques are sometimes useful, we may be unnecessarily restraining progress of our field – it is quite clear that there is a trade-off. Should we accept, without proof, the claim that the corner solution happens to be optimal? Further, as long as most experimental economists agree that deception is very bad but nobody knows exactly and with certainty what deception is and is not, high-quality papers may be unfairly rejected, particularly those written by young authors, who, reviewers might argue, do not yet understand what deception is and why it must be avoided.

This paper seeks to achieve several interrelated goals. First, (in Section 2) I try to formalize the notion of deception. I argue (and of course I am not

the first) that deception is not an all-or-nothing phenomenon, and I propose a classification of deceptive techniques along a spectrum, indicating which ones are more risky or harmful than others. From the most practical viewpoint, I show which techniques are likely to get a paper rejected. In doing so I focus on design choices, such as surprise restart, that are typical for economic experiments. In contrast, previous studies, even those written by economists, have typically devoted a lot of space to “psychological” tricks, such as cover stories, the use of confederates, and the application of experimental manipulations or observations while subjects think they are merely waiting for the experiment proper. Next (in Section 3) I show that even if one adopts the strictest definition of deception, it is not difficult to find studies published in economic journals that fulfill the criteria.¹ In other words, many reviewers and editors make exceptions to what may seem to be a rule carved in stone. Third (Section 4), I argue that a blanket ban on deception enforced by journal editors is very unlikely to be the optimal solution because the fear of possibly devastating consequences to experimentalists’ general reputation from sometimes allowing deception is unsubstantiated. Instead (Section 5) I propose that each journal should specify what types of deception they will not allow or would consider a major weakness in a submitted paper. At the same time, implementing more stringent regulations at the lab level may be a reasonable solution. All of the preceding is largely based on the results of the first (to the best of my knowledge) large-scale systematic survey of experimentalists and their subjects² focused on deception. The procedures applied and the transcripts of the online questionnaires are provided in the Appendices.

¹To substantiate this statement, I provide a number of specific references. This is not meant to be comprehensive, for it would be impossible and not very useful to make such a list. Nor are the studies I mention necessarily “more deceptive” than others that I do not cite. Instead, many of them have been chosen because they are either well known or very precise in their description of the experimental design; hence, it is easier to categorize them. I must emphasize that it is certainly not my intention to criticize specific authors. First, I argue that the negative consequences of using deception for the profession are much overrated. Second, many of the papers I cite are path-breaking studies whose contributions outweigh by far whatever externalities their use of deception may have caused. Finally, some of them are classics from times when budgets were lower, ingenious experimental design tricks less developed and the experimental economists’ attitude towards deception still being shaped. Thus the use of deception was even more justified than it is now.

²I have used subject pools from the experimental labs in Munich and Warsaw.

2 Defining deception in experimental economics

Definitions from other disciplines that have run experiments (as well as considered reputational effects of deception, vide (Kelman, 1967)) for much longer may not seem very useful for economists. For example, according to Broder (1998), “*Deception* may be defined as concealing or camouflaging the real purpose of an experiment (i.e. the data in which the scientist is interested) to avoid conscious reactivity of participants that would make these data worthless” (emphasis in the source text). It appears that most economists perceive concealing the real purpose of the experiment by withholding their hypotheses from the subjects as perfectly fine, whereas they perceive as deceptive many aspects of design that are clearly not covered by this formulation. I was not able to find an explicit definition of deception within economics (and leading authors Hertwig and Ortmann admit in several of their texts that it is not easy to come up with such a definition).

Given that no ready-made definition seems to exist in the literature, let us first identify some building blocks. A message may be *intentional* [INT] or *unintentional* (e.g. resulting from a slip of tongue, typo in instructions or other type of mistake on part of the experimenter). Further, if subjects do not get complete information, the message can be *explicitly false* [EXP] or just fail to convey all the relevant information that the subject may want to have [*incomplete* INC].³ In the latter case, the subjects may be told precisely what information they are barred from [INC-AWARE] or not [INC-UNAWARE]. The message may be likely to change at least some subjects’ behavior as compared to the benchmark of complete information [BEH_REL] or not [BEH_IRREL]. Finally, again, compared to the case of complete information, the message may be likely to significantly decrease subjects’ willingness to participate [PART_REL] or not [PART_IRREL].

The overview of the definitions proposed by the responders to the survey (see point 15 in the Appendix A: survey for researchers) reveals broad consensus for INT being a necessary condition for deception, and I shall thus assume from now on that it is fulfilled. Then, nearly everyone agrees that EXP is sufficient.⁴ As for the other conditions, there is a lot of disagreement. For instance, of 20 responders whose definition explicitly mentioned INC, 11 said such omission of relevant information was not deceptive and nine

³McDaniel and Starmer (1998) use a somewhat frightening notion of “economy with the truth” yet call it “perfectly legitimate”

⁴This is also in line with discussions found in the literature. Hertwig and Ortmann (2008) note that “[a]lthough deception is not easily defined, a consensus has emerged across disciplinary borders that intentional and explicit provision of erroneous information—in other words, lying—is deception”.

said it was. It is a strong indication that, first, INC should be considered more benign than EXP and, second, that it cannot determine per se whether deception is present.

Some examples in which INC is *not* deceptive are quite obvious – for example, if subjects are told that, say, the number of rounds or other subjects’ private information will not be revealed to them, then clearly they are not deceived (although, if possible, it is usually better to at least tell them the distribution from which these values are randomly picked). I therefore propose that INC-AWARE messages are not deceptive.

Finally, it appears that withholding information is quite acceptable when there is little reason to expect that it could alter behavior or diminish willingness to participate.⁵

Definition 1 *The message is Explicitly Deceptive if it satisfies INT and EXP. It is Deceptive by Omission if it satisfies INT, INC-UNAWARE and either BEH_REL or PART_REL.*

I also propose partial ordering of types of deception: Explicitly Deceptive techniques are worse than those Deceptive by Omission, other things being equal. BEH_REL and PART_REL deceptive techniques are worse than, respectively, BEH_IRREL and PART_IRREL, other things being equal. For example, promising subjects that they will be paid if they can successfully solve a task while planning not to deliver on this promise, is INT, EXP, BEH_REL and PART_REL, the worst type of deception.

These impressions are confirmed by my survey data. Among other things, I asked both the experimentalists and subjects to judge, on a 0-7 scale, how deceptive various experimental techniques or design features are, see Table 1 and points 4-14 in Appendix A. Most of them were taken directly from specific published papers; my faculty responders were also provided with a list of these papers (although no explicit link between the papers and the techniques was made) and asked if they could recall whether any specific paper was deceptive or not.⁶ I classified the rated techniques (but this was not shown to the subjects) on three dimensions, EXP, BEH_REL and PART_REL, giving each the value of 0 (does not fulfill), .5 (perhaps does) or 1 (certainly does). For example if “Subjects are told that they are playing with another subject. In fact, the other subject does not exist - his or her moves are computer-simulated. [no other player]”, then I would

⁵One could wonder why then omission would be used in the first place. One typical reason is that it may simplify instructions considerably.

⁶I reasoned it would be too much to require a refined judgment on a 0-7 scale; besides, the entries for papers would not be comparable to those for techniques anyway, because there was obviously very strong selection – most responders refrained from rating papers.

argue, it involves explicit deception that may affect willingness to participate in the first place and is quite likely to change behavior, thus values for EXP, PART_REL and BEH_REL are (1, 0.5, 1).

Researchers' judgments of various techniques being deceptive depended strongly on their EXP, BEH_REL and PART_REL status, see Table 1. For example, the "matching groups" technique (0,0,0) was judged to be the least deceptive of all, while the four techniques with positive entries for all three dimensions all had values above 5 (7=clear case of deception). More systematically, a simple regression (results available upon request) showed that any of these three variables would be a significant predictor of researchers' judgment, despite there being only 11 observations (all of them continued to have positive impact but ceased to be significant when they were all included in a single model). In particular, the belief that "economy with the truth" is "perfectly legitimate" (thus that all techniques with EXP=0 are benign) does not seem to be shared by most researchers – "predictions revealed" was judged as quite deceptive. Of course, given the small number of items and somewhat arbitrary ratings on EXP, PART_REL and PART_REL, these data must be treated with caution, but it provides a clear hint that these variables do affect judgments in the predicted direction.

What is perhaps most striking in Table 1, is that subjects' judgments were remarkably consistent with those of researchers⁷ – the coefficient of correlation between ratings of researchers and Warsaw subjects⁸ was as high as .86. As a result, EXP, PART_REL and PART_REL were also found to induce subjects to judge a given technique as more deceptive (although in this case the coefficients were not significant; this difference was largely driven by the two techniques that were highly deceptive by my definition, yet for which subjects' judgments were much less harsh than those of researchers – [no other player] and esp. [dummy player].) I can see two natural (and not mutually exclusive) ways of interpreting this agreement between the two types of responders. First, it may be that experimenters take into account their largely correct intuition concerning what their subjects might find deceptive. Second, it is possible that such judgments are based on certain natural criteria (such as those that I have specified) that appeal to professionals and lay people alike.

⁷Interestingly, neither group was particularly homogeneous – in each of them the standard deviation for each item was around 2 (ca. 30% of the available rating scale) or higher.

⁸The Munich sample was not asked to rate any specific technique, see Appendix B.

Table 1: *Judgments on possibly deceptive techniques*

Evaluator	Subjects		Researchers	The author		
	Technique		Paper	EXP	PART_REL	BEH_REL
Surprise continuation	3.61	3.35	14%	.5	0	1
Matching groups	2.01	1.69		0	0	0
Dummy player	2.38	5.24		1	.5	1
No other player	3.81	5.75	55%	1	.5	1
Linked questions	4.22	4.94	0%	1	0	1
Endogenous matching	2.03	2.61	24%	0	0	1
Predictions revealed	3.97	3.92	54%	0	.5	1
Non-representative sample	4.39	4.47	43%	.5	0	.5
Forget past earnings	2.02	2.37	0%	1	0	.5
Cover story	4.29	5.05		1	.5	1
Confederate	3.77	5.08		.5	.5	1

Please note: Only Warsaw subjects rated specific techniques. Techniques were rated on a 0-7 scale (0=not deceptive at all). Papers were judged as deceptive or not. For EXP, 1 means explicit deception, .5 means possibly explicit deception and 0 means no explicit deception. Only papers judged by at least 10 responders were included in the table (except that judgments for four different papers that used the “no other player” technique are reported jointly).

3 Are there deceptive experiments out there?

Is it indeed fair to say that experimentalists almost never deceive their subjects? Indeed, my responders claimed to have had little to do with deceptive experiment design in their own work – 48% said none of their papers ever involved deception and the mean self-reported fraction of own deceptive projects was low, equal to 8%. About 7% said that they decided to publish a study in non-economics journal at least once because of potential criticism concerning deception.

Responders were quite critical of deception in others' works but by far did not declare they would uniformly reject deceptive papers. Of those who have ever reviewed a paper for an economics journal that they considered deceptive (as many as 60% of the sample!), 33% said they would always recommend rejection of such a paper, 52% said they would consider deception a major weakness and 15% said it would have little impact on their judgment. Thus, there is negative attitude towards deception, but there is no universal ban.

Twenty-one percent said they had at least one paper criticized because of alleged use of deception. Only one in four agreed with the assessment. Forty percent of those who reported such criticism had their paper rejected or one of the treatments discarded because of the accusations, and a few more complained about having to “fight the editor for eight months” and so forth.

Overall, it appears that some economists run deceptive studies every now and then; face some level of difficulty publishing them in economics journals, partly because of disagreement on whether deception was involved; and frown upon deception in others' work but typically do not immediately recommend rejection. All in all, this summation seems to deviate quite a bit from the “economists-don't-deceive-and-don't-accept-deceptive-studies” assertions.

Another approach to get a grip on cases of deception is to analyze specific published papers. Because deception is obviously quite rare in economic experiments compared with psychology or market research, I have decided against systematic assessment of frequency of such techniques such as undertaken by Christensen (1988) – it would have been a very unrewarding task. Instead, I simply provide a handful of references confirming that, while rare, deception is clearly not absent in economic papers.

3.1 Explicitly deceptive and BEH_REL

The clearest case of behavior relevance is when the deceptive message misrepresents incentives for subjects. Interestingly, such examples can be found even among the most-cited experimental papers such as (Holt and Laury,

2002). The authors first let their subjects run the now-famous risk-preference task with low stakes, promising them “After you have made all of your choices, we will throw this die [...] to select one of the ten decisions to be used [...]. [...] one of these [decisions] will end up affecting your earnings [...]”. When they were done, additional instructions for the high-money-payoff condition were distributed: “Now, we will provide you with the chance to make another choice, with much higher potential payoffs [...]. If you choose to participate in this round, we will not pay you what you just earned in the Option A/B choice that you just finished, i.e. you can choose which one you want to count, and to be added to your earnings from all previous parts.” As Holt and Laury note, nobody declined to participate, which, given that stakes were much higher, was hardly surprising. In other words, the authors knew full well that, for all practical purposes, the choices their subjects were making in the first part of the experiment *had no impact on their earnings whatsoever*. I thus propose that “one of these [decisions] will end up affecting your earnings” was a case of INT, EXP and likely BEH_REL deceptive message.

It is thus remarkable that the technique (which I call “forget past earnings”, see table 1) is judged quite non-deceptive (2.37 on 0-7 scale), and none of responders who had rated (Holt and Laury, 2002) believed it was deceptive! One may speculate that either of two *modus tollens* type of heuristics could be at work here (strengthened by the fact that responders were probably not willing to spend too much time on any single question)⁹:

Deception is not a good experimental practice
Holt and Laury is a great experimental economics paper
 No deception was used here

Deception is bad for those being deceived
Subjects were more than happy in this case
 No deception was used here

As it happens, both generally useful heuristics failed on this particular occasion. In particular, the second heuristic was only partly correct – presumably subjects could have been even happier if they only did the high-stakes part. It should also be noted that Holt and Laury’s approach – used

⁹The second heuristic may also explain favorable rating of the technique by my subject responders.

in order to avoid wealth effects – could have been quite easily replaced by a less deceptive technique. The subjects could have been informed upfront that there was another part coming and that only one part would be randomly selected for real payment. Another apparently deceptive technique involves chained questions. In these designs, experimenters try to economize on the number of items a typical subject faces by dynamically adjusting the parameters of subsequent questions by using data from previous questions. The problem is of course that it generally destroys incentive compatibility. For example van de Kuilen and Wakker (2009) use chained questions and yet told their subjects: “At every decision it is best for you to choose the prospect that you want most. If you select the envelope containing the blue card at the end of the experiment, that decision can be selected at the end of the experiment. Then, the chosen prospect will be played out. Of course you want that prospect to be your preferred prospect.” It is my understanding that the first sentence is intentionally false and changes subjects’ likely behavior. This time, however, there is no simple fix to the problem. Telling the subjects something like “parameters of later questions depend on earlier answers, so it might be optimal for you to misrepresent your preference in some of the questions, but we are not going to tell you how you should do it and it is very difficult to figure it out, so you should probably take the second best and report truthfully”, while being all true, is probably not something that most experimentalists would be happy to see in their instructions.

Perhaps the most often used technique involving explicit deception is the one in which subjects are told that they are playing against other humans, but their counterparts are actually pre-programmed computers. Examples include studies by Weimann (1994) (E6,E7), Blount (1995) (studies 1 and 2)¹⁰, Scharlemann, Eckel, Kacelnik, and Wilson (2001) and Winter and Zamir (2005). As mentioned before, entries in Table 1 (rows “no other player” and “dummy player”) show that researchers generally agreed that such techniques are deceptive while student subjects were much more lenient.¹¹

Several studies in economics have used explicit deception concerning role assignment procedures that may or may not be BEH_REL. For example, in a double oral auction conducted by Ball, Eckel, Grossman, and Zame (2001),

¹⁰Additionally, in the end the participants received a flat fee, regardless of what choices they had made. Because the study involved an ultimatum game using the Minimum Acceptable Offer approach, the deception was not really a necessity.

¹¹Other forms of misrepresenting the composition of the set of players are common as well, e.g. Kim and Walker (1984) told their subjects there were 100 people in each group in their public goods experiment, while in fact there were only five. A similar approach was used by Bohm (1972).

status was supposedly determined based on answers to a quiz, but in truth it was done randomly. The same procedure was used i.a. by Kumru and Vesterglund (2010) and Visser and Roelofs (2011). Gibbons and Boven (2001) made their subjects believe that they could read their counterparts' responses to a personality questionnaire, although in fact these were prepared by experimenters in advance. Again, to the extent that subjects were conditioning on alleged traits of their counterparts, the deception was BEH_REL.

3.2 Deception by omission

Clearly, it is even more common that researchers withhold some relevant information. For instance, Selten and Stoecker (1986) “tried to create the impression that 26 subjects participated in each session and that they never would meet the same opponent again in a later supergame.” In fact, they only interacted in groups of six [matching groups]. Had they known this, their behavior might have been different (e.g. more cooperative).

Surprise continuation Unexpected restart of a multi-round experiment could be the most common type of omission of relevant information. For example, Andreoni (1988) used surprise continuation after round 10 *and then surprise halt* after round 13(!), commenting that “Had the budget for subjects been bigger, this would have been unnecessary. Such deceptive practices are, under less restrictive circumstances, not recommended”. Just as in the case of Holt and Laury, the recommendation to use deception as a last resort (which can be found i.a. in the rules of conduct of the American Psychological Association) does not seem to have been followed in this case. Similar techniques have been used in several other studies, also written by experienced experimentalists (Burnham, McCabe, and Smith, 2000; Gächter, Herrmann, and Thöni, 2004). In some cases they even seem to involve explicit deception rather than deception by omission. Interestingly, although this is a common procedure, it was not universally judged as non-deceptive (see Table 1) although the paper by Andreoni was (perhaps for reasons similar to those for Holt and Laury).

Matching conditional on behavior Another commonly used technique involves re-matching subjects based on their past behavior [endogenous matching]. Again, subjects have no reason to suspect anything like that will happen. For example, Rigdon, McCabe, and Smith (2007) matched “cooperative” types together. Clearly, this would have provided additional incentive

to behave pro-socially in early rounds, had subjects been aware of this procedure, a case of INC, BEH_REL deception.

The same is true of Gächter and Thöni (2005), in which high contributors were matched together in a later part of the experiment. Similarly in Reuben, Sapienza, and Zingales (2009), subjects were not told that their reported expectations would affect role assignment in the trust game.¹² They were simply informed of their assigned role.

Information on others' behavior not representative A less typical technique involves manipulating subjects' subjective norm governing the situation by misrepresenting the distribution of choices within it. For instance, Al-Ubaydli and Lee (2012) showed "average expectations of 10 senders" and failed to mention that these were in fact hand-picked, unusually high (or low) values.

4 What will happen if economics journals allow more deception

The reputational argument for journals' ban on deception actually consist of three assertions:

1. The use of deception in economics will surge as soon as editors become more lenient.
2. Subjects, also at non-deceiving labs, will learn about cases of deception. . .
3. . . and will change their behavior because of resulting mistrust.

Let us try assess the credibility of each of these assertions.

Will experimental economists start to cheat? Advocates of banning deception often point to the neighboring field of (social) psychology, in which deception is quite common. I think that the validity of this analogy is limited. Economic experiments are largely aimed at determining how human subjects would respond to certain sets of incentives. The best way to make these determinations is to implement these incentives. This is quite different from psychologists trying to gain insight into beliefs, appraisals, emotional states,

¹²In several other papers, e.g. Ellingsen, Johannesson, Tjøtta, and Torsvik (2010) experimenters concealed the fact that expectations would be transmitted to the other party.

automatic or cue-driven behaviors, etc. For example, it is very rare that economists feel they could benefit from conveying incorrect information about the purpose of a study.

A desire to observe behavior in situations of special interest that do not often arise naturally (which has frequently driven the use of deception in psychology) may be shared by the two disciplines. However, economists have developed tools (such as the strategy method) that help to alleviate the problem.

Although numerous exceptions can be mentioned, by and large economists tend to investigate conscious, deliberate decisions in an abstract contest, while psychologists are often interested in forces that make us act in one way or another although we may be unaware of them.¹³ Clearly, there is greater need to misinform in the latter case. As Cook and Yamagishi (2008) put it, “insofar as researchers deal with conscious decision-making, deception is primarily a matter of economic (i.e. pragmatic) concerns, not of necessity.”

Another important distinction is the frequency with which direct face-to-face interactions take place in the experiment. It appears that such cases fuel the use of confederates in psychology as opposed to economics. For example, if a psychologist wants to investigate reactions to an insult, a natural idea would be to use, say, jostling and verbal abuse by a hired assistant under cover (a confederate). An economist would instead think of observing reactions to very low offers in an ultimatum game (perhaps using strategy method if they are too rare).

It is also instructive to look at specific subfields of social psychology. It seems that those further away from economics indeed show a greater need for deception, as is the case with conformity studies (Gross and Flemming, 1982). Finally, psychologists but not economists often wish to induce (strong) emotions in their subjects, which may require deception, e.g. in the form of an “unrelated” preceding experiment.

It is thus not very surprising that only 13% of my responders said they would run more deceptive studies if economic journals were more ready to publish them.

Will subjects get to know? Only 18% of either of my subject samples said they ever realized that they had experienced deception, often in what they referred to as a “psychology experiment”. One reason for these numbers being relatively low is that deception is apparently not that easy to notice:

¹³As a side note, even in such cases ingenious techniques may be developed that allow investigating such an issue even if the subject knows the purpose of the study (Implicit Associations Tests etc.)

only 15-18% of those who experienced deception explicitly said they had realized it during the experiment (at least 30% were debriefed). Interestingly, of those who believed they had not participated in a deceptive study, ca. 30% in the Munich sample and ca. 50% in the Warsaw sample spontaneously added a remark that they may have been involved in such a study yet failed to notice.¹⁴

Deception in economic experiments may be even harder for subjects to detect than in a typical psychology study. First, as of now there is little, if any pressure on debriefing subjects in economics. One could argue that this is simply due to participants having complete information to begin with, but that is not so. I have provided some counterexamples in the previous section; moreover, champions of *field* experiments seem to imply that it is fine to use subjects unaware of the experiment and refrain from debriefing them *ex post facto*. Clearly, this has to do with debriefing being indispensable if it helps to avoid psychological harm, which is rarely an issue in economic experiments. Further, anonymous interaction via a computer network, which is typical for economic experiments, clearly makes it more difficult to notice, for example, that the interaction partner was in truth pre-programmed by the experimenter compared with direct, face-to-face contacts often seen in psychological science labs. All in all we do not expect subjects to associate economic experiments with deception.¹⁵

Refraining from the use of deception in economics while it continues to be present in psychology and other social sciences could only make a difference if (a) economists' subjects rarely take part in psychology studies or (b) they can very clearly see the methodological difference between the two types. My data yields rather limited support for either of these claims. In both of my samples, the majority said they had participated in at least one psychology experiment. In the Munich sample, the reported perceived frequency of deception in economic experiments was 3.7 (0=never deceive, 7=always deceive), thus lower than for psychology (4.7) but still very substantial.¹⁶ Similarly, psychologists rated 3.9 on a 0-7 trust scale when conducting experiments, while economists only fared somewhat better, scoring 4.7.¹⁷

¹⁴All of the numbers reported in this paragraph are based on the author's rating of the responses to the open-ended question on experience of deception (question 10 in Appendix B).

¹⁵And even if deception turns out to be an attractive option in some subfields, subjects are unlikely to guess whether they happen to be participating in such a study or not.

¹⁶A previous study (Barrera and Simpson, 2012) reports no difference in perceived frequency of use of deception by sociologists, social psychologists and economists at all.

¹⁷Both differences are significant. These questions were not included in the Polish questionnaire.

These limited differences would suggest that the costly effort of economists to protect their reputation is not paying off.¹⁸ One important reason may be that subjects cannot tell whether they are participating in a “psychological” or “economic” study, let alone whether the study is going to be submitted to a psychology or marketing journal (and hence deception is quite possible) or to an economics journal (thus deception is unlikely).

It could be that first-hand experience of deception (Barrera and Simpson (2012) used the term “direct exposure”) affects subsequent behavior. Still, the world is and will continue to be full of people who have never participated in an experiment, as aptly noted by Henrich (2001). The use of deception will thus only be a real problem if indirect exposure matters as well. A possible channel through which such subject pool polluting could happen is via teaching. However, unlike in psychology, economic experiments do not often make it to undergraduate textbooks. And if they do, then typically they will be the studies with no deception. In my student samples only 11-17% (and much less among non-economics majors) said they were told about “several” economic experiments during their curriculum. More importantly, only 2-5% were told about “several” *deceptive* economic experiments, almost three quarters said they had never heard about a single one.

It is worth noting that the between-lab reputational concerns were not shared by all researchers who participated in my study. Fifty percent said deception destroys the reputation of the entire profession, 33% – only the lab involved and 17% thought the reputational threat was limited.¹⁹

Will subjects’ behavior change? Several researchers have tried to assess the impact of deception on attitudes towards research and researchers, suspiciousness and willingness to participate (see Hertwig and Ortmann (2008) for a review). The results have been rather mixed. Further, as suggested before, types of possibly deceptive techniques that economists are typically tempted to use (e.g. endogenous re-matching of participants) are quite different from the most popular forms of deception in psychology (the use of cover stories, confederates and bogus stimuli, see Sieber, Iannuzzo, and Rodriguez (1995)). Additionally, subjects in these studies were psychology students, whose attitude towards deception may be strongly shaped by their curriculum. It

¹⁸Of course this could be due to spillover between disciplines; if so, the current state of affair is still undesirable and only coordinated efforts of all social scientists could make a change in the long run.

¹⁹As a side note, the opinions concerning the ethical aspect were similarly dispersed: 31% said that for ethical reasons deception should generally not be used and 51% believed it was justifiable in some cases, while 17% saw little problem with deception from an ethical viewpoint.

would therefore be unwarranted to assume that deception in experimental economic studies would have similar results.

Further, I was able to find only two studies that address the impact of deception on *behavior in subsequent experiments*, which is the type of impact truly relevant to the reputational argument. Jamison, Karlan, and Schechter (2008) reported that subjects that were randomly selected for a deceptive treatment ([no other player]) made more erratic choices in a Holt and Laury task at a later session. However, this was the only significant effect out of several measures and actually the one for which we would *least* expect any effect in the first place – the others were social tasks in which mistrust whether the counterpart is real or simulated might play a major role. Barrera and Simpson (2012) also pointed out the high drop-out rate (nearly 40%) in the study by Jamison and colleagues,²⁰ a weakness they were able to fix in their own study, finding no impact of direct (firsthand) exposure on behavior whatsoever.

Barrera and Simpson (2012) also investigated indirect (secondhand) exposure (see their Study 2). Again, they found that subjects who had just read a detailed description of a study involving explicit deception did not behave any differently than the control group exposed to an analogous description including no deception.

To investigate possible effects of direct exposure, I asked all my student responders to rate a number of items reflecting possible reactions to experienced deception, see Table 2. What is most important for my line of reasoning is the observation that the ratings on future distrust were very different, depending on whether it concerned the very experimenter who had conducted a deceptive study, another experimenter at the same lab or any experimenter anywhere. The values for the latter item, which are crucially important for the reputational argument, were relatively low.²¹

As mentioned before, the subjects were also asked to recall in a free form any experience of deception in an experiment (question 10 in Appendix B). The responses were subsequently rated on a number of dimensions. One

²⁰Jamison et al found that deception had no impact on drop-out rate in the entire population. Interestingly, they reported interaction with gender – deceived females returned significantly less often but deceived males significantly more often, a rather unexpected finding.

²¹A number of other interesting observations can be readily made. In particular, in line with the results of Jamison and colleagues, subjects were unlikely to drop out because of deception (low entries for abstain and discourage) and females seemed to react consistently stronger than males in either sample. Further, ratings on different items were reassuringly consistent across samples, although entries tended to be higher in the Warsaw sample. Finally, the subjects did not perceive deception as highly unethical yet clearly supported the notion of debriefing.

Table 2: *Subjects’ anticipated reactions to deception*

	Warsaw		Munich	
	Male	Female	Male	Female
Fooled	4.05	4.77	3.67	4.09
Abused	3.44	4.26	2.77	3.43
Unethical	3.06	3.76	2.16	2.75
Distrust this experimenter	4.60	5.11	4.29	4.38
Distrust this lab	3.95	4.37	3.53	3.90
Distrust any experimenter	2.71	3.02	2.97	2.83
Abstain	1.16	1.64	1.55	1.87
Discourage	1.43	1.97	1.59	1.86
Warn	3.88	4.09	3.22	3.49
Debrief	2.10	2.38	1.58	1.93

See Appendix B for explanations of the variables. Scale from 0-strongly disagree to 7-strongly agree, except for debrief (-3 should not debrief to +3 should debrief).

important observation is that subjects did not experience much resentment – about 14% of either sample reported some negative emotions (“...I felt helpless and not well ...”, “...I was a bit annoyed ...” etc.) while some 10% in the Warsaw sample and as many as 22% of the Munich sample recalled positive feelings (“It was fine, I had to think hard what exactly was true and what was not ...I had to focus but I did not give me an uneasy feeling”, “I realized that I was deceived. I was rather amused when I noted this”, “I did not feel deceived, perhaps surprised, but, generally speaking, in a positive way”, “I considered [the deceptive technique used] very interesting, I was captivated” – these are author’s translations from German or Polish). These findings are broadly in line with the assertions of Smith and Richardson (1983) and Christensen (1988).

5 Specifying policy towards deception

The evidence and reasoning laid out in the previous sections strongly suggest that there is little rationale indeed for a stubborn “we don’t publish any deceptive papers” journal policy. I believe that the bulk of responsibility should be shifted to the labs. It is there that reputation effects (if any) may mostly affect subjects’ behavior. Punishing deceptive studies with rejections is an unnecessary burden. Still, it is not even likely to be effective in advancing its stated goal. Indeed, it is next to impossible for subjects to tell whether

a study is going to be submitted to an economic or, say, marketing journal. Thus, if reputation spillover effects were there, rejecting deceptive studies only in economics journals would still be insufficient.

Instead, the journals could ask for evidence concerning rules on deception used in the lab where the study was run, especially in cases in which subjects' behavior might suggest that the experimenter was not reliable. This policy would make sense even under the present editors' strict anti-deception attitude. Alas, to the best of my knowledge, it is not currently practiced.

Further, I think that, at both the lab and journal level, the rules concerning deception should be much more nuanced. I have argued in section 2 that not all deception techniques were made equal. Each journal and each lab can and perhaps should specify which types are always acceptable, which must be painstakingly justified, and which are taboo. In the case of labs, this information would then have to be carefully communicated to the subjects. For example, a lab accepting INC as long as it is followed by debriefing and it is not PART_REL could announce on its website something like "... we do not use deception. We spare no effort to ascertain that everything you read in the instructions or are told during the experiment is true. In some cases, though, it may be that you are not provided with some bit of information that could potentially affect your decisions in the lab. In such situations, we are obliged to explain it thoroughly at the end of the experiment. We always do our best to make our participants come back to the lab ..." etc.

As far as I can tell, economics labs presently do not have such detailed rules. I have inspected websites of 122 labs listed at <http://leem.lameta.univ-montp1.fr>.²² Of those, 87 (71%) did not seem to announce any rules concerning the use of deception. Of the 35 that did, the large majority displayed a short statement that "the use of deception at [lab] is prohibited" or similar.²³

Curiously, deception is proscribed but never defined (and the same holds true for the information I have gathered via an informal e-mail survey of a

²²The list is longer but some links seem to be broken or labs inactive. There were a few duplicates. I have also excluded websites in languages other than those that either I or one of my research assistants could understand (Dutch, English, French, German and Spanish). Obviously I have not considered my own lab.

²³Interestingly, in a few cases, a specific reference to "reputation" was made. The CESS lab of New York announces that the "laboratory and subject pool cannot be affiliated with any experiment that involves deception. The laboratory is very concerned about developing and maintaining a reputation among the student population for honesty; this is to ensure that subject actions are motivated by induced monetary rewards rather than by psychological reactions to suspected manipulation... 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".

small group of lab managers).²⁴ The only exception that I could find was the detailed rules of the Konstanz lab: “. . . no experiments are conducted in which the participants are consciously [INT] deceived. The main criterion is whether it is expected that the participants would behave differently if they had been aware of the actual course of the experiment from the beginning [BEH_REL]” (author’s translation from German, with references to the concepts developed in this paper added) which also give specific examples of unacceptable techniques [surprise continuation, matching groups].

The journals in turn, could specify the minimum requirements for the rules implemented in the lab where the study under consideration was conducted (e.g., no EXP in conjunction with PART_REL or BEH_REL) and provide much-needed guidance for the referees (e.g. that they should verify that the announced lab rules were observed and take the strictness of these rules into account when judging whether the study was optimally designed and whether subjects’ behavior could be explained in terms of mistrust). I believe such a system could prove to be much fairer and more efficient than the present practice of rather haphazardly smashing otherwise decent papers with the “economists don’t deceive” hammer.

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²⁴The Berkeley Xlab represents an interesting case, for “[a]s of 1 January 2011, the Xlab will be a deception-free environment”.

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Appendix A: Survey for experimentalists

Procedures

Subscribers of the Economic Science Association Google group, the most active and largest forum focused on experimental methods in economic research, were sent a link to the online questionnaire (see next paragraph) on December 13, 2012, followed by a reminder on December 19. In total, 143 scholars responded.²⁵ The e-mails specifically invited the receivers to take part in a survey on deception, so it is likely that researchers with well-defined views on deception were overrepresented. This is not necessarily a bad thing, given that it is this group that is likely to shape the prevailing practice and opinion concerning deception now and in the future. Still, I had a healthy mix of experience levels: responders had run economics experiments for any time period between zero and 30 years (mean=10.3). About 20% were graduate students, 10% post-doc researchers, 20% assistant professors, 15% associate professors and 27% full professors. Thirty-four percent of responders declared having run most of their experiments in the United States and 43% in one of several European countries (notably Germany or the Netherlands). There were 29% females in the sample.

The questionnaire

[also available here: <http://profitest.pl/start/82/ATccOLQbNQJBsINn>] [Eight different orders of questions were used. Some variables were significantly affected by their location in the survey but general picture remains unchanged in all version; details are available from the author.]

This survey concerns the use of deception in laboratory experiments in economics. It will take about 15 min. to complete. You will remain anonymous unless you choose otherwise. The results will be used for research purposes only.

1. People have different opinions concerning whether or not it is OK to deceive subjects. Which of the following comes closest to what you think about the use of deception in economics experiments from an ETHICAL VIEWPOINT:

- (a) For ethical reasons, deception should generally not be used.

²⁵It is not clear exactly how many members the group has in total. As of December 26, 2012, its website says 2226, but the fact that the “two” most active members are Glenn Harrison and Glenn (Harrison, I presume) suggests that duplicates may be common.

- (b) From an ethical standpoint deception may be justified in some cases.
 - (c) In a large majority of cases, the use of deception does not pose a substantial problem from an ethical standpoint.
2. Some people claim that deception has a very negative impact on the professional reputation of experimental economists, which may affect experimental control in the long run. Others disagree. Which of the following comes closest to what you think about the use of deception in economics experiments from a REPUTATIONAL VIEWPOINT:
- (a) Deception destroys the professional reputation of all experimental economists.
 - (b) Deception only destroys the professional reputation of the lab that uses it.
 - (c) The reputational threat of using deception is of minor importance.
3. Which of the following best describes what you do when you conclude that authors of a paper you are reviewing for an economic journal have used deception:
- (a) I recommend rejection, no matter how good the paper is otherwise.
 - (b) I consider it a substantial drawback.
 - (c) It has little impact on my assessment of the paper.
 - (d) I can't recall having reviewed a paper involving deception.

Please look at each of the descriptions of features of experimental designs. Would you say that they involve deception or not? [0 to 7 scale for each case, whereby 0-clearly no deception 7-obvious case of deception; additional space for comments provided]

4. The experiment involves a type of cooperation game. Subjects are told the experiment will go for 10 rounds. In fact, after round 10, they are told that new groups will be randomly formed and additional 10 rounds will be played.[surprise continuation]
5. Subjects are randomly re-matched in each round of a game, but only within pre-determined "matching groups", e.g. only within 10-person groups in a lab seating 20. They are not informed about such a restriction in the matching procedure. [matching groups]

6. Subjects are told that they are playing with another subject. In fact, there is another subject but he or she has no say - his or her moves are computer-simulated.[dummy player]
7. Subjects are told that they are playing with another subject. In fact, the other subject does not exist - his or her moves are computer-simulated. [no other player]
8. Subjects are told it is in their best interest to answer each question truthfully because it may be selected for real payment. In fact, however, their responses determine parameters of subsequent questions so it may actually be beneficial for them to misrepresent their true preference. [linked questions]
9. Subjects' responses in early rounds of a game determine the formation of groups in subsequent rounds (e.g. subjects are matched on their "cooperativeness" as shown in early rounds). Subjects are not told about this link.[endogeneous matching]
10. Subjects are asked to predict behavior of their partners in a game. Their predictions are revealed to these partners but, when making the predictions, they are not informed that this would happen.[predictions revealed]
11. Subjects are shown the mean of choices of 10 participants in the game they are to play. What they are not told is that these are actually 10 extreme choices in a much larger sample. [nonrepresentative sample]
12. Subjects are told that one of their 10 choices would matter. When done, they are asked if they are willing to make some new choices, one of which would matter, on the condition that they forgo the earnings from the first block (whatever they may be). The payoffs in the new choices are obviously more attractive (so that every subject is actually willing to play the new block).[forget past earnings]
13. Subjects are actively misled as to the purpose of the experiment.[cover story]
14. The experimenter hires a "confederate" who poses as one of the subjects yet behaves exactly as ordered by the experimenter.[confederate]
15. What is your DEFINITION OF DECEPTION?
Now, please answer the following questions:

16. What fraction of your laboratory experiments in economics have involved some form of deception? [0-100%]
17. Most experimentalists agree that the use of deception often makes it difficult to have that paper published in an economic journal.
18. If the attitude among journal editors became more lenient as far as the use of deception is concerned, would you conduct more or less studies involving deception than you do now? [0-much less to 7-much more]
19. Have you ever had a paper of yours criticized because of the (alleged) use of deception? Did you agree with the critique? Was a paper of yours ever rejected in a journal because of such a judgment (perhaps among other factors)? [OPEN-ENDED QUESTION]
20. Have you ever decided to publish your paper in a non-economics journal because the study involved deception? [YES, NO]

Below is a list of studies in experimental economics that, some could claim, did involve deception. To be sure, others could think differently, it may well be a matter of judgment and the definition of deception that someone adopts. Please also note that the list is not meant to be a collection of “most deceptive” experiments in economics. Nor is it a comprehensive list of studies in experimental economics that someone could consider deceptive. Some of these studies involve a possibly deceptive technique that has been used in several other projects and have been selected as representative of a class, either because they happened to be known to the author of the survey or because they are particularly influential or precise in the description of experimental design. If you are familiar with any of these papers and you are convinced that they did or did not use deception, please indicate as such below. [DECEPTIVE, NOT DECEPTIVE. The rated studies are provided in the list of references and marked with a star (*)]

21. If you recall other published experiments that you believe involved deception, please add them here:
Finally, a few points about yourself:
22. Gender [man, woman]
23. Qualification level: [Master student, PhD student, post-doc researcher, assistant professor, lecturer, associate professor, full professor, other:___]

24. For how many years have you been conducting research experiments?
[0-50]
25. In which country have you conducted most of your laboratory experiments? []
26. Name, surname, affiliation (optional)
27. E-mail address (optional)
28. Additional comments [OPEN-ENDED]

Thank you for your time! I will be happy to acknowledge your assistance if you care to type in your name above. If you have further questions or comments or you want to see the results, you can also contact me at mkrawczyk@wne.uw.edu.pl. I would appreciate that, for the time being, you do not discuss the points raised here on the ESA mailing list or similar forums so as to give everyone the chance to express his or her own independent opinion.

Appendix B: Survey for subjects

Procedures

This survey made use of two databases: subject pool of the Laboratory of Experimental Economics at the University of Warsaw (predominantly students or very recent graduates) and the subject pool of MELESSA lab at the University of Munich. They had either already participated in economic experiments (some of them in several) or had only recently registered to start doing so. They were sent an e-mail resembling any other that invited them to take part in a study. The e-mail specified that the survey was focused on deception. Polish subjects were promised that a few prizes of ca. 50 euro each would be randomly distributed among participants (provided they agreed to be identified by their e-mails). Eventually almost 400 people out of nearly 1000 registered took part, and three monetary prizes were allocated. Similarly, two Amazon gift certificates of 25 euro each were promised and sent out to the German responders. Two-hundred twenty completed the questionnaire, out of some 1200 subjects addressed.

The questionnaire

[The Munich version was in English, although participants were allowed to respond to open-ended questions in German. It is available here: <http://profitest.pl/start/113/MjpX>
The Warsaw version was in Polish: <http://profitest.pl/start/95/R2q9hPUmcz5cxLYg>
[Four different orders of questions were used.]

[The Warsaw version started with explanations of the nature of experimental economics, deception in experiments and the disclaimer that the Warsaw LEE researchers do not use deceptive techniques. Subsequently the subjects were shown items 4-14 of the researchers' questionnaire (minus experimental economics jargon) and asked to rate each of them on the (0-clearly no deception to 7-obvious case of deception) scale]

[questions 1-2 below were only included in the German sample]

1. If you were to participate in an experiment run in an ECONOMIC SCIENCE laboratory, would you generally trust what the person in charge was telling you regarding the course of the experiment? [0 to 7 scale for each case, whereby 0-not at all 7-completely]
2. If you were to participate in an experiment run in a PSYCHOLOGICAL SCIENCE laboratory, would you generally trust what the person in charge was telling you regarding the course of the experiment? [0 to 7 scale for each case, whereby 0-not at all 7-completely]

In some studies, experimenters use DECEPTION, that is, they intentionally give their participants incorrect information or withhold important information from them. Such techniques may be attractive from methodological viewpoint.

Consider, for example, an experiment in which the experimenter tries to learn something about the participants' racial prejudice. If participants realize what the experiment is about, they may try to hide such bias (if any). The experimenter may attempt to prevent this effect by giving the participants misleading information concerning the purpose of the experiment (a cover story).

The type described above is but one of many different experimental techniques that some people find deceptive. People may have different opinions about what constitutes a case of deception and under what circumstances it can be acceptable. The goal of this survey is to better understand how experiment participants perceive deception.

3. How often do you think ECONOMISTS use deception in their experiments?
4. How do you think, how often do PSYCHOLOGISTS use deception in their experiments?
5. Generally speaking, what is your attitude towards deception in experiments?
[the remaining questions were asked in both samples, albeit in a slightly different order]
6. During your university curriculum, have some of your teachers told you about (findings from) economic experiments (for example some that tested theories presented in the class)? [never/once/a few times/several times]
7. During your university curriculum, have some of your teachers told you about (findings from) economic experiments INVOLVING DECEPTION (for example some that tested theories presented in the class)? [never/once/a few times/several times]
8. Approximately in how many experiments in an economic science lab have you participated?
9. Approximately in how many experiments in a psychological science lab have you participated?

10. Have you ever participated in an experiment that involved deception? Could you describe it in detail? Did you discover deception or has the experimenter explained that it was used? How did you feel about it? [open-ended question] [in the Munich version the disclaimer explaining that econs typically do not deceive was shown here]
- Now, please imagine you are participating in AN EXPLICITLY DECEPTIVE STUDY – YOU ARE BEING ACTIVELY MISLED BY THE EXPERIMENTER and you realize just that after the experiment. How do you think it would affect your feelings and behaviors? [from 0-strongly disagree to 7-strongly agree]
- [in the Warsaw version the subjects were instead asked to focus on the techniques they have considered “most deceptive” among those rated (points 4-14 of the researchers’ questionnaire)]
11. I would feel I have been fooled. [fooled]
12. I would feel my trust in the experimenter has been abused. [abused]
13. I would think the experimenter has behaved unethically. [unethical]
14. In the future, I would not trust this experimenter any more (for example I would tend to disbelieve the instructions I get at the beginning of an experiment).[distrust this experimenter]
15. In the future, I would not trust experimenters at the same laboratory any more (for example I would tend to disbelieve the instructions I get at the beginning of an experiment).[distrust this lab]
16. In the future, I would not trust any experimenter any more (for example I would tend to disbelieve the instructions I get at the beginning of an experiment).[distrust any]
17. In the future, I would rather not participate in experiments any more.[abstain]
18. In the future, I would tend to discourage my colleagues from participating in experiments.[discourage]
19. In the future, I would warn my colleagues that they should not readily believe the experimenter.[warn]
20. If you participated in such a deceptive experiment, would you like the researcher to explain to the participants at the end of the experiment how they have been misled and why it was necessary or do you think

the researcher should not do that? [from -3=the experimenter should certainly not give such explanations to +3=the experimenter should certainly give such explanations], [debrief]

21. Age
22. Gender [man, woman]
23. Major [Economics/Business, Psychology, Other social science, Science, Law, Arts, Other:-----, I have never been a student]
24. Year of study [1, 2, 3, 4, 5, doctoral studies, I am not a student]
25. What is your e-mail address? (we will only use it to notify if you're randomly selected as one of prize winners)
26. Any additional comments



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