

2014

The Meaning of Deceive in Experimental Economic Science

Bart J. Wilson

Chapman University, bjwilson@chapman.edu

Follow this and additional works at: https://digitalcommons.chapman.edu/esi_working_papers

Recommended Citation

Wilson, B.J. (2014). The meaning of deceive in experimental economic science. ESI Working Paper 14-05. Retrieved from http://digitalcommons.chapman.edu/esi_working_papers/21

This Article is brought to you for free and open access by the Economic Science Institute at Chapman University Digital Commons. It has been accepted for inclusion in ESI Working Papers by an authorized administrator of Chapman University Digital Commons. For more information, please contact laughtin@chapman.edu.

The Meaning of Deceive in Experimental Economic Science

Comments

Working Paper 14-05

The Meaning of Deceive in Experimental Economic Science[†]

Bart J. Wilson*
Economic Science Institute
Chapman University

4 March 2014

Two principles distinguish experimental economic science from other experimental social sciences. First, we experimental economists pay our participants based upon the decisions they make. Incentives matter. Not only might it seem hypocritical not to practice what we preach, but we are also concerned that the experiment's rewards are salient and achieve dominance. Smith (1982b, pp. 931, 934) explains:

In order that subject rewards in a laboratory experiment have motivational relevance such rewards must be associated indirectly with the message actions of subjects. This is called *Saliency*: Individuals are guaranteed the right to claim a reward which is increasing (decreasing) in the goods (bads) outcomes...of an experiment...

A condition sufficient to guarantee that we have not lost control over preferences...[is called] *Dominance*: The reward structure dominates any subjective costs (or values) associated with participation in the activities of an experiment.

Note the subtle ethical imperatives: "must be," "guarantee," and "the right to claim." The integrity of our experimental conclusions rests on the soundness of our methods. A casual perusal of experimental papers reveals that the average salient earnings in economic experiments, i.e., excluding any payment for showing up on-time, range roughly from US\$10 for an hour long session to US\$40 for a two-hour long session.

Secondly, we do not use deception, not out of any deontic aversion to dishonesty, but rather because we are "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" (Davis and Holt, 1993, pp. 23-24).¹ Again, our concern is for the integrity of the inferences from our observations.

While there is apparent wide agreement on what it means to maintain salience and achieve dominance of rewards, the same is not true of deception. Hertwig and Ortmann (2008) open their answer to the question of *What is deception?* by uncontentiously positing that "[d]eception is not easily defined" (p. 61). Krawczyk (2013) conducts a survey of experimental

[†] I thank George DeMartino, Andreas Ortmann, David Rojo Arjona, and Vernon Smith for constructive comments that have improved this essay.

* Email: bartwilson@gmail.com

¹ As good practicing utilitarians, experimental economists are consummate consequentialists.

economists and confirms that there is no clear agreement in practice on what deception is. My goal in this chapter is to shine a little light on the subject.

Is 'What is Deception?' a Perspicuous Question?

Like ordinary people, economists are serial nominalizers. If people do not sign up for retirement plans, it is because of hyperbolic discounting; if someone bids aggressively in a first-price auction, it is because of risk aversion; if proposers offer more than the minimum increment in an ultimatum game, it is because of fairness; etc., etc. So it is not surprising that when it comes to the ethics of experimental economists, we pose the question as about *deception*, the noun.²

Perhaps the form of the question befuddles us. As Wittgenstein (1958) recognized, “the mistake we are liable to make [is]...that we are looking for a ‘thing corresponding to a substantive’” (p. 5). A thing is something we classify by its boundaries, its features. Is this pest a termite or an ant alate? Well, let’s look at the wings, antennae, and waist. Is this procedure deception by commission or omission? Well, let’s look at the experimenter’s behavior and/or the participants’ perceptions of the experiment (Ortmann and Hertwig, 2002; Hertwig and Ortmann, 2008). But deception isn’t a thing like a household pest is a thing; it’s a conceptualization of what we *do* and so it doesn’t have boundaries that make it readily identifiable. I know I have a termite problem and not an ant problem if the specimen on the floor has fore and hind wings of equal size, beaded antennae, and a broad waist. In contrast, we cannot envision all the forms of deception that we may encounter because there is no finite set of characteristics by which to classify it.

That doesn’t deter us from trying to categorize the features of deception. Ortmann and Hertwig (2002) propose a boundary with the imperative that “default assumptions should be avoided” (p. 113). They explain by example what a default assumption is: “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” (p. 113). The difficulty with creating this boundary, as they mention in an endnote, is (p. 126):

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 sample pool.” One sensible approach to assessing default assumptions would be appropriately constructed pilot sessions.

² I am no exception. My list of protocols to the Institutional Review Board includes the statement that “I adhere to the principle of never using misinformation or deception in my research.”

Invoking appropriateness does not take us far, for it adds yet another unclear feature as to what deception is.

In his survey of experimental economists, Krawczyk (2013) makes the distinction between instructional statements to participants that “may be likely to change at least some subjects’ behavior” and those that convey “complete information” (p. 3). Ortmann and Hertwig’s referee would ask, “How will we know what complete information is and what would change some subjects’ behavior?” Complete information amounts in practice to what economists themselves need to know in order to solve the mathematical representation of a problem presented to experimental participants. Not only is this odd—delimiting the meaning of dishonesty by the current reach of positive economic analysis—but it also rules out as honest inquiry questions not deducible from economic theory. As we shall see, there are many questions regarding changes in participant behavior that are not covered by complete knowledge. Must an experimenter be deemed dishonest by a charge of deception to explore them?

No, but we do avoid pinning a scarlet *D* on an experimenter by posing the question as *What is deception?*. Listen to the tone of questioners in seminars and conferences and watch the body language of the presenter. To question a procedural detail as deceptive comes rather close to calling into question the ethical character of a person, and in everyday conversations that carries strong emotional force. Posing the question as *Is that not deception?* softens the charge and puts the focus on the procedural detail, not the experimenter and his or her moral character. But here’s the rub. The experimenter is responsible for each and every detail of the experiment. The experimenter did what he or she did for a reason, to answer a specific question. It is the experimenter of whom we can ask questions and inquire about what is in fact the case or not the case. So rather than work with the nominalization of the problem, let’s let the action of a verb do the work and pose the question like we generally do for ethical infractions: Who did what to whom?

Did the Experimenter Deceive the Participants?

Notice how this question differs from *What is deception?*. We are not trying to settle what deception is by covering all the positively creative ways that we can construct an experiment. We are asking a question of a specific case, of something that has happened, of what someone did, and the answer to this question rests on the meaning of the word *deceive*. I propose that we use pithy definition #1 from Dictionary.com: to mislead by a false appearance or statement.

Two elements must be established with this definition to attain an experimenter as having done something to deceive a participant. First, the experimenter must have wrongly led

or wrongly guided. Whatever assistance the experimenter gave the participants to make decisions, some component of what the experimenter *did* must have been wrong. Secondly, as in a court of (common) law, establishing wrongness of actions consists in assessing evidence with reference to objective criteria. What criteria might these be? A false appearance or a false statement.

Consider the case of Ortmann and Hertwig's default assumption. Instead of posing the question in terms of the default assumptions of the participants, let's examine the experimenter's action with the following yes-or-no questions directed to the experimenter.

Questioner: As the participants entered the laboratory, did you collect data as a part of this experiment?

Experimenter: Yes.

Questioner: Do you collect data as part of every other experiment of yours as participants enter the laboratory?

Experimenter: No.

Questioner: That seems peculiar and difficult to reconcile as a genuine appearance. Why did you choose as part of this experiment to collect data as the participants enter the laboratory?

At this point, like in all research, we must rely on the integrity of the experimenter. In the subsequent sections I consider several economic experiments and answer the question, did the experimenters mislead the participants by false appearance or statement?

Kim and Walker (1984)

Let's start with a straightforward case. In this public goods experiment with voluntary contributions Kim and Walker instruct their participants that "[t]here are exactly 100 people involved in this experiment, including yourself" (p. 16). They disclose to the reader, however, that "[w]e did not in fact use 100 subjects in the experiment...Instead, we used only five subjects" (p. 19). By their own admission Kim and Walker misled their participants by false statement.

Burnham, McCabe, and Smith (2000)

Krawczyk (2013) includes this experiment as an example of "deception by omission" for the use of a "surprise continuation" of the session. In Burnham, McCabe, and Smith's words, though, "[t]here was no deception. Subjects were given the instructions for Single play [of an extensive form trust game] with no mention of whether there would be other experiments. After Single play, subject were paid and told that today they would be part of a second experiment" (p. 64). Burnham, McCabe, and Smith instructed their participants on how to play their trust game and how their decisions mapped into cash payoffs. After the participants

made their decisions, the experimenters paid the participants as they said they would. Then they told the volunteers that they could participate in a second experiment.

A false statement about the second experiment would be informing the participants upfront that there would be one and only one play of an extensive form trust game and then springing the surprise second experiment on them. Burnham, McCabe, and Smith made no such false statement, but is the surprise second experiment a false appearance? At first blush this case reminds us of Ortmann and Hertwig's default assumption. Is the surprise continuation part of this experiment? Yes. Is the surprise continuation part of every other experiment of yours? No. But these answers don't appear as peculiar in tandem. Why? Because entering the laboratory in the Ortmann and Hertwig example is a necessary part of conducting an experiment in a laboratory. It appears in every experiment and so guiding participants to their seats as if it does not matter constitutes a false appearance—not unlike treating a confederate as if he or she is not a confederate is a false appearance.³ The key, I argue, is what did the experimenters *do* and the appearance thereof. The experimenters collected data as the participants entered the laboratory. In other experiments they do not do this. The appearance is the same in both cases but former's appearance is a false one because as they collected data they appeared not to be doing something when in fact they were.

A surprise second experiment is not necessary for a project, so what would the experimenters do if they didn't conduct a surprise second experiment? Exactly the same thing as when they conducted the first experiment plus a surprise second experiment, just without the second experiment. There was no false appearance that there was only one experiment when in fact there was a second experiment. Nor was there a false appearance to the second experiment. The second experiment simply appeared. The surprise continuation does not constitute leading the participants because there is nothing to lead the participants to. That the experimenters knew something at the start of the experiment that the participants did not is not a matter of the experimenters *doing* anything to mislead the participants by false appearance or statement.⁴

³ Krawczyk (2013) reports some agreement among his respondents that the use of confederates is deceptive. The average rating is 5.08 on a 0-7 scale (with 0 representing not deceptive at all).

⁴ The editor raises the interesting question as to what the experimenters would have done had a participant inquired at the start of the experiment whether there would be a second or third experiment. To deny that there would be a subsequent experiment would be to mislead by false statement. So to avoid deceiving the speculating participant the experimenter would have to plead the 5th. This question raises a separate, but related issue with conducting surprise experiments, namely the negative externality of participants second-guessing the experimenters. Recall Davis and Holt's reasoning for not deceiving participants, an expectation among the participants that they are not being deceived by the experimenter. I submit that one reason why experimental economists consider a surprise continuation to be deceiving a participant is that both can lead to second-guessing of future experiments by repeat participants. A procedure may invite second-guessing but that doesn't make it

Holt and Laury (2002)

This well-cited article for measuring risk aversion pushes the boundaries of the previous case. Here is what they said to the participants in what they called the “Option A/B” choice:⁵

You will make ten choices and record these in the final column, but only one of them will be used in the end to determine your earnings...After you have made all of your choices, we will throw this die twice, once to select one of the ten decisions to be used, and a second time to determine what your payoff is for the option you chose, A or B, for the particular decision selected. Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used.

The second throw of the die is the realization of the lottery that has been randomly selected for payoff. After all decisions have been made, the experimenters passed out another page of instructions. This is the first paragraph of the second set of instructions:

Now, we will provide you with the chance to make another choice, with much higher potential payoffs, as you can see from the sheet we are passing around. As always, we will pay you the exact amount you earn in this part, and we have the cash required. 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. The procedures are exactly as before, after you make your ten decisions, we throw the die once to determine which decision is to be used, and then we throw the die to determine the payoff for that decision.

Unlike Burnham, McCabe and Smith, Holt and Laury did not complete the first experiment. They wished to avoid having the realized wealth from the first lottery affect the choices in the second lottery with much higher stakes. Did Holt and Laury mislead their participants by false statement? Holt and Laury said that they “will throw this die twice” for the first experiment, but they never did because all of the participants chose to forego their first experiment earnings. They also said that “only one of these [Option A/B decisions] will end up affecting your earnings.” If we were to ask Holt and Laury what they would have *done* had a participant elected not to participate in the second experiment, we would have to take them at their word if they said that they would have executed the first experiment lottery and subsequently dismissed the participant. At some point with these questions we reach bedrock and can’t avoid relying on the integrity of the scholars, and this makes it difficult to conclude that Holt and Laury misled their participants by false statement.

Gunnthorsdottir, Houser, and McCabe (2007)

In 1997, “a good decade between first sessions and eventual publication,” Gunnthorsdottir, Houser, McCabe, and Holly Ameden were interested in how the

deceptive. Not all digits are thumbs. Let’s be precise about the everyday meaning of *deceive* and its application to experimental economic science.

⁵ Accessed at <http://people.virginia.edu/~cah2k/highdata.pdf> on February 15, 2014.

disappointment of expectations in the early rounds of a public goods experiment contributed to the standard decay of voluntary contributions (see, e.g., Isaac and Walker, 1988).⁶ The stylized facts of the voluntary contributions mechanism for public goods is that some people start the experiment contributing all of their tokens to the “Group Account,” others contribute none, and some others hedge splitting their tokens between the Group and Private Accounts. Over time, however, the full contributors stop fully contributing and the hedgers stop hedging. What if, Gunnthorsdottir et al. surmised, the full contributors met other full contributors in the first period, would their contributions still decay over time?⁷

Since full contributors aren’t identifiable ex ante by a mark on their foreheads, they designed a procedure by which a pool of participants would first decide their contributions and then the software would allocate the individuals to proper subsets of four-person public goods. In the baseline condition the software randomly assigned participants to different subgroups of four people. In the other condition, the software assigned the four highest contributors to one group of four, the next four highest contributors to another group, and so on. To hold constant the expectations of whom the participants may be meeting, the experimenters did not inform the participants in either treatment on how they were being matched. Here are their exact instructions for both conditions:⁸

Each period proceeds as follows:

First, decide on the number of tokens to place in the private and in the public account, respectively. Use the mouse to...

Second, once everyone has submitted his or her investment decision, you will be assigned to a group with 4 members (including yourself). Your total group investment will then be determined and your experimental earnings calculated.

Third, you will receive a message with your experimental earnings for the period. This information will also appear in your Record Sheet.

A new period will begin after everyone has acknowledged his or her earnings message.

After the last period, you will receive a message with your total experimental earnings (sum of earnings in each period).

Rigdon, McCabe, and Smith (2007) applied a similar procedure to a two-person extensive form trust game. In the baseline condition, first movers were randomly matched with second movers. In the sorting condition, more trustworthy second movers were matched with more trusting first movers. Their instructions for both conditions state that “[t]he joint decisions made by you and your counterpart for that period will determine how much money

⁶ Daniel Houser (personal communication, February 18, 2014).

⁷ Their answer is no, they sustain high contributions without decay.

⁸ I received a copy of the instructions by contacting the first author.

you will earn in that period. After each period you will be re-paired.”⁹ My co-authors and I adopted a similar one-time matching mechanism in Wilson, Jaworski, Schurter, and Smyth (2012). Our instructions said nothing about how the participants were matched (and not one of 144 participants at two different universities ever asked).¹⁰ For the first 13 periods only two avatars appeared on each of the twelve participant screens, and in period 14 two pairs joined them out of nowhere to form a sextet.

Not once in the instructions of these three experiments did the authors mislead the participants. The experimenters created an exercise in which they simply did not inform the participants on how the pairs or groups were formed. A false statement in these experiments would be explicitly telling the participants that the experimenters randomly formed the groups when in actuality they sorted according to the participants’ actions. In all three experiments the task was what it was; there were no false appearances or false statements.

And yet something about this bothers many economists.¹¹—This must be deception by omission because had the participants known about the matching algorithm, they would have acted differently.—Yes, if the authors had informed the participants of the matching mechanism, this knowledge would have likely changed their decisions, but that is beside the point and misses the point. These authors are not interested in how people behave when they know how they are being matched. What people do knowing how they are matched is a different and, I submit, a less interesting question than what people do when they have no idea of how they are matched.

The unexamined assumption for claiming deception in these experiments is that because the experimenter knows something that the subject does not, that somehow the experimenter is lying to the subjects. Experimenters know a lot about their participants’ tasks that their participants do not: what the Nash equilibrium is, how the last session performed, why this is an interesting question, etc. Participants in the canonical double auction experiment, for example, are typically not told what all the values and costs are. In fact, the point of that experiment is to demonstrate that the participants can realize nearly 100% of the gains from trade even without knowledge of the supply and demand arrays (Smith, 1982a). Not

⁹ Available in a working paper dated February 15, 2006.

¹⁰ Answering this question in the other two designs requires a stoic response. Having conducted the sorting trust game version probably a hundred times in workshops and courses, inevitably a student will ask, “How are we matched?” Or, “Are we matched randomly?” My reply is to simply reread the relevant sentences from the instructions.

¹¹ One referee for Wilson, Jaworski, Schurter, and Smyth (2012) expressed his or her concern thusly: “A more pertinent issue would seem to concern whether or not telling subjects how their groups are formed is a deceptive practice or at least something that spells long run loss of control. It seems to me what is done is OK, but perhaps questionable. There is this design feature subjects are not told about but which if they knew about it would possibly have changed their behavior in the early stages.”

telling the participants about all the costs and values, moreover, is not a “loss of control” in the experiment. To wit, we experimenters are expressly interested in observing what people do without that knowledge.¹²

Not providing information in and of itself is insufficient grounds to conclude that the experimenters have deceived the participants. The nontrivial difference between misleading by false appearance or statement and simply not providing information in an experiment is *a matter of fact that can be demonstrated to be false*. We cannot articulate a general rule on knowing things (by experimenters or participants) that delineates when experimenters have deceived their participants and when they have not. We can, however, examine what the experimenters *did* in guiding their participants through the experiment and judge the evidence of those actions as involving a false appearance or false statement. Rather than attempting to classify experimental procedural details as deception or not, I propose a negative test of experimenter actions: Did the experimenters mislead the participants by false appearance or statement?

References

- Burnham, Terence, Kevin McCabe, and Vernon L. Smith. 2000. “Friend-or-Foe Intentionality Priming in an Extensive Form Trust Game,” *Journal of Economic Behavior and Organization* 43(1): 57–73.
- Davis, Douglas D. and Charles A. Holt. 1993. *Experimental Economics*. Princeton, NJ: Princeton University Press.
- Gunnthorsdottir, Anna, Daniel Houser, and Kevin McCabe. 2007. “Dispositions, History and Contributions in Public Goods Experiments,” *Journal of Economic Behavior and Organization* 62(2): 304–315.
- Holt, Charles A. and Susan Laury. 2002. “Risk Aversion and Incentive Effects,” *American Economic Review* 92(5): 1644-1655.
- Hertwig, Ralph and Andreas Ortmann. 2008. “Deception in Experiments: Revisiting the Arguments in Its Defense,” *Ethics & Behavior* 18(1): 59-92.
- Isaac, R. Mark and James M. Walker. 1988. “Group Size Effects in Public Goods Provision: The Voluntary Contributions Mechanism,” *Quarterly Journal of Economics* 103(1): 179-99.
- Kim, Oliver and Mark Walker. 1985. “The Free Rider Problem: Experimental Evidence,” *Public Choice* 43: 3-24.

¹² Logico-deductive economic theory cannot handle this open question because there is nothing from which to deduce outcomes. This makes some economists uncomfortable, and perhaps they conflate this feeling that something is wrong with the notion that experimenter has done something wrong.

- Krawczyk, Michał. 2013. "Delineating Deception in Experimental Economics: Researchers' and Subjects' Views," Faculty of Economic Sciences Working Paper, University of Warsaw.
- Ortmann, Andreas and Ralph Hertwig. 2002. "The Costs of Deception: Evidence from Psychology," *Experimental Economics* 5(2): 111-131.
- Rigdon, Mary L., Kevin A. McCabe, and Vernon L. Smith. 2007. "Sustaining Cooperation in Trust Games." *Economic Journal*, 117(522): 991-1007.
- Smith, Vernon L. 1982a. "Markets as Economizers of Information: Experimental Examination of the 'Hayek Hypothesis'," *Economic Inquiry* 20(2): 165-179.
- Smith, Vernon L. 1982b. "Microeconomic Systems as an Experimental Science," *American Economic Review* 72(5): 923-955.
- Wilson, Bart J., Taylor Jaworski, Karl Schurter, and Andrew Smyth. 2012. "The Ecological and Civil Mainsprings of Property: An Experimental Economic History of Whalers' Rules of Capture," *Journal of Law, Economics and Organization* 28(4): 617-656.
- Wittgenstein, Ludwig. 1958. *The Blue and Brown Books*. New York: Harper & Row.