Experimental Economics' Inconsistent Ban on Deception

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Abstract

According to what I call the 'argument from public bads', if a researcher deceived subjects in the past, there is a chance that subjects will discount the information that a subsequent researcher provides, thus compromising the validity of the subsequent researcher's experiment. While this argument is taken to justify an existing informal ban on explicit deception in experimental economics, it can also apply to implicit deception, yet implicit deception is not banned and is sometimes used in experimental economics. Thus, experimental economists are being inconsistent when they appeal to the argument from public bads to justify banning explicit deception but not implicit deception.

Keywords: experimental economics, deception, public bad

1. Introduction

There is no formal ban on explicitly telling subjects a falsehood in economic experiments. The closest thing to such a ban is a mention of deception in the guidelines in the editor's preface to the first issue of *Experimental Economics*, the leading journal in the field, according to which "Papers must meet certain high standards in terms of methodology... Also, any deception should be carefully explained" (Holt and Schram, 1998). Nevertheless, journals informally ban explicit deception by almost never publishing papers employing explicit deception and research requiring it almost never gets funded (Cook and Yamagishi, 2008, p. 125).

This informal ban is endorsed by many experimental economists since they "believe that deception is highly undesirable in economics experiments, and for this reason, they argue that the results of experiments using deceptive procedures should not be published" (Davis and Holt, 1993, p. 24). While several arguments are advanced in the literature on the subject, the most common and the most convincing argument is what I call the 'argument from public bads', according to which if a researcher deceived subjects in the past, there is a chance that subjects will discount the information that a subsequent researcher provides, thus compromising the validity of the subsequent researcher's experiment.

Nevertheless, experimental economists can still get their work published even when they tell their subjects things that while not explicitly false are nevertheless misleading. In this paper I discuss the argument from public bads (APB) in favor of banning explicit deception in experimental economics and argue that economists' attitudes are not consistent. If the APB can be taken to justify a ban on explicit deception, it can also be taken to justify a ban on implicitly deceptive experimental methods.

In §2 I present the APB and discuss the negative effects deceptive experimental methods can have on non-deceptive research. In §3 I discuss some purported benefits of using deceptive experimental methods. In §4 I argue that the APB can apply to implicitly deceptive research methods. In §5 I conclude that if the APB is successful, it justifies banning both explicit and implicit deception.

2. The argument from public bads

John Hey (1991, p. 398) succinctly expresses what seems to be a general view among experimental economists regarding deception: "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." Thus, deception is taken to only be the deliberate telling of a falsehood.[1] In this section I spell out the argument that motivates many economists to be in favor of a ban

[1] Although such a definition of deception seems excessively narrow, in the next two sections I follow the standard way economists use it to prevent confusion. In §4 I expand my definition and distinguish between explicit and implicit deception.

on deception in experimental economics - the argument from public bads.[2] First, I present a highly cited experiment (Forsythe et al., 1994) which does *not* use deception, in order to discuss how, according to the APB, deceptive experiments *could* have an adverse effect on non-deceptive experiments. Second, I discuss why many economists take the APB to justify a ban on using deception in experimental economics.

In their paper, Forsythe et al. (1994) test whether a concern with fairness (conceived as an unconditional disposition to give to others) can by itself explain senders' willingness to make nontrivial offers that deviate from the sub-game perfect Nash equilibrium in two simple and widely used bargaining games – the ultimatum and dictator games.[3] Forsythe et al. hypothesize that if the discrepancy between the game theoretic predictions and the experimental results can be explained solely by the senders' concern with fairness, then the senders would offer the same amount in both the dictator game and the ultimatum game. However, Forsythe et al. find that senders are more generous in the ultimatum game than in the dictator game. While Forsythe et al.'s results appear to be valid, according to the APB, if other experimental economics researchers deceived their subjects in the past, then Forsythe et al.'s results might not be valid.

The APB starts by assuming that in any given experiment, such as Forsythe et al.'s, subjects' beliefs regarding the experimental setting are partially determined by their beliefs and partially by the information the researcher provides. If current subjects believe that a researcher deceived subjects in the past, it is reasonable for them to believe that Forsythe et al. *might* use deception as well.[4] Such subjects will, to some degree, discount

[2]While I focus on the APB, it is not the only argument in favor of a ban on deception in economics. Some economists think that deception should not be used in an experimental setting because it is morally wrong. Other, for example, McDaniel and Starmer (1998) argue that deception ought to be banned in experimental economics in order to sustain the respectability of experimental economics in the eyes of economists in general. Alternatively, Hey (1991) argues that deception should be banned because it exposes the researcher to litigation.

[3]In the dictator game the sender is given a sum of money to divide between herself and the receiver as she pleases. In the ultimatum game the sender is given a sum of money to divide between herself and the receiver, but the receiver can either accept the offer, in which case both the sum is divided accordingly, or reject the offer, in which case neither player receives any money.

[4]I leave open whether the APB only works if subjects are aware of deception by ex-

the information that Forsythe et al. provide. Forsythe et al., who provided their subjects with carefully selected information in their experiment in order to set the subjects beliefs, would not know to what extent the subjects would discount the information provided. Consequently, Forsythe et al. would not know the subjects' beliefs in their experiment.

What are the consequences of the fact that Forsythe et al. would not have known the subjects' beliefs in their experiment? If subjects' behavior in the experiment is understood to be a function of their beliefs, their preferences, and their available actions, then to make inferences regarding the subjects' preferences from their behavior, Forsythe et al. needed to know the subjects' beliefs and their available actions. Forsythe et al. knew the subjects' available actions, which for the senders was to offer a division of $10 between sender and receiver, because these were designed by them.[5] Forsythe et al. also knew the subjects' behavior, which was for senders in the dictator game to offer far less than the senders in the ultimatum game, because they observed it.[6] Yet if Forsythe et al. did not know the subjects' beliefs, they could not have made inferences regarding the subjects' preferences from the subjects' behavior.

Kim and Walker (1984), is a published economics paper that explicitly deceived their subjects by telling them that there are 100 participants in the experiment when there were actually only five participants.[7] Kim and

periencing it directly as subjects in past experiments or if it is enough that they become aware of deception indirectly (e.g. from friends who participated in such research or reading about deceptive experiments in academic journals). Both opponents (Bonetti, 1998) and advocates (Ortmann and Hertwig, 2002) of a ban on deception do not think there is evidence that learning about deception indirectly affects behavior in subsequent experiments. Since currently researchers very often share subjects and subjects participate in multiple experiments, even if the APB only works when subjects experience deception directly, the APB's consequences are still worrying.

[5]In the first set of experiments the sum to divide was $5.

[6]The actual numbers Forsythe et al. (1994, p. 362) mention are: "[I]n the $10 dictator game 21% of the players are pure gamesmen and 21% give away an equal share (none give more than an equal share), whereas in the $10 ultimatum game there are no pure gamesmen and 75% offer at least an equal share."

[7]The fact that some papers that use deception get published in economic journals makes clear that the ban on deception is not absolute. Since some of these papers are explicit about using deception, one cannot simply write off their getting published as due to an oversight on the part of the journal editors and reviewers. A more complex picture emerges, one which Krawczyk capture through his survey:

Walker's experiment was meant to examine free riding behavior in 'large' groups (around 100 individuals) in a public goods scenario. However, since paying 100 subjects was prohibitively expensive for them, Kim and Walker opted instead to use only five subjects, whom they explicitly deceived into believing that they were part of a group of 100 subjects by telling the subjects that "[t]here are exactly 100 people involved in this experiment, including yourself" (p. 16).

Imagine that Forsythe et al.'s subjects were aware of Kim and Walker's deception. First, Forsythe et al.'s experimental design provided ample opportunities to use deception. The fact that the senders and receivers were placed in separate rooms and communication between members of a sender-receiver pair was through written forms that were carried between rooms by the researchers allowed Forsythe et al. to manipulate offers, generate new offers, or not actually have real receivers. Second, Forsythe et al. had a financial motivation to use deception. If Forsythe et al. merely gave the same instructions to the senders without actually carrying out their instructions, Forsythe et al. could have saved nontrivial amounts of money – all the money that went to the receivers. Third, if the senders had suspected that Forsythe et al. were deceiving them about the existence of real human receivers in the other room, it plausible that they would make lower offers than if they wholeheartedly believed Forsythe et al. that real people were receiving the money.[8] Forsythe et al., however, did not deceive their subjects.

If senders suspected deception and believed that there were no receivers, they would not be guided by any considerations of fairness (or benevolence) to the non-existent receivers, let alone guided *solely* by considerations of

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> 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. (Krawczyk, 2013, p. 7)

[8]For a discussion on whether subjects actually alter their behavior in subsequent experiments after being subjected to deception, see (Ortmann and Hertwig, 2002; Hertwig and Ortmann, 2008a; Jamison et al., 2008) who think that subjects alter their behavior, and (Bonetti, 1998; Barrera and Simpson, 2012) who think they do not. Hey (1991); McDaniel and Starmer (1998) argue that the mere possibility of deleterious effects is a sufficient reason to worry about deception.

fairness. Consequently Forsythe et al. would find that senders are not guided solely by considerations of fairness (just as they actually did).

But Forsythe et al.'s findings would not be valid, because it is not clear to what extent the different behavior in the dictator and ultimatum game is influenced by a suspicion that there are no receivers playing the game. Finding that subjects are not guided solely by considerations of fairness when they suspect that there is no agent to be fair to, is different than finding that subjects are not guided solely by considerations of fairness when they actually believe that there is an agent to be fair to. Thus, if other researchers deceived their subjects in the past and the APB is sound, it seems that Forsythe et al.'s results would not be valid.

According to the APB, if subjects believe that researchers might be deceiving them, the researchers will not be able make inferences regarding the subjects' preferences from the subjects' behavior. Since making such inferences is the goal of at least some economic experiments, for a researcher to succeed in doing this, other experimental economists must be discouraged from using deception. While it seems plausible that there will be some level of use of deception in experimental economics that will not invalidate the work of experimental economists in general, at present it is not clear what such an acceptable level is. Thus, if experimental economic journals want researchers succeeding in making interesting inferences, they can play it safe and discourage researchers from using deception in their experiments by banning papers that use deception.[9] Since getting one's research published is a central goal for researchers, if a researcher's experimental results will not get published, she has little reason to conduct the experiment.

While many economists find the APB convincing, deceiving subjects is a commonly accepted methodology among psychologists (Christensen, 1988). This difference in attitudes towards deception is made clear by Hertwig and Ortman's attempt to convince psychologists to ban, or more seriously regulate, the use of deception in psychology (Hertwig and Ortmann, 2001; Ortmann and Hertwig, 2002; Hertwig and Ortmann, 2008a,b). Yet if the APB is only convincing to economists but not to psychologists, and deception is

[9] While the argument is presented here only with respect to economic journals, there are other individuals, groups and institutions that can reduce the motivation to conduct research that involves deception, and might be motivated to do so. Examples might be institutions that fund experimental economic research (e.g. the NSF), committees who determine academic hiring, as well as the practicing economists themselves.

not banned in psychology, then the supporters of a ban in economics must believe that the ban in economics is sufficient for avoiding the negative externalities that deception in psychology might have and that there is little or no spillover effect from psychology to economics. For this to be true, it must be the case that, as Grether and Plott (1979, p. 629) believe, subjects can differentiate between economics experiments and psychology experiments, and so are able to restrict their discounting of the information provided by the researcher only to the latter. Whether subjects can differentiate between the two disciplines or there is a spillover effect is an empirical question that has not yet been answered.[10]

3. Why use deception in experimental economics?

If using deception in economic experiments is even only potentially problematic to the discipline at large, what benefits could deception have as a methodological tool that can make it seem worth defending? This is especially interesting since experimental economists have come up with some ingenious ways of getting around actually deceiving their subjects, such as providing some subjects incentives to deceive other subjects, and so act as a deception subcontractor, which allows the researcher to keep her hands clean of deception (Alberti and Güth, 2013; Erat, 2013). In this short section I lay out three possible benefits some economists think deception has as a methodological tool.

First, deceiving one's subjects can sometimes allow significant financial savings. For example, Kim and Walker (1984) (discussed earlier) are explicit that they deceived their subjects to cut costs: "We did not in fact use 100 subjects in the experiment: the cost of doing so would have been far too great" (p. 19). However, while keeping expenses low is an important concern, paying subjects is generally considered a necessary cost of conducting economic experiments, and if one cannot pay subjects they probably ought not do experimental economics.[11]

Second, deception can also be useful for achieving control when testing a hypothesis. Scharlemann et al. (2001) ask "[d]oes smiling elicit trust among strangers?" (p. 619). The subjects were led to believe that they were playing with the pictured subjects but were in fact playing against a pre-programmed

[10] For some work on the subject see (Krawczyk, 2014).

[11] I thank an anonymous reviewer for raising this point.

strategy. Scharlemann et al. deceived their subjects so that they could control for amounts offered while alternating between smiling or non-smiling pictures.

Third, as Cook and Yamagishi (2008) argue, the use of deception may be indispensable for examining some aspects of non-conscious and automatic behavior, such as the effects of elicited aggression on punishment frequency and duration (Ohira, 1989). Similarly, Ariely and Norton (2007, p. 337) view deception as often necessary for creating situational pressures and eliciting spontaneous or unconscious reactions similar to those in the real world, for example by subliminally priming subjects. While fraught with ethical problems, the Milgram (1963) obedience study is an extreme example as it did make interesting findings that could not have been made without deception.[12]

4. Expanding the argument from public bads

Assuming that the APB is a good argument, it also applies to other methodological practices that are currently not banned. For the APB to work a researcher must affect subjects' beliefs in a way that might lead subjects to discount the information that other researchers provide in subsequent experiments. One way a researcher might do this is by using explicit deception. Yet this is by no means the *only* way subjects' beliefs can be affected.

Until now, since I was following the regular usage in experimental economics, when I used the term 'deception' I restricted it to only encompass messages that are explicitly deceptive and argued that the APB motivates many economists to be in favor of a ban on explicit deception in experimental economics. I now distinguish between explicit and implicit deception, and discuss three implicitly deceptive experimental methods that have been published in experimental economics journals.

4.1. Explicit and implicit deception

Hey (1991) restricts what he calls deception in experimental settings only to cases in which the researcher explicitly tells the subjects a falsehood.[13] Such a restrictive definition of deception masks the fact that the APB can apply more widely. By contrast, philosopher James Mahon offers a more inclusive definition:

[12] Another example is Gächter and Thöni (2005), which I discuss later in the paper.

[13] For more discussion on the issue see (Hertwig and Ortmann, 2008a; Krawczyk, 2013).

> To deceive = to intentionally cause another person to have or continue to have a false belief that is known or truly believed to be false by bringing about evidence on the basis of which the person has or continues to have the false belief. (Mahon, 2007, pp. 189-190)

On Mahon's definition more things count as deception, such as intentionally letting a subject believe that all the "players" in an experiment are human, despite some being simulated.[14]

Michał Krawczyk (2013), an economist, agrees that deception encompasses more than explicit falsehoods. Krawczyk divides deceptive messages in an economic experimental setting into those that are explicitly deceptive and those that are deceptive by omission. According to Krawczyk, a message is "Explicitly Deceptive" if it is intentional and explicitly false. It is "Deceptive by Omission" if it is intentional and fails to convey all the relevant information that the subjects may want to have without the subjects being made aware of this and if either the message may be likely to change at least some subjects' behavior as compared to the benchmark of complete information or the message may be likely to significantly decrease subjects' willingness to participate (pp. 3-4).[15]

Krawczyk's definition captures much of what Mahon takes to constitute deception in general, yet it also usefully distinguishes between something being explicitly deceptive and it being deceptive by omission, which for convenience I denote "implicit deception." In his survey, Krawczyk shows that implicitly deceptive experimental methods are not subject to the same ban that explicitly deceptive methods are subject to, and that papers that employ implicit deception do get published.

[14]See for example Selten and Stoecker (1986).

[15]Both Mahon's and Krawczyk's definitions require intentionality for an act to count as deceptive. The APB, however, only requires that the researcher do something that induces in subjects certain background beliefs that might distort their behavior in a future experiment. Because the APB makes no appeal to intentionality, whether there was an intention only matters for the act being labeled as deception. Consequently, the APB can apply equally well to non-deceptive methods. All that matters for my purposes is that the APB can apply to methods that are *not explicitly deceptive*, not whether we classify them implicitly deceptive or non-deceptive.

4.2. Matching conditional on behavior

Matching conditional on behavior, discussed by Krawczyk (2013), is implicitly deceptive. This kind of matching aims to examine how different "types" of subjects (e.g. altruists, conditional cooperators and free riders) behave in different groupings. To do this a researcher needs first to determine which subjects are of what type, and then match them accordingly. This can save time and money because it eliminates the need for researchers to run enough rounds so that subjects get randomly allocated into the type-based groups the researchers seek to examine. Instead, researchers just need to run one behavior-revealing round and then group subjects based on type in the main round of interest.

Rigdon et al. (2007), for example, sought to examine whether the level of cooperation and its stability can be encouraged by population clustering. To do this Rigdon et al. had subjects play a single shot trust game with 20 different partners under two treatments.[16] In the Random treatment the partners were assigned randomly, and in the Sorted treatment they were sorted according to their trust score which in essence measured how cooperative the subjects were. Rigdon et al. did not tell subjects in their Sorted treatment that their partners were not being chosen at random:

> We did not reveal the exact assignment rule to any of the subjects because we were concerned that such information might generate a difference in strategic behaviour. This is especially the case in the Sorted environment - knowing that cooperators are being matched each period might lead individuals to alter their type for strategic reasons rather than due to reciprocity type motives. (996)

Running a similar conditional matching but for a different reason, Gächter and Thöni (2005) did not tell subjects during the preliminary experiment that their actions in it would influence their grouping because they wanted to measure subjects' cooperation preference as accurately as possible in the preliminary experiment in order to group subjects in the main experiment based on levels of cooperation. Because Gächter and Thöni were interested

[16]The trust game expands on the dictator game by allowing the receiver to either have a certain sum of money divided equally between her and the sender, or allow the sender to unilaterally divide a larger sum, and so risk receiving less than she was offered upfront.

in examining whether there is higher cooperation among subjects who know they are paired with like-minded subjects, the subjects were informed during the main experiment that they were grouped homogeneously. As a result, the more cooperative subjects were in the preliminary experiment, the better placed they would be to have higher earnings in the main experiment, since they would be placed with other cooperators that are more likely to donate more to the public pot. Gächter and Thöni make it clear that they believe it is in the subjects' interest to act strategically in the first round (e.g. pretend to be an altruist) in order to maximize payoffs in the second round, and so they made sure not to divulge this information.

In neither of these experiments were the subjects explicitly lied to or told something untrue, and so they were not explicitly deceived. While in the Gächter and Thöni (2005) experiment subjects learned about the implicit deception directly during the second part of the experiment, in the Rigdon et al. (2007) experiment subjects had no way of discovering the implicit deception directly (they might have still discovered the deception indirectly).[17] However, knowing that who subjects were matched with depended on what they did in the first part of the experiment would have allowed the subjects to increase their payoffs. Appearing to be cooperative in the first part of the experiment would result in being matched with other cooperative players, and so increase the likelihood of getting higher payoffs. Knowing this, or merely suspecting that this is the case, would give subjects reason to misrepresent their type as being more cooperative than they actually are.

4.3. Role assignment procedures

Allocating status based on answers to an exam is implicitly deceptive, yet it allows researchers to maintain experimental control. The idea is to examine whether status, divorced from any of the usual causes of status (beauty, intelligence, wealth, etc.), can have a causal role in social interactions. The implicit deception is necessary in order to create an illusion of status among the subjects that is not dependent on any *real* advantage to the members of the higher status group have.

Ball et al. (2001) allocated status to subjects by summing the five numerical answers to an economics quiz taken by the subjects. In some cases

[17]While subjects could potentially be made aware of deception through debriefing at the end of the experiment, debriefing subjects following an experiment is not standard practice in experimental economics.

a gold star and a round of applause, which supposedly confer ‘status’, were awarded to those with the highest cumulative sums, and in other cases these honors were conferred to those with the lowest cumulative sums (this was meant to assure randomness of the status allocation). The subjects were not told on what basis the gold stars were awarded, merely that it was based on their exam answers. Since the sum of the answers had nothing to do with the correctness of the answers, status was *de facto* allocated randomly. However, there is no explicit deception *per se* because the status was based on answers to the quiz.

Kumru and Vesterlund (2010) performed a similar assignment of status based on summing up numerical answers to trivia questions instead of economics questions, which they suspected might be viewed by subjects as unfair. Kumru and Vesterlund are explicit in the thought and effort they put into creating this atmosphere of status:

> We first called out the ID numbers for those who were assigned to the star-group. One by one they were invited to come to the front of the room where they were given a shiny black folder with a gold star as well as a congratulatory ribbon that they were asked to wear for the remainder of the experiment. A public applause was given once all six members of the star-group were standing at the front of the room. Members of the star-group were then seated in the two front rows of the laboratory. The walls of this section were marked by three large gold stars, and the individual computers had a gold-star sticker attached to the board. While seating members of the star-group, members of the no-star-group were asked to come and receive a yellow manila folder, and were then seated in the back two rows of the laboratory (p. 718)

Again, while it is strictly correct that the allocation of status was based on answers in the quiz, it seems likely that subjects thought that the status was conferred based on *correct* answers, rather than simply an addition of the numerical values and high status based on the highest value. If subjects did not make this mistaken inference, the researchers would not have been able to examine how subjects behave with respect to status, since the subjects would not attribute status to one another. Thus, the success of the experiment depends on the subjects *mistakenly* believing that status was conferred based on correctly answering the quiz.

4.4. Surprise restarts

In the surprise restart method the researcher adds additional rounds to the experiment beyond those that she informed the subject about. James Andreoni (1988) told his subjects that they would play the game exactly 10 times (p. 294), and then, after their tenth round of play, they were "unexpectedly told" that they would restart a new set of 10 rounds (p. 295). According to Andreoni, the surprise restart helps to tease apart strategic behavior from learning. If a subject contributed in earlier rounds to the public good and changed her behavior in later rounds, one cannot distinguish whether it is the result of learning what is in the subject's self-interest to do or whether the subject was behaving strategically by signaling cooperation in earlier rounds and reverting to free-riding in later rounds. If after a surprise restart the subject behaves similarly to the way she did in the first ten rounds, this would indicate that her behavior was due to her playing strategically, whereas if now she simply free-rode the entire game her behavior in the previous rounds could be attributed to learning.

Andreoni's design was explicitly deceptive for two reasons. First, Andreoni told his subjects that the game would be played "exactly" ten rounds but then added an additional ten rounds. The phrasing could have easily been rectified by dropping the word 'exactly' in order not to be considered explicitly deceptive. Second, play was suspended after only three additional rounds, despite subjects being explicitly told that it would last ten additional rounds. In a footnote Andreoni explains that: "Had the budget for subjects been bigger, this would have been unnecessary. Such deceptive practices are, under less restrictive circumstances, not recommended" (p. 295).[18] Here too explicit deception was easily avoidable by simply telling subjects that there will only be three additional rounds.

Nevertheless, Andreoni's surprise restarts introduced a novel method for teasing apart learning and strategic behavior. Indeed, several researchers followed Andreoni in using surprise restarts in their work (Croson, 1996; Cookson, 2000; Merlo and Schotter, 1999), yet they did so while being careful to avoid repeating Andreoni's explicit deception. However, surprise restarts are implicitly deceptive. The reason surprise restarts are useful is that subjects

[18]By "deceptive practice" Andreoni is referring to playing only an additional three rounds instead of an additional ten, not to the surprise restart in general. This is one example of holding a narrow view of deception solely as explicit deception.

believe that when the researcher tells them there will be 75 rounds, that is the total amount of rounds there will be. Merlo and Schotter, for example, state that "[a]fter the 75 rounds... subjects were then informed that they would perform the experiment one more time. . . They had not been told about this extra experiment until after they had finished their 75-round experiment." (p. 31). Of course Merlo and Schotter did not tell subjects there will be *only* 75 rounds, and so (unlike Andreoni) they were not explicitly deceptive. However, the additional round would not have been helpful for the researchers if subjects suspected that there would be more rounds beyond the 75 and so were not at all surprised.

4.5. Implicit deception and the APB

Many researchers employing these research methods get published in experimental economics journals, which shows that there is no *de facto* ban on their use and that many economists do not think that implicit deception is as problematic as explicit deception. Yet if the APB is a valid argument, it applies to implicitly deceptive methods just it applies to explicitly deceptive ones.

If subjects who are aware of explicit deception believe that researchers might use explicit deception in future experiments and so might discount information those researchers provide in their experiments, subjects who are aware of implicit deception might discount information researchers provide for the same reasons. Once such a discounting is in place, the rest of the APB would apply to implicit deception in the same way it applies to explicit deception.[19]

Nonetheless, when Burnham et al. (2000), for example, discuss their surprise restart experiment they do not seem to suppose that surprise rounds can be as problematic as explicit deception since they are careful to stress that they are not "deceiving" their subjects:

> There was no deception in the experiment. Subjects were given

[19] Subjects might discount information provided by researchers either narrowly or broadly. Subjects narrowly discount information if in cases that the researcher tells the subjects that there will be a certain amount of rounds, subjects will discount this and believe there might be more. Subjects broadly discount information if the subjects simply do not trust subsequent researchers across the board and discount *all* information provided by them. How the discounting works, if it does at all, is a matter for empirical investigation.

> the instructions for Single play with no mention of whether there would be other experiments. After Single play, subjects were paid and told that today they would be part of a second experiment.

Believing that sometimes researchers implicitly deceive their subjects can affect subjects' behavior. For example, believing that in experiments that match conditional on behavior researchers have subjects reveal their type in early rounds in order to match them based on their type in latter rounds gives subjects reason to discount some information a researcher might provide and instead play strategically even in experiments in which there is no such advantage with respect to payoffs. Alternatively, although in experiments that for all practical purposes allocate 'status' randomly subjects could not reasonably increase their payoff by acting strategically, knowing how the 'status' was allocated could affect subjects' behavior, since they would no longer attribute the same reverence to the 'status' symbolized by allocated gold stars. Lastly, were subjects to come to expect surprise restarts, it seems reasonable that the subjects would not treat the original rounds as a finite game, but rather as part of the larger game that includes the surprise restarts. Yet if subjects did this, surprise restarts would become a useless methodology (since they are no longer clearly a surprise).

For the APB to apply to explicitly deceptive experiments, subjects need to believe deception occurred in past experiments and expect deception in future experiments, causing them to discount the information provided by the researcher. This is also true for implicitly deceptive experiments. Thus, if the APB justifies banning explicit deception, it just as well justifies banning implicit deception.

5. Conclusion

While the ban on using explicit deception in economic experiments is not as stringent as some believe, explicit deception is perceived by many experimental economists as a highly suspect methodological tool. If using explicit deception by some researchers in experimental economics can have a seriously negative effect on the validity of the results of the field as a whole, there seems to be good reason to ban such use. But then there is also reason to ban the use of implicitly deceptive methods, which economists consider less problematic. Economists who endorse the current practice of only banning explicit deception while allowing the use of implicit deception are being inconsistent.

Defenders of the status quo might accept that consistency requires either removing the ban on explicit deception or placing a ban on implicit deception. Nevertheless, they might argue that a line between allowed and banned deception must be drawn *somewhere*, and the distinction between explicit and implicit deception is as good as any.[20] However, such an argument would require addressing why the possibility of drawing a permissive line–all deception is allowed, or a restrictive line–all deception is banned, are not as good of lines, especially since they have the advantage of consistency. One reason defenders of the status quo might put forth is that the current state of affairs nears optimality from the point of view of the experimental economics field. Currently, some deception is allowed (implicit deception), which they could claim results in more experiments and more scientific discovery than if no deception was allowed. At the same time, too much deception is prevented (explicit deception), which they could claim precludes the reduction of trust by subjects that would bring into question the validity of the field as a whole. I am skeptical that such a fortuitous alignment between a conceptual division (explicit and implicit deception) and a contingent division (too much or too little deception) exists. Nonetheless, such a position acknowledges that the APB also applies to implicit deception, as well as acknowledges that whether the current state of affairs is close to optimal is a contingent matter that is subject to further empirical research.

The fact that the APB applies more widely than has been thought can motivate both those that use it to justify a ban on explicit deception and those that take it to be unconvincing to empirically explore many of its assumptions. Much of the empirical research into the soundness of the APB is still controversial. Some questions for future research on the topic are: Do some research methods actually cause, rather than merely possibly cause, the relevant discounting by subjects? Is discounting of information provided by subsequent researchers limited to the specific methods, or is it exhibited by a wider mistrust of researchers? Does the way subjects are exposed to the deception, either directly during the experiment or during debriefing at the end of the experiment affect the level of discounting? Lastly, what are other methods that are susceptible to the APB?

[20] I thank an anonymous reviewer for asking me to address such a possibility.

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