The Importance of Being Honest?:
Evidence that Deception May Not Pollute Social Science Subject Pools After All

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Abstract

Deceiving participants about the goals or content of a study is permitted in psychological research but is largely banned in economics journals and subject pools. This ban is intended to protect a public good: if experiencing deception causes participants to be suspicious in future studies, and suspicion meaningfully influences their behavior, then the entire field suffers. We report a survey on psychologists and economists' attitudes towards deception (N=568), and a large, non-deceptive multi-site study in which we measured participants' histories, suspicion levels, and behavior in four common economic tasks (N=636). Economists reported more negative attitudes towards deceptive methods and greater support for the deception ban, compared to psychologists. The results of the behavioral study, however, do not support the public goods argument for banning deception about the goals or content of a research study: participants' present suspicion was not clearly related to past experiences of deception, and there were no consistent behavioral differences between suspicious and credulous participants. We discuss the implications of these results for the ongoing debate regarding the acceptability of deceptive research methods.

Keywords: Research methods; Deception; Economics; Psychology

The importance of being honest?:

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Behavioral scientists sometimes mislead their participants about the nature of a study, and then fully debrief them when it is over. Such deception can take many forms: participants might be told they are interacting with other participants but instead are engaging with confederates of the research team or even pre-programmed computer scripts in order to more tightly control participants' experience; the presence of a memory test might be omitted from the study description to allow the researchers to more accurately assess incidental learning; the researchers may obscure their hypotheses to minimize participants' attempts to conform to them; etc. Explicit deception is generally banned, however, in the field of economics (there may be greater tolerance for deception by omission, however, e.g. Colson, Corrigan, Grebitus, Loureiro, & Rousu, 2016; Krawczyk, 2013). While the objections to deception are many and varied, the ban is typically justified on pragmatic, rather than ethical, grounds (e.g. Bröder, 1998; Davis & Holt, 1993; Hertwig & Ortmann, 2001; Wilson, 2014). The chief pragmatic concern is that participants' knowledge that deception may occur will cause their behavior to be driven by suspicion of the methodology, rather than by the incentives the experimenters intend to study (Davis & Holt, 1993). The trust of the subject pool is then treated as a public good: while one experimenter might reap the benefits of deceiving participants, the entire field bears the costs of a more suspicious subject pool (Hertwig & Ortmann, 2001; Ledyard, 1995). A researcher who chooses to use deception in their study design can thus be interpreted as free-riding on the public good of a credulous subject pool. Corroborating evidence from both psychology (Delton, Cosmides, Guemo, Robertson, &

Tooby, 2012; Delton, Nemirow, Robertson, Cimino, & Cosmides, 2013) and economics (Fehr & Gächter, 2000) reveals that regardless of the domain, free-riders are often moralized and deemed worthy of punishment. To protect the trust of the subject pool, economics laboratories and journals generally prohibit deception (Gächter, 2009; Wilson & Isaac, 2007). Moreover, economists have expressed concern that participants who have been deceived in other social science experiments might subsequently become suspicious in economics experiments, despite their own field's explicit ban on deception (Barrera & Simpson, 2012; Davis & Holt, 1993; Ledyard, 1995), which could be used to argue for expanding the ban beyond the field of economics.

In contrast, limited deception has long been used in psychological research (Hertwig & Ortmann, 2008; Nicks, Korn, & Mainieri, 1997). Psychologists (e.g., Bonetti, 1998; Bröder, 1998; Christensen, 1988; Cook & Yamagishi, 2008) justify this on the grounds that (i) it allows the methodological flexibility and precision necessary to achieve certain experimental controls (e.g., controlling for partner behavior or characteristics within social interactions), (ii) it enables researchers to efficiently investigate responses to uncommon social circumstances (e.g., behaviors that are uncommonly generated by real partners in social interactions), (iii) it increases the validity of results by improving experimental realism (i.e., by more closely approximating the real world circumstances under study in ways that could not be done non-deceptively), and (iv) it reduces experimenter demand effects (participants basing their behavior on their impressions of the experimenter's expectations). Note that these justifications for deception are consistent with economists' reasons for banning deception: a goal of both fields is to remove confounding elements from experiments in

order to best measure human behavior. The disagreement is instead over what the best policy is to achieve that goal.

The ongoing debate over this policy difference has resulted in a growing tension between the fields of psychology and economics, at a time when their methods and areas of study are otherwise converging (e.g., Hertwig & Ortmann, 2001). Our goals with the present studies were to 1) measure the current state of opinion in the fields of psychology and economics regarding deception, and 2) provide a novel empirical test of the claims used to argue for banning it. To measure current professional attitudes towards deception, we conducted a survey of economists and psychologists at top colleges and universities within the United States and, as expected, found broad disagreement between the disciplines (Study 1). To test the assumptions motivating the deception ban, we conducted a study in five distinct subject pools using common economic games and measured participants' experiences with and suspicion of being deceived (Study 2).

For the deception ban to be justified as the protection of a public good, two effects should hold (Bonetti, 1998; Hertwig & Ortmann, 2008): First, experience being deceived or in a subject pool in which deception is possible should cause participants to be suspicious in future studies (even studies occurring in subject pools that explicitly ban deception). Second, participants' suspicion that they are being deceived should affect their behavior in ways that matter to researchers. These effects form a causal chain by which exposing participants to deception could harm future scientific study, and therefore could justify banning deception across fields. This result is certainly plausible; for example, if a study involves sharing money with a partner, it is reasonable to expect participants to share less if they doubt their partner is a real person. Or, if a study

attempts to remove participants' reputational concerns by telling them that no one will be aware of their choices, suspicion that others will actually learn of their decisions might cause participants to behave differently than they otherwise would, even if the study is actually completely non-deceptive. However, the proposals that deception causes future participant suspicion and that suspicion alters participants' behavior are empirical claims. If either or both are untrue, then the causal chain falls apart, taking with it the pragmatic justification for banning deception in social science research.

Prior research has produced mixed evidence of the effects of suspicion and prior deception on participant behavior. For example, Stang (1976) showed that participant suspicion reduces conformity in conformity experiments, and that this effect may have undermined conformity research as suspicion in these studies became more common throughout the 1960s and 1970. Two recent meta-analyses report that behavior in multiple paradigms differs between deceptive and non-deceptive variants, such as the rate of lying when lying is incentivized (e.g., in sender-receiver games; Gerlach, Teodorescu, & Hertwig, 2019), and investment by senders in trust games (Johnson & Mislin, 2011). However, these reviews do not directly report an effect of suspicion *per se*, nor whether suspicion is directly linked to a prior history of being deceived. Other studies have shown that prior deception does not affect later behavior in common games with partners, like the prisoner's dilemma, dictator game or trust game (Alberti & Güth, 2013; Barrera & Simpson, 2012; Jamison, Karlan, & Schechter, 2008). One literature review (Bonetti, 1998) drew the conclusion that there was not reliable evidence for effects of suspicion on subsequent behavior (but see McDaniel & Starmer, 1998; Ortmann & Hertwig, 2002 for criticism). A later, systematic review (Hertwig & Ortmann, 2008) found that the effect of

suspicion of deception depends on the concreteness of that suspicion; general knowledge that deception might occur – as may be the case in psychology subject pools – has no consistent effect on behavior, but more concrete knowledge of the likelihood of deception (e.g. a specific prebriefing) may alter behavior in some tasks.

There are multiple routes by which deception and suspicion of being deceived may – or may not – be problematic and thus justify prophylactic measures. Exposure to deception can influence future behavior (i) because participants perceived (correctly or incorrectly) they were deceived, therefore increasing later suspicion; (ii) because participants were debriefed that they were deceived in a study, therefore increasing later suspicion; or (iii) by the general knowledge that participants might be deceived, therefore increasing suspicion. Distinct from these processes, however, suspicion of deception in an experiment may be due to features of the experiment itself that engender suspicion (e.g. instruction phrasing, the physical space used for the experiment, the affect of other participants or the experimenters, etc.), or even participants' individual tendencies toward suspicion in general (e.g. Stang, 1976). Importantly, this is true for both deceptive and nondeceptive studies. These routes to suspicion are also distinct from any differences in participant behavior between deceptive and non-deceptive studies that are due to methodological differences unrelated to participant suspicion. For example, Gerlach et al's 2019 meta-analysis reports a cross-paradigm effect whereby deceptive methods produce modestly different results than non-deceptive methods. However, 31 of the 33 papers reporting deceptive methods analyzed by Gerlach et al. either did not measure participant suspicion at all or did not test and report its effect, while the 2 papers which measured and tested for such effects report a null effect of suspicion on participant

behavior (Gu, Zhong, & Page-Gould, 2013; Kilduff, Galinsky, Gallo, & Reade, 2015). Thus, the difference that emerges in Gerlach et al's analysis may be due to other methodological differences between studies (e.g. differences in participant recruitment, self-selection, or instruction; in interactions between participants and experimenters; in psychological realism; etc.), rather than due to participant suspicion *per se*. These methods effects may be most likely in deceptive studies, but they could occur in non-deceptive studies as well.

Thus, both deceptive and non-deceptive studies may promote participant suspicion, both may employ methods that inadvertently influence behavior in ways unrelated to suspicion, and furthermore general suspicion that deception could occur may have no consistent effect on behavior. Since much of the prior research assumes experience or knowledge of deception as the primary source of suspicion, there is a gap in the literature such that the relationships between past deception (or participation in deception-tolerant pools) and later suspicion, and between suspicion (regardless of its source) and theoretically relevant behavior, are unclear.

Moreover, to the extent that effects of suspicion have been investigated, suspicion has often been operationalized in fairly narrow terms – answering affirmatively to an explicit probe of suspicion. However, some participants may only consider that they might have been deceived when asked such a leading question, while other subjects may have a small degree of suspicion they feel fails to motivate an affirmative answer to an explicit question of whether they believe they have been deceived, complicating the interpretation of responses. A more comprehensive test of whether suspicion affects participant behavior should measure whether any forms of suspicion are impactful,

including spontaneous and unprompted suspicion and varying levels of certainty of deception. Therefore, we set out to test the effects of prior exposure to deception on suspicion, and suspicion's effects (regardless of its source and degree) on behavior in common economic tasks (Study 2). If prior exposure to deception does not increase participant suspicion, or if participant suspicion does not affect behavior, the justification for banning deceptive methods is undercut.

We designed this behavioral study without any deception, and used popular, theoretically-important economic games to be representative of the class of methods often used in experimental economics and economic psychology. We tested participants in five different subject pools: both psychology and economics subject pools at two major universities, and online workers on Amazon Mechanical Turk (an increasingly common participant pool for social science research; Buhrmester, Talaifar, & Gosling, 2018). We measured behavior in the ultimatum game, dictator game, welfare tradeoff allocations, and welfare tradeoff allocation estimates. The ultimatum and dictator games are classics of experimental economics, meant to capture important prosocial and punitive tendencies that matter for the functioning of societies large and small (e.g. Henrich et al., 2005). Welfare tradeoff allocations and their estimates are less commonly measured, but these constructs are hypothesized to underlie a wide range of social decisions (Delton & Robertson, 2016). Importantly, each of these tasks involves considering another person's welfare, outcome, or decision-making such that they are prime examples of the kind of economic tasks that should show differences due to suspicion of deception (e.g., deception about sham partners). In addition to these tasks, participants reported their history of participation in different subject pools, and their suspicion that they were

deceived in the present study. In sum, we designed this study to be maximally likely to detect the effects of past exposure to deception on suspicion, and of suspicion on theoretically-relevant behavior, if such true effects exist.

If the assumptions underlying the deception ban are correct, we should observe two broad effects. First, deception must pollute the subject pool; that is, the experience of past deception, or prior experience in a subject pool that permits deception, should predict participants' suspicion of deception in the present study. Second, this suspicion must have deleterious effects; that is, suspicious participants should behave differently from credulous participants. Both of these are reasonable expectations, but they must both be true to justify banning deception as the protection of a public good. If they are not, then the lack of observable public costs imposed by deceptive methodology undercuts the pragmatic justification for banning it.

Before testing the assumptions underlying the deception ban, we report current professional attitudes towards the use of deception from a survey of economists and psychologists at top colleges and universities within the United States.

Study 1

Method

Subjects. All faculty listed on the psychology and economics department websites of the U.S. News and World Report's 50 Best Liberal Arts Colleges and 50 Best National Universities (2015) were contacted via email, of whom 9.3% responded, yielding a sample of 568 faculty members who completed a voluntary survey on their perceptions of deception in social science. One hundred ninety-four self-identified as

economists, 337 as psychologists, 34 as other social scientists, and 3 declined to answer. Two hundred and twenty five respondents were full professors (86 economists, 122 psychologists, 16 other, and 1 declined to answer), 87 were associate professors (24 economists, 59 psychologists, 4 other), 142 assistant professors (56 economists, 77 psychologists, 9 other), and 114 identified as another professional rank (including emeritus, lecturer, visiting, research scientist, or declined to answer).

Procedure. The survey consisted of 15 multiple choice, Likert, and open-response items which participants completed online. The survey was designed to take approximately 10 minutes to complete. Items asked about participants' opinions of deceptive methodology, beliefs about the prevalence of deceptive methodology, preferred methodology in their own work, and experience as journal reviewers/editors (see SI for complete questionnaire; dataset deposited at the OpenScienceFramework, osf.io/f3kzr). Respondents received no monetary compensation for completing the survey.

After providing informed consent, participants answered a series of questions about their impressions of deceptive research. Participants first indicated what percentage of total published studies in psychology, economics, and general science they believed employed deceptive methods (0% - 100%). They were then asked: "Do you think that studies that use deception are inherently less rigorous than those that are non-deceptive?" and could elaborate in free response. Free responses were coded by two independent coders who rated whether the response contained one or more content types (e.g., that deceiving participants is immoral, harms the future of the field, is ineffective at achieving experimenters' goals, is lazy, etc. See SI for full list of codes and summary statistics).

Participants next answered a series of questions about whether they reviewed for or were editors of psychology, economics and general science journals. They were asked the number of such journals they serve in each category for each role, and to indicate whether they would treat a submission differently if it reported a deceptive method.

The next set of questions asked participants to rate on a seven-point Likert scale how important they felt it was to ban deceptive methods from being used in psychology and economics subject pools, and whether studies that report such methods should be precluded from being published in psychology, economics and general science journals. In addition, they indicated on seven-point Likert scales their belief that banning deceptive methodology was harmful for the fields of psychology, economics, and science in general.

Participants then answered questions about their own research practices including what percentage of their research studies included deceptive methods, and whether they feared that using deceptive methodology would cause their work to be viewed as less rigorous. Finally, the survey asked participants to report their department affiliation (i.e. psychology, economics, or other), their rank, and non-identifying demographic details. No deception was used at any point in this study and the protocol was approved by the Harvard University Institutional Review Board.

Results

We find large field level differences in attitudes towards deception (see also Colson et al., 2016; Krawczyk, 2013). Economists expressed stronger support than psychologists for banning deception in economics subject pools and journals, and

revealed similar opposition to deception in psychology subject pools and journals as well as general science journals (Fig. 1). More economists than psychologists stated that studies using deceptive methods are inherently less rigorous than non-deceptive studies (25.3% vs. 5.3%; Test of independence: $\chi^2(1) = 44.02, p < .001, V = 0.289$), elaborating further in free response (completed by 59.3% of economists and 65.9% of psychologists) that deception harms the future of the field (18% of psychologists that elaborated vs 1.4% of economists that elaborated; Test of independence: $\chi^2(1) = 32.75, p < .001, V = 0.312$), is ineffective at achieving the experimenter's goals (16.5% vs 2.3%; Test of independence: $\chi^2(1) = 23.32, p < .001, V = 0.263$), and is immoral (5.2% vs 1.4%; Test of independence: $\chi^2(1) = 4.36, p = .037, V = 0.114$). Psychologists, instead, were more likely to report in free response that deceptive methods can be as rigorous as non-deceptive methods (36.4% vs 25.2%; Test of independence: $\chi^2(1) = 4.38, p = .036, V = 0.114$), or even more rigorous and sometimes necessary (49.10% vs 20.0%; Test of independence: $\chi^2(1) = 26.92, p < .001, V = 0.283$). While economists viewed banning deception as important across subject pools and journals, psychologists were more likely to view banning deception as harmful to psychology, economics, and science in general (Welch's t 's $> 6.99, p$'s $< .001$; d 's = 0.62 – 1.44; Fig. 1). These results suggest that professional attitudes reflect the norms in each field, though opposition to deception among economists was hardly uniform; for every question except the two regarding the harm of the deception ban to the field of economics and to science generally, economists' ratings were significantly more variable than were psychologists' (Levene's F 's $> 9.04, p$'s $< .003$). These differences may be in part due to, or possibly exacerbated by, differences in perception of the frequency of deceptive methods use. Economists believed

deceptive methods were more commonly used in psychology ($M = 34.3\%$ of studies, $SD = 23.90$) than did psychologists ($M = 25.6\%$ of studies, $SD = 17.12$), $t(512) = 4.77$, $p < .001$, $d = 0.44$. The fields did not differ in their estimates of the use of deceptive methods within economics (Economists: $M = 8.6\%$, $SD = 10.68$; Psychologists: $M = 8.4\%$, $SD = 9.36$; $t(437) = 0.22$, ns).

Figure 1. Ratings of the importance & harm of the deception ban. This figure plots the mean ratings of Psychologists (blue) and Economists (red) on the importance of banning deception in subject pools and journals (left) and the field-wide costs of banning deception (right), with 95% confidence intervals.

Interim Discussion

This survey revealed attitudes towards deception broadly consistent with field-level norms. Compared to psychologists, economists more strongly endorsed the perspective that experimental deception is harmful to their field, is less rigorous, and should be banned from economics, psychology, and general science journals. Psychologists, on the other hand, more strongly endorsed the position that banning deception is itself harmful, and this harm was likewise seen as extending to economics and general science in addition to psychology.

Two limitations of this study must be noted. First, our sample of academic economists and psychologists was self-selected. It is possible that researchers with stronger feelings about deception (both in favor and in opposition) were more likely to complete the survey than their more ambivalent colleagues. Second, we did not define deception for our respondents. There are multiple types of experimental deception (e.g. explicit deception vs. omitting information), and attitudes on the acceptability of deception vary with the type being considered (Krawczyk, 2013). Since our respondents were asked simply about “deception,” they may have had slightly different types of deception in mind when responding.

The attitudes of psychologists and economists are broadly consistent with policies in their respective fields, but this result alone does not provide evidence for or against the use of deception. Therefore, we now turn to Study 2, in which we test the relationship between past experience and present suspicion, and between present suspicion and behavior in common economic tasks.

Study 2

Methods

Subjects. Six hundred and thirty-six people (327 women, 309 men) participated in this study. Participants were drawn from Harvard University’s psychology study pool ($N = 176$) and Decision Science Laboratory (economics) study pool ($N = 116$), the University of California, Santa Barbara’s psychology study pool ($N = 104$) and Experimental and Behavioral Economics Laboratory study pool ($N = 124$), and Amazon Mechanical Turk (AMT; $N = 116$). Our sample included both undergraduates ($N = 370$) and community members ($N = 266$) with participants ranging from 17 – 70 years old (M

= 26.25, $SD = 11.52$) – students and community members were both already members of the Harvard and UCSB psychology and economics subject pools when they were recruited for this study via subject pool emails. Participants received a fixed show-up payment (\$5.00 in lab; \$1.00 on AMT) in addition to variable pay on the basis of their own and other participants' decisions during the experiment and random factors (final payments averaged \$11.30 in lab, ranging from \$6.00 - \$17.50, and averaged \$4.70 on-line, ranging from \$3.30 to \$5.60). This study was completely non-deceptive, and the protocol was approved by the Harvard University Institutional Review Board and the University of California, Santa Barbara Human Subjects Committee.

Procedure.

All laboratory-based experimental sessions had 4, 8, or 12 mixed-sex participants divided into groups of 4. In both the economics and psychology laboratories, participants sat at semiprivate computer terminals; they were not informed which individuals in the session were assigned to their 4-member interaction group. AMT experimental sessions each had 4 participants who interacted in real-time online as in the laboratory-based session, but because AMT participants participated remotely, they never saw the other participants in their group. AMT participants did not interact with any experimenter – all their instructions were written and presented by the computer program. In-lab participants interacted with two experimenters during the study to minimize any effects of expectancy or experimenter demand. Experimenter A gave participants the written consent form, directed participants to the computer laboratory (where participants could choose their own computer terminal), and answered any pre-experiment questions. After the study, Experimenter A directed participants to take the card that was at their respective

computer terminals (labeled with a letter “A” through “L”) to a second room where Experimenter B had envelopes also labeled “A” through “L” which contained each participant’s payment and additional debriefing information. Participants traded their labeled card for the matching envelope. Neither experimenter was aware of which participant was assigned which role in the study and neither experimenter could not see any of the specific decisions that participants made. Experimenters only had access to each participant’s total payout from the computer program. Participants were aware of the experimenters’ inability to see their specific choices and role assignments. During the study, participants engaged in four interactions in fixed order: a welfare tradeoff allocation task, a dictator game, an ultimatum game, and a welfare tradeoff allocation estimation. The within-experiment written instructions explained all four parts of the study before participants started any of the interactions, but participants were not told which role they would have in each interaction until the time of that interaction.

Participants were informed that they would interact with the members of their group in real time, but that they would never interact with the same participant in more than one task. Both lab and AMT sessions were run using the online platform SoPHIE (Software Platform for Human Interaction Experiments; Hendriks, 2012), which enabled a nearly identical experience across the cohorts and no experimenter interaction after the consent process and before the payment/debriefing. In each case, participants waited until each member of the 4-person group logged into an online “waiting room” to begin the first interaction, and then the pace through the study was constrained by the slowest responder within the group. In-lab experimental sessions were advertised as 30 minutes in duration including the consenting, payment and debriefing, and generally took between 15 and 25

minutes. Mturk experimental sessions were advertised as between 15 and 40 minutes in duration (due to the variable time it took for 4 participants to log into the online waiting room) and did not exceed this time.

Welfare Tradeoff Allocation. All participants first completed a welfare tradeoff allocation task (see Delton & Roberston, 2016). After completing practice allocations, participants made 12 forced-choice decisions between allocating a varying amount of money (\$0.05 - \$0.70) for themselves or a constant amount (\$1.00) to an anonymous member of their interaction group. The order of these decisions was randomized. For instance, a participant might first be asked to decide whether they would rather receive \$0.10 or for another participant in their group to receive \$1.00, then whether they would rather receive \$0.50 or the same other participant receive \$1.00, etc. Participants made their choices knowing that only one of their decisions would be randomly selected and paid out (resulting in either a \$0.05- \$0.70 bonus for themselves or a \$1.00 bonus for the anonymous other depending on their choices).

Dictator Game. Participants next completed a Dictator Game (DG). In each four-person interaction group, two participants were assigned the role of “dictator” and two were assigned the role of “recipient.” Dictators could allocate some, none, or all of their endowment [\$5.00 in lab; \$1.00 on AMT] in one cent increments to the anonymous recipient. Recipients had no say over this division and could only passively accept the dictator’s allocation. To avoid using directive language, participants’ materials referred to dictators as “deciders.”

Ultimatum Game. Participants next completed an Ultimatum Game (UG). In each four-person interaction group, the two participants who had been assigned the role of

“recipient” in the DG were assigned the role of “proposer” in the UG. The two participants who had been “dictators” in the DG were assigned the role of “responder” in the UG. In addition to changing roles, the participants who had been paired together in the DG were not paired in the UG. Participants were informed that they were paired with a new partner. Proposers could allocate some, none, or all of their endowment [\$5.00 in lab; \$1.00 on AMT] in one cent increments to the anonymous responder. Responders could then accept or reject the proposer’s offer. If the responder accepted the proposer’s offer, the responder received the amount that the proposer allocated, and the proposer kept the remainder of their endowment. If the responder rejected the proposer’s offer, neither the responder nor proposer received any money from this task.

Welfare Tradeoff Allocation Estimation. In the final task, each participant learned the amount that an anonymous other participant from their interaction group allocated as either a dictator in the DG or as a proposer in the UG. Given this information, participants were asked to guess how that anonymous other participant made each of the 12 tradeoff allocation decisions during the initial tradeoff allocation task. For each correct estimate, participants earned \$0.10. Participants could earn between \$0 and \$1.20 in this task depending on the accuracy of their estimations.

After the four tasks, participants’ bonus pay was calculated and displayed to them on their private computer screen (participants were unaware of other participants’ total payouts). Depending on participants’ choices and random factors, bonus pay ranged from \$0.00 - \$12.90 in lab sessions and \$0.00 - \$4.90 on AMT. Participants completed demographic questions followed by a post-experimental questionnaire that assessed their suspicion of having been deceived in the study. Participants freely responded to prompts

asking whether they had any questions about the study, whether anything about the study seemed unusual, whether they thought there might be more to the study than met the eye, and whether anything about the study had been unclear before answering a forced choice: “Do you think you were intentionally deceived about any part of this study?” Participants then elaborated on their answer by rating how confident they were that they had (or had not) been deceived on a seven-point Likert scale and in an additional free-response item. They then answered a free-response question about whether they believed their suspicion level influenced their behavior (see SI Appendix 2). Participants next answered questions about their prior enrollment in studies in the university’s psychology and economics subject pools (in-lab participants), or past enrollment in studies with deceptive methods (AMT participants). Once all participants in a session had completed the questionnaire, in-lab participants were directed by Experimenter A to take the labeled card at their computer terminal to Experimenter B who gave participants their payment envelope and additional debriefing information. AMT participants were immediately directed to online debriefing information after all participants in their session completed the questionnaire and they were paid their bonus and base pay within 7 days via the AMT interface. See the Open Science Framework for the full dataset (osf.io/f3kzr).

We coded suspicion in four variables. First, ‘spontaneous suspicion’ was coded from the responses to the first four free-response questions of the post experiment questionnaire, which did not explicitly mention deception. Three independent raters coded these responses, with disagreements settled by majority rule. Any participant who was coded as suspicious in at least one of these questions was coded as ‘spontaneously suspicious’, otherwise as ‘not spontaneously suspicious’. ‘Prompted suspicion’ was

coded from the next item, the forced choice question: “Do you think you were intentionally deceived about any part of this study?”. ‘Suspicion confidence’ was measured as the response to the subsequent Likert rating question assessing confidence in the belief the participant was or was not deceived. Finally, we constructed a ‘suspicion type’ variable that coded participants as either ‘spontaneously suspicious’, ‘suspicious only when prompted’, or ‘not suspicious’.

The final free-response question regarding the influence of suspicion on behavior in the task was also coded by the same three independent coders for containing the sentiment ‘my suspicion had no influence on my behavior’, ‘my suspicion made me less generous’, ‘my suspicion made me more generous’, and ‘other, hard to interpret sentiment’.

Results

Does prior deception encourage suspicion? Previous experience being deceived or participating in a deception-tolerant subject pool did not predict suspicion in the current study. We tested this in two ways. First, because AMT is a single pool where some researchers use deceptive methods and some do not, we tested whether individual differences in prior history of being deceived predicted suspicion of being deceived in the present study. About half of AMT participants reported having been deceived about actually interacting with other players “many times” prior to the current study and 80% reported having this experience at least once. Yet, this prior experience of deception did not predict suspicion that they were deceived in this experiment: prior experience being deceived did not predict a greater rate of spontaneous suspicion of deception (‘Never Deceived’ 2/22, ‘Deceived Once’ 6/30, ‘Deceived Many Times’ 12/64; Test of

independence: $\chi^2(2) = 1.29, p = .526, V = .105$), nor a higher rate of suspicion when explicitly prompted ('Never Deceived' 1/22, 'Deceived Once' 6/30, 'Deceived Many Times' 7/64; Test of independence: $\chi^2(2) = 3.03, p = .220, V = .162$). Prior experience did not change relative rate of these types of suspicion (Test of independence: $\chi^2(4) = 3.40, p = .494, V = .121$). Finally, prior experience being deceived did not lead to greater confidence in being deceived in the current study ($M_{\text{Never}} = 2.95, SD = 1.56, M_{\text{Once}} = 3.00, SD = 1.39, M_{\text{Many}} = 3.31, SD = 1.45, F(2,113) = 0.75, p = .473, \eta^2 = .013$).

Second, we tested whether suspicion rates differ across subject pools as would be expected if experience in deception-tolerant subject pools (e.g. psychology pools, AMT) fostered suspicion and banning deception (e.g. economics pools) protected against suspicion. Figure 2a-c presents the rates of prompted and spontaneous suspicion in each of the five pools. There was no pool difference in the rate of spontaneous suspicion, $\chi^2(4) = 5.27, p = .261, V = .091$ (Figure 2a). Suspicion when prompted (regardless of spontaneous responses) did, however, differ across the pools, $\chi^2(4) = 26.96, p < .001, V = .206$ (Figure 2b). Prompted suspicion was higher in the psychology pools than the economics pools ($\chi^2(1) = 5.36, p = .021, V = .102$), and higher in the economics pools than AMT, $\chi^2(1) = 11.18, p = .001, V = .177$ (Figure 2b). Confidence in being deceived also differed among the pools ($F(4,631) = 4.53, p = .001, \eta^2 = .028$), with Tukey's post-hoc comparisons revealing two significant differences: Suspicion confidence was higher in the Harvard Psychology pool ($M = 3.82, SD = 1.59$) than both the Harvard Economics Pool ($M = 3.21, SD = 1.72$) and AMT ($M = 3.16, SD = 1.45$), p 's $< .05$, but no other pairs significantly differed (Figure 2d).

These pool-level effects do not show a clear positive relationship between the permission of deception and participant suspicion. When prompted, psychology participants were more suspicious than economics participants, who were more suspicious in turn than AMT participants. Note that the majority of participants who expressed suspicion when prompted did not do so spontaneously (see Fig. 2c), suggesting that prompted reports of suspicion may be, in part, the result of a demand characteristic: participants may be more likely to suspect deception when researchers have suggested to them that they ought to consider whether they have been deceived. Furthermore, there were no pool effects on spontaneous suspicion. The pool effects on prompted suspicion may be the result of pool-level policies on participants' expectations of being deceived (though this would predict effects on spontaneous suspicion as well), differential self-selection into these subject pools (e.g. Frank, Gilovich, & Regan, 1993; Vedel, 2016), differential sensitivity to the demand characteristics of being asked explicitly about deception, or the effect of that demand differing based on the physical proximity of the experimenter (e.g., either nearby as in the lab or remote as on MTurk). That over a quarter of economics participants were suspicious when asked despite explicit pool-level instruction that deception is banned, and the fact that AMT participants are commonly deceived but were the cohort least likely to express suspicion when prompted, argues against the hypothesis that banning deception maintains the naivety of the subject pool.

Figure 2. Pool differences in suspicion of deception. A) Rates of spontaneous suspicion coded from non-leading free response text. B) Rates of prompted suspicion in response to an explicit forced-choice question. C) Combined suspicion codes depicting all spontaneous suspicion, suspicion only after prompting, and the remaining non-suspicious participants. Participants who expressed both spontaneous and prompted suspicion are categorized as spontaneously suspicious. D) Confidence in belief about being deceived or not deceived (whiskers depict 95% confidence intervals).

Does suspicion affect participant behavior? Each game behavior was modelled with a set of 5 stepwise regressions (Tables 1-5). At the first step of each regression, dummy variables coding pool were added to test and control for pool differences in each behavior. Models 2-4 individually added each of the three suspicion measures (spontaneous suspicion, prompted suspicion and suspicion confidence) one at a time. Model 5 adds the three suspicion measures simultaneously. Only one suspicion measure for one behavior yielded a significant effect: Participants who were more confident that they were deceived gave less in the dictator game, $b = -2.97$, $SE = 1.35$, $R^2_{change} = .02$, $F(1,310) = 4.85$, $p = .028$. In no other case did any suspicion measure predict behaviors, R^2_{change} 's $< .01$, p 's $> .05$. As these models do not control experiment-wise alpha, it is reasonable to suspect that this single significant result (out of 15 tests in Models 2-4) may be a false-positive. To test this question more holistically across the full dataset, we proceeded with a within-study meta-analytic approach. For each of the 3 suspicion measures we calculated the effect size of suspicion for each game behavior in each pool, for every aggregation by pool and game behavior, and then in total for the full dataset. Figure 3 plots the effect of prompted suspicion (in terms of Cohen's d) across tasks, sites, and subject pools, clearly revealing that suspicion has no consistent effects on behavior. While two of 25 such comparisons exceed test-wise alpha for the equivalent t test, they do so in opposite directions, and every aggregation of these results by pool or measure suggests the underlying effect of suspicion is non-existent (aggregations by pool and in total based on subject-wise means of z-scored game behaviors).

Figure 3. Forest plot of the effect of prompted suspicion of deception. This figure plots the standardized mean difference (d) between suspicious and credulous participants (with 95% confidence intervals), separated by pool and site, for each behavior measured (DG: Dictator game gift proportion plotted as circles; UG-offer: Ultimatum game offer proportion plotted as closed squares; UG-resp: Ultimatum game acceptance rate plotted as open squares; WTR: Welfare tradeoff task ratios plotted as triangles; eWTR: Estimations of other player's welfare tradeoff task ratio plotted as diamonds; Psychology pool participants plotted in blue; Economics pool participants plotted in red; Amazon Mechanical Turk participants plotted in green; Cross-pool subtotals and totals plotted in black). Positive values indicate credulous > suspicious.

We highlight here the results investigating the effect of prompted suspicion because it more closely matches how many prior studies operationalize suspicion. Further, because more participants expressed suspicion when prompted, these tests are higher-powered. However, we also completed the same analysis for spontaneous suspicion and suspicion confidence, and those results (Figures S1 & S2) provide convergent evidence that the true effect of suspicion of deception is likely null.

Note that we have moderate power to detect even small effects of suspicion here. Our full sample provides greater than 63% power to detect a small effect of $d = .2$, 80%

power to detect an effect of size $d = .24$, and greater than 99% power to detect a medium effect of $d = .5$ (power calculated ex post for a two-tailed t-test, Cohen, 1988).

To assess the evidential weight that should be given to this null result, we calculated Bayes factors for each game behavior measure and the subject-wise aggregate (i.e. the first five subtotals and the Total in Figure 3) testing a ‘null’ hypothesis that the true effect of suspicion is negligible ($|d| < 0.10$) against the alternative that the true effect is larger than that (Morey & Rouder, 2011). These Bayes factors can be interpreted as a ratio of the support in these data for the null versus alternative hypotheses and are independent of any individual’s prior beliefs about these hypotheses. In all cases these Bayes factors were found to be moderate to large: DG = 14.47, UG-prop = 7.65, UG-resp = 14.57, WTR = 30.73, eWTR = 31.11, Total = 30.57. That is, for the aggregate (Total) scores, these data support the null hypothesis of negligible differences over 30 times more than they do the alternative hypothesis of substantial differences. This result is robust to the precise size of the interval of negligible differences; we find across a wide range of such intervals ($|d| < 0.01$ to $|d| < 0.20$), and across a wide range of prior beliefs, the evidence supports posterior preference of the null hypothesis (Figure 4 presents results for prompted suspicion; see SI for additional details and convergent analyses for the other suspicion measures.) Therefore, the present results provide strong evidence against the existence of even a very small effect of suspicion on behavior in the tasks used.

Figure 4. Posterior probability of the null hypothesis as a function of priors and the weight of the evidence. This figure plots posterior probabilities of the null hypothesis that suspicion has a negligible effect on behavior (with various effect sizes as the cutoff for a negligible effect) versus the alternative hypothesis that suspicion does effect behavior, as a function of prior probability and the weight of the evidence (the Bayes factor). Figure based on the subject-wise aggregation of the effect of prompted suspicion (bottom ‘TOTAL’ row of Figure 3). As can be seen, even if one thinks it is likely that prompted suspicion has an appreciable effect – that is, has a low prior belief in the null hypothesis – the weight of the present evidence argues strongly that suspicion that has no appreciable effect on behavior.

Interestingly, participants themselves expect this null effect. In free response to a probe asking how they believed their suspicion of being deceived influenced their behavior, 76.1% of participants thought it had no influence, 4.9% reported they thought they were more generous as a result, and 8.3% reported they thought they were less

generous as a result (the remaining 10.7% of responses were not clearly interpretable); these beliefs did not differ between Psychology, Economics and AMT pools (Test of independence: $\chi^2(6) = 8.59, p = .198, V = .082$).

Pool effects on behavior. In contrast to suspicion measures, behavior in all five tasks did differ by pool (Tables 1-5). For example, compared to participants in the economics pools, participants in the psychology pools were more generous in the dictator game (29.85% vs. 15.74%; $b = 14.11, SE = 4.73, t(311) = 2.98, p = .003$) but AMT participants were not (19.31%; $b = 3.57, SE = 6.04, t(311) = 0.592, p = .554$; Table 1). Participants in the psychology pools also offered more in the ultimatum game (45.05%) than those in the economics pools (40.29%; $b = 4.76, SE = 2.28, t(317) = 2.084, p = .038$) but AMT participants again did not (40.69%, $b = 0.40, SE = 2.94, t(317) = 0.136, p = .892$; Table 2). Participants in the psychology pools were more generous in the welfare tradeoff task ($M = 0.39$) than were participants in the economics pools ($M = 0.32$; $b = 0.07, SE = 0.02, t(633) = 3.196, p = .001$), while AMT participants were the least generous ($M = 0.26$; $b = -0.06, SE = 0.03, t(633) = 2.290, p = .022$; Table 4). Participants in the psychology pools also assumed their partners were more generous ($M = 0.30$ in the welfare tradeoff estimation task) than did those in the economics pools ($M = 0.26$; $b = 0.04, SE = 0.02, t(633) = 2.406, p = .016$), while AMT participants assumed the least generosity ($M = 0.20$; $b = -0.06, SE = 0.02, t(633) = 2.475, p = .014$; Table 5). The effect of pool on behavior may be due to differential self-selection into these pools (Frank et al., 1993; Vedel, 2016), or other structural differences (e.g. while student members of the psychology pools often complete studies for course credit, student and community members of the economics pools and AMT only complete studies for pay). These pool

effects on behavior show that this study was indeed capable of revealing differences, further strengthening our confidence in the null effects of suspicion.

Discussion

The results of our survey of researchers (Study 1) suggest that attitudes towards banning deception vary as expected by field, with economists supporting banning deceiving study participants more so than psychologists, but the results of our behavioral study (Study 2) do not support the public goods argument for banning deception. Recall that the logic of banning deception to preserve a public good rests on the assumptions that experience in a subject pool which permits deception will foster participant suspicion, and that suspicion will affect participant behavior in future studies. Here, we employed three complementary measures of suspicion (a spontaneous measure, a forced-choice response, and a measure of the strength of participants' suspicion), and tested effects of suspicion (regardless of their source) on behavior in four common, theoretically important economic tasks in five different subject pools. We found that experience in a pool which permits deception (or in deceptive experiments) did not reliably lead to greater suspicion, and that suspicion was surprisingly widespread in economics subject pools despite the deception ban. Furthermore, suspicious participants did not behave differently from credulous participants. In other words, we did not find evidence for the effectiveness or necessity of banning deceptive research methodologies.

Of course, the present null results only extend to the measures included. We deliberately used common partner-based experimental economics tasks because suspicion of the use of sham partners (a common form of deception) may be expected to affect behavior in precisely these tasks (e.g. Jamison et al., 2008), making them representative

of the types of tasks behavioral economists use in which a detrimental effect of suspicion may be expected. We found no effect of suspicion in these tasks, but our data cannot speak to the effect suspicion of deception may have in other tasks. For example, psychological realism is an important consideration in experimental design, and to the extent that suspicion of deception undermines psychological realism, it may indeed affect behavior. Future research may continue to test different possible pathways by which suspicion may affect behavior in different possible domains.

A second limitation of our behavioral study concerns self-selection into the subject pools we used. Our AMT and economics pools consisted entirely of self-selected participants, and there is evidence that the experience of deception affects the choice to participate in future research (e.g. Jamison et al., 2008). Therefore, the null effects of suspicion observed here may be due in part to the persistence of participants who are either less prone to or less affected by suspicion, compared to participants who left those subject pools. The convergent results from the psychology subject pools suggest that self-selection is unlikely to have had a major effect on the results, however, since psychology subject pools (including those used here) generally compel undergraduates to participate in studies in exchange for course credit (though note that all participants in Study 2 received pay, and that the Harvard psychology subject pool contains some self-selected community members in addition to students). Therefore, we expect self-selection in response to previous experiences of deception to have had less of an effect on the results from psychology subject pools, which were broadly consistent with the results from economics subject pools and AMT.

Conclusion

The increasing unification of the social sciences and advent of powerful shared research platforms like Amazon Mechanical Turk benefit the entire scientific community. However, these developments may create friction between fields (e.g. Hertwig & Ortmann, 2001), and suggest the need for evidence-based policies to govern social science research as a whole. Fortunately, there is a growing body of research from both economics and psychology assessing the effects of deceptive methodologies on the research enterprise (e.g. Barrera & Simpson, 2012; Bonetti, 1998; Bröder, 1998; Christensen, 1988; Cook & Yamagishi, 2008; Epley & Huff, 1998; Gerlach et al., 2019; Hertwig & Ortmann, 2001, 2008; Jamison et al., 2008; Krawczyk, 2013, 2015; Ortmann & Hertwig, 2002; Stang, 1976). Across five subject pools and four common tasks, we did not find evidence of increased suspicion among participants previously exposed to deceptive pools or studies, and behavior did not significantly differ between suspicious and credulous participants, undercutting the pragmatic logic of the deception ban. If participant suspicion is not primarily dependent on prior exposure to deception, and suspicion does not reliably influence participant behavior, it may be that the benefits of the judicious use of deception could outweigh its costs (see e.g. Bonetti, 1998; Bröder, 1998; Cook & Yamagishi, 2008; Christensen, 1988). Although the economists we surveyed had more negative views of deception than psychologists and were more likely to endorse banning deceptive methodologies across fields, we also found variance and ambivalence in their attitudes (Figure 1; see also Colson et al., 2016; Krawczyk, 2013), suggesting that field-wide policies may evolve as more relevant evidence – such as the results presented here – accumulates. We hope that further interdisciplinary research will

continue to reveal what role, if any, different types of deception should have in the social science toolbox.

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Author Contributions: M.M. Krasnow conceived of the experimental study and designed it with R.M. Howard. R.M. Howard wrote code for data acquisition. R.M. Howard collected data at Harvard University and on Amazon Mechanical Turk; A.B. Eisenbruch collected data at University of California, Santa Barbara. R.M. Howard and M.M. Krasnow conceived and designed the survey study. M.M. Krasnow and A.B. Eisenbruch conducted data analysis. All the authors wrote the manuscript and approved of the final version for publication.

Open Practices Statement: The data for the experiments reported here is available online (osf.io/f3kzr), the materials are available by request to the corresponding author, and none of the experiments were preregistered.

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Table 1. Dictator giving (proportion of endowment) as a function of Pool & Suspicion (Hierarchical Linear Regression). Model 1 tests pool differences relative to the Economics pools. Models 2-4 test the effect of individually adding each measure of suspicion of deception. Model 5 adds all three suspicion measures simultaneously. Bold type indicates significant predictors, $p < .05$.

Predictor	Model 1			Model 2			Model 3			Model 4			Model 5		
	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β
<u>Pool</u>															
Constant (Econ)	15.74	3.45		15.90	3.49		16.47	3.69		25.55	5.62		27.67	5.97	
Psych	14.11	4.73	0.18	14.20	4.75	0.18	14.41	4.76	0.19	15.86	4.77	0.21	15.83	4.78	0.21
AMT	3.57	6.04	0.04	3.71	6.07	0.04	3.13	6.10	0.03	3.07	6.01	0.03	3.95	6.11	0.04
<u>Suspicion Type</u>															
Spontaneous				-2.17	6.98	-0.02							-0.01	7.30	0.00
Prompted							-2.75	4.85	-0.03				6.64	6.37	0.08
Confidence										-2.97	1.35	-0.13	-4.14	1.75	-0.17
	$R^2 = 0.03$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} = 0.02$			$\Delta M1 R^2_{change} = 0.02$		
	F(2,311) = 4.71, $p = .010$			F(1,310) = 0.10, $p = .756$			F(1,310) = 0.32, ns			F(1,310) = 4.85, $p = .028$			F(3,308) = 1.99, $p = .115$		

Table 2. Ultimatum proposal (proportion of endowment) as a function of Pool & Suspicion (Hierarchical Linear Regression). Model 1 tests pool differences relative to the Economics pools. Models 2-4 test the effect of individually adding each measure of suspicion of deception. Model 5 adds all three suspicion measures simultaneously. Bold type indicates significant predictors, $p < .05$.

Predictor	Model 1			Model 2			Model 3			Model 4			Model 5		
	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β
<u>Pool</u>															
Constant (Econ)	40.29	1.68		40.29	1.72		39.43	1.81		38.48	2.74		38.66	2.85	
Psych	4.76	2.28	0.13	4.76	2.29	0.13	4.64	2.29	0.13	4.66	2.29	0.13	4.63	2.30	0.13
AMT	0.40	2.94	0.01	0.40	2.96	0.01	0.64	2.97	0.01	0.56	2.95	0.01	0.73	3.01	0.02
<u>Suspicion Type</u>															
Spontaneous				0.03	3.07	0.00							-0.74	3.23	-0.01
Prompted							1.55	2.29	0.04				0.79	2.91	0.02
Confidence										0.52	0.62	0.05	0.42	0.77	0.04
	$R^2 = 0.02$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$		
	$F(2,317) = 2.50, p = .083$			$F(1,316) < 0.01, ns$			$F(1,316) = 0.46, ns$			$F(1,316) = 0.70, ns$			$F(3,314) = 0.27, ns$		

Table 3. Ultimatum acceptance (rate) as a function of Pool & Suspicion (Hierarchical Linear Regression). Model 1 tests pool differences relative to the Economics pools. Models 2-4 test the effect of individually adding each measure of suspicion of deception. Model 5 adds all three suspicion measures simultaneously. Bold type indicates significant predictors, $p < .05$.

Predictor	Model 1			Model 2			Model 3			Model 4			Model 5		
	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β
<u>Pool</u>															
Constant (Econ)	0.85	0.03		0.85	0.03		0.85	0.03		0.88	0.05		0.89	0.05	
Psych	0.08	0.04	0.12	0.08	0.04	0.12	0.08	0.04	0.12	0.08	0.04	0.13	0.08	0.04	0.13
AMT	0.03	0.05	0.04	0.03	0.05	0.04	0.03	0.05	0.03	0.03	0.05	0.03	0.03	0.05	0.04
<u>Suspicion Type</u>															
Spontaneous				0.02	0.06	0.02							0.03	0.06	0.03
Prompted							-0.01	0.04	-0.02				0.01	0.05	0.02
Confidence										-0.01	0.01	-0.05	-0.01	0.02	-0.07
	$R^2 = 0.01$ $F(2,311) = 1.92, p = .15$			$\Delta M1 R^2_{change} < .01$ $F(1,310) = 0.08, ns$			$\Delta M1 R^2_{change} < .01$ $F(1,310) = 0.09, ns$			$\Delta M1 R^2_{change} < .01$ $F(1,310) = 0.70, ns$			$\Delta M1 R^2_{change} < .01$ $F(3,308) = 0.35, ns$		

Table 4. Welfare tradeoff allocation as a function of Pool & Suspicion (Hierarchical Linear Regression). Model 1 tests pool differences relative to the Economics pools. Models 2-4 test the effect of individually adding each measure of suspicion of deception. Model 5 adds all three suspicion measures simultaneously. Bold type indicates significant predictors, $p < .05$.

Predictor	Model 1			Model 2			Model 3			Model 4			Model 5		
	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β
<u>Pool</u>															
Constant (Econ)	0.32	0.02		0.32	0.02		0.33	0.02		0.35	0.03		0.35	0.03	
Psych	0.07	0.02	0.14	0.07	0.02	0.14	0.07	0.02	0.14	0.07	0.02	0.14	0.07	0.02	0.14
AMT	-0.06	0.03	-0.10	-0.07	0.03	-0.10	-0.07	0.03	-0.11	-0.07	0.03	-0.10	-0.07	0.03	-0.11
<u>Suspicion Type</u>															
Spontaneous				0.03	0.03	0.05							0.05	0.03	0.07
Prompted							-0.03	0.02	-0.06				-0.03	0.03	-0.05
Confidence										-0.01	0.01	-0.06	-0.01	0.01	-0.04
	$R^2 = 0.04$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$		
	F(2,633) = 13.09, $p < .001$			F(1,632) = 1.33, $p = .250$			F(1,632) = 1.90, $p = .168$			F(1,632) = 2.16, $p = .143$			F(3,630) = 1.80, $p = .146$		

Table 5. Welfare tradeoff estimation as a function of Pool & Suspicion (Hierarchical Linear Regression). Model 1 tests pool differences relative to the Economics pools. Models 2-4 test the effect of individually adding each measure of suspicion of deception. Model 5 adds all three suspicion measures simultaneously. Bold type indicates significant predictors, $p < .05$.

Predictor	Model 1			Model 2			Model 3			Model 4			Model 5		
	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β	B	SE B	β
<u>Pool</u>															
Constant (Econ)	0.26	0.01		0.26	0.01		0.27	0.01		0.29	0.02		0.29	0.02	
Psych	0.04	0.02	0.10	0.04	0.02	0.10	0.05	0.02	0.11	0.05	0.02	0.11	0.05	0.02	0.11
AMT	-0.06	0.02	-0.11	-0.06	0.02	-0.11	-0.06	0.02	-0.11	-0.06	0.02	-0.11	-0.07	0.02	-0.12
<u>Suspicion Type</u>															
Spontaneous				0.04	0.03	0.06							0.05	0.03	0.08
Prompted							-0.02	0.02	-0.04				-0.01	0.02	-0.03
Confidence										-0.01	0.01	-0.06	-0.01	0.01	-0.06
	$R^2 = 0.03$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$			$\Delta M1 R^2_{change} < .01$		
	F(2,633) = 10.25, $p < .001$			F(1,632) = 2.20, $p = .138$			F(1,632) = 1.06, $p = .304$			F(1,632) = 2.26, $p = .134$			F(3,630) = 2.02, $p = .110$		