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**Working Paper**

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# One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects

ZACHARIAS MANIADIS, FABIO TUFANO AND JOHN A. LIST[1]

*Some researchers have argued that anchoring in economic valuations casts doubt on the assumption of consistent and stable preferences. We present new evidence that questions the robustness of certain anchoring results. We then present a theoretical framework that provides insights into why we should be cautious of initial empirical findings in general. The model importantly highlights that the rate of false positives depends not only on the observed significance level, but also on statistical power, research priors, and the number of scholars exploring the question. Importantly, a few independent replications dramatically increase the chances that a given original finding is true.*

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## I. Introduction

Much of modern economics is predicated on the notion of durable and meaningful consumer preferences. In an influential study, Ariely, Loewenstein, and Prelec (2003) (ALP hereafter), however, report that people's preferences are characterized by a very large degree of arbitrariness. In particular, they provide evidence that subjects' preferences for an array of goods and hedonic experiences are strongly affected by normatively irrelevant cues, namely anchors.[2] Importantly, the ALP results suggest that arbitrariness of preferences is extremely strong, even in situations that we would expect traditional economic theory to have descriptive power.

Summing up the implications of their results, ALP argue (p. 102) that: "These results challenge the central premise of welfare economics that choices reveal true preferences (...). It is hard to make sense of our results without drawing a distinction between 'revealed' and 'true' preferences". The broader literature has followed, as these results have been received as strong evidence against traditional normative economics. If economic preferences are unstable and subject to the vagaries of the environment, then even the simplest choices may not be traced back to any optimization principles. In this case, a reevaluation of the fundamental building blocks of utility theory is warranted.

Our study begins by revisiting the seminal ALP results. In doing so, we present new experimental evidence that questions the robustness of the original results. In particular, we find an effect size that is about half of what ALP found (the effect size is roughly zero before excluding outliers following ALP's methodology), and across several outcome measures we find no significant differences between treatments. In a review of relevant studies, we show that

[2] Decades of controlled experiments have also provided evidence that, in certain contexts, people might not always have predefined preferences, but "construct" them, when facing a choice problem (e.g., Lichtenstein and Slovic, 2006, for a summary of the accumulated evidence).

independent experiments by different researchers also find weaker anchoring effects than ALP. The evidence thus points to more modest effects than reported in the original study.

Importantly, however, one must recognize that many novel and surprising experimental results are likely not robust—not because of falsification or something egregious, but merely due to the mechanics of the problem**.** The second contribution of our study is to illustrate this basic point by means of a theoretical framework that provides insights into the mechanics of proper inference. The model highlights that we should be cautious when interpreting new experimental findings. For example, we show that the common benchmark of simply evaluating *p*-values when determining whether a result is a true association is flawed.

The common reliance on statistical significance as the sole criterion leads to an excessive number of false positives. In this sense, our theoretical model suggests that many surprising new empirical results are likely not recovering true associations. Our framework highlights that, at least in principle, the decision about whether to call a finding noteworthy, or deserving of great attention, should be based on the estimated probability that the finding represents a true association, which follows directly from the observed *p*-value, the power of the design, the prior probability of the hypothesis, and the tolerance for false positives.

Beyond providing a paradigm in which to view new empirical results, our model indicates that we need a new approach for deciding which findings to highlight among the set of results from an empirical exercise. Since new and surprising results many times spur research meant to extend the original analysis, publishing false positives may have a costly effect in terms of misallocated resources. For the economics profession, the stakes are important because after the publication of such results it is possible to make the logical error that if the

conventional economic model is rejected, then theory based on psychology is necessarily correct. In this way, entirely new research efforts may commence based on false insights.

The remainder of the paper is organized as follows. Section II describes how canonical anchoring experiments are conducted, briefly reports on ALP's empirical evidence, and then presents our study alongside other studies that have attempted to replicate ALP. Section III introduces the basic framework of analysis, which provides insights into the mechanics of inference and the issue of false positives. Section IV discusses the relevance of the framework and then concludes.

## II. The ALP Investigation and the New Evidence

Consider how a typical "anchoring" experiment is conducted. Subjects enter the experimental laboratory and, before starting the experimental task, they are exposed to a salient, but irrelevant, number. For example, a subject is asked to take the last two digits of her social security number and to turn those numbers into a dollar value (i.e., if your numbers are 12 then you provide a value of $12). Then, she is asked whether she would buy a certain item for the dollar value thus formed. Subsequently, the subject is asked the maximum amount of money she would pay for a certain item, commonly called "willingness to pay" (WTP).[3]

### *A. The Original Investigation*

ALP's first experiment was conducted in a classroom, and the items involved were six common market goods: a cordless trackball, a cordless keyboard, a bottle of average wine, a bottle of rare wine, a design book, and a pack of Belgian

[3] Similarly, when the decision involves selling, rather than buying an item, or when the item is a 'bad', willingness to accept (WTA) is elicited.

chocolates. The other four experiments were conducted in a laboratory, and the relevant items were different durations of a high-pitched noise, which subjects heard through their headphones.

For illustration and quantitative comparison purposes, we summarize ALP's results in Table 1.[4] In Table 1 all numbers are denominated in US dollars. We present ALP's Experiments 1-5, in the order in which they were presented in their paper, in rows 1 to 5. As can be seen, the smallest effects are in the order of 50 percent, and the largest are approximately 200 percent.

TABLE 1—THE ANCHORING EFFECTS IN ALP

| Number of study | Type of study | Anchor | | Results | | Effect | N |
|---|---|---|---|---|---|---|---|
| | | Low | High | Low | High | (%) | |
| 1 | WTP, goods | 0-49 | 50-99 | 14.237 | 25.017 | 76 | 55 |
| 2 | WTA, sounds | 0.10 | 0.50 | 0.398 | 0.596 | 50 | 132 |
| 3 | WTA, sounds | 0-4.9 | 5.0-9.9 | 3.550 | 5.760 | 62 | 90 |
| 4 | WTA, sounds | 0.10 | 1.00 | 0.430 | 1.300 | 202 | 53 |
| 5 | WTA, sounds | 0.10 | 0.90 | 0.335 | 0.728 | 117 | 44 |

*Notes:* The amounts in the "Anchor" columns denote the size (or range) of the anchor price in the low and high treatment, in each study. In the "Results" columns, the amounts represent the average WTP or WTA (depending on the study) in each of the two treatments. "Effect" denotes the effect size, or the percentage change in the average outcome due to the treatment. In the last column, "N" denotes the sample size of the given study. Study 5 involves multiple anchors: a different one in each round. Thus, we report the results from the first round, where subjects have been exposed to a unique anchor, which is the case which is comparable with all the other studies reported here.

[4] In Experiments 1 and 3 the anchor was a random price between 0 and 99 dollars (or between 0 and 9.9 dollars). We follow the natural convention of considering as a "low" anchor one that belongs to the lower half of this support. Moreover, in Experiment 4, the authors report the mean WTA for each of the three durations of the sound, and we have taken the average of the three means.

ALP interpret these data as importantly refuting the foundations of economics. The literature has broadly concurred:[5] Fehr and Hoff (2011) interpret the ALP results as "striking" evidence that preferences are reference-dependent, and suggest that a person might have multiple preference orderings, depending on the "social identity" invoked at the moment of a choice. For Kahneman and Sugden (2005), these results imply that "people can be unsure what their preferences are", and they argue that stated WTP and WTA should not be used in policy valuation. For Beshears et al. (2008), the ALP results reflect the powerful influence of third-party manipulation on consumer choices, which casts doubt on whether these choices represent "normative preferences." Likewise, Bernheim and Rangel (2007, 2009) use the results as motivating evidence for proposing modifications of the traditional, revealed-preference based welfare analysis.

### *B. The Replication Study*

As Levitt and List (2009) discuss, there are at least three levels at which replication can take place. The first of these entails re-analyzing the original data generated by an experiment in order to corroborate the results. A second conception of replication refers to implementing an experiment under a similar protocol to the original experiment to verify whether similar findings can be obtained using different subjects. The third notion of replication (the most general one) pertains to the employment of a new research design with the purpose of testing the hypotheses of the first study.[6] Our primary focus here and in the theoretical model below is on the second notion of replication. Yet, our fundamental points apply equally to the third replication concept.

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[5] All of the following authors base their arguments on a large set of evidence, and not only on ALP's results. However, as we shall argue, the ALP results are singularly important because they provide extremely strong evidence in favor of arbitrary preferences, in some of the most favorable environments conceivable for traditional economic theory.

[6] This taxonomy is in agreement with the one proposed by Cartwright (1991). Hunter (2001) also defines three levels of replication, but the first level he suggests concerns the exact repetition of the original study in all dimensions, rather than the routine checking of the original study. His other two levels are largely equivalent with ours.

As Table 1 shows, ALP's main body of evidence concerns the "annoying sounds" treatment.[7] As ALP argue, these hedonic goods are particularly appropriate for testing economic valuation, since they involve a very simple experience, a sample of which can be readily provided without satiating the subjects. Moreover, a market price does not exist, and neither do outside-the-lab substitutes. For these reasons, it is possible that the large effects found in ALP, for the simple hedonic experiences, represent the "true" effects of anchoring, net of all possible distorting factors.

In order to examine this hypothesis, we replicated Experiment 2 of ALP as closely as possible. Our experiment took place at the BLESS (Bologna Laboratory for Experiments in Social Science) lab of the University of Bologna (Italy). It consisted of six experimental sessions programmed and conducted in a computerized environment using z-Tree (Fischbacher, 2007). A total of 116 subjects, recruited and randomly invited through ORSEE (Greiner, 2004), attended our experiment. Participants were students of the University of Bologna drawn from a range of academic disciplines.[8] The original instructions, as well as an English translation, can be found in the Appendix.

In each session, subjects entered the lab and were asked to put on their headphones, and to keep them on for the duration of the experiment. Then, they listened to a sample of 30 seconds of the annoying sound, which was the same as ALP's sound. The anchoring question followed: each subject was asked whether she/he would be willing to repeat the same experience for a given amount of

[7] ALP's cleverly designed study included six experiments. The five experiments that we are presenting in Table 1, and an additional one, that did not involve WTA or WTP in monetary units, and therefore is not comparable with the other studies. This experiment showed that anchoring matters even when people express their preferences directly in terms of substituting one hedonic experience for another, and not only when they are substituting one hedonic experience for money.

[8] 18 participants (i.e., 15.52 percent of our sample) had attended at least one different experiment before taking part in our own (14 out of those 18 subjects had participated in only one experiment). Our sample featured a narrow majority of men (56.90 percent), while consisting almost exclusively of Italian nationals (95.69 percent). Subjects received a show-up fee of 5 euros, plus their earnings from the experiment. Average payoffs per subject were equal to 7.65 euros, including the show-up fee.

money (the anchor).[9] Subsequently, subjects participated in nine experimental rounds. In each round, subjects were asked to state the minimum amount of money (i.e., WTA) for which they would be willing to hear the same sound, with certain duration.

Then, the Becker-DeGroot-Marschak (1964) mechanism was implemented to ensure incentive compatibility.[10] As in ALP, we varied the anchoring manipulation and the sequence in which the sound durations appeared. The anchoring manipulation involved either an anchor of 10 cents, of 50 cents, or no anchor at all. We crossed these three treatments with two sequences: an increasing sequence and a decreasing one.[11] In the increasing (resp. decreasing) sequence, the first round had a sound of 10 (resp. 60) seconds, the second of 30 (resp. 30) seconds, and the third of 60 (resp. 10) seconds. This triplet was repeated three times, for a total of nine rounds.

The results are summarized in Figure 1.[12] For the increasing-sequence (resp. decreasing-sequence) treatment, the average stated WTA in the 10-cent anchor condition, the no-anchor condition and the 50-cent anchor condition was 23.05 (resp. 16.50), 25.16 (resp. 20.37) and 28.79 (resp. 21.61), respectively. For both sequences pooled, the average stated WTA was equal to 19.60, 22.76 and 25.20, respectively. Using the pooled data from the two sequences, and comparing only the 10-cent and the 50-cent anchor conditions, we find an effect size equal to 28.57 percent, about half of what ALP found. The p-value of the two-sided t-test

[9] As in ALP, in our experiment the anchoring question was not incentivized.

[10] In particular, in each round a random price was drawn by the computer. The number was drawn from a triangular distribution with mode zero, and maximum 100 cents. Subjects were shown a picture of this distribution. If their stated WTA was lower than this price, they would receive the computer's random price, and listen to the sound. If their stated WTA exceeded this price, they would neither receive any money, nor listen to the sound. Subjects were told that this process ensured that it was in their best interest to state their true minimum for listening to the sound.

[11] All six experimental sessions had 20 subjects, except the 10-cent anchor condition in the increasing-sequence treatment and the 10-cent anchor condition in the decreasing-sequence treatment that had 17 and 19 participants, respectively (due to subjects that did not show up).

[12] The scales of the axes were chosen purposely to increase visual comparability with ALP's Figure I (p. 83).

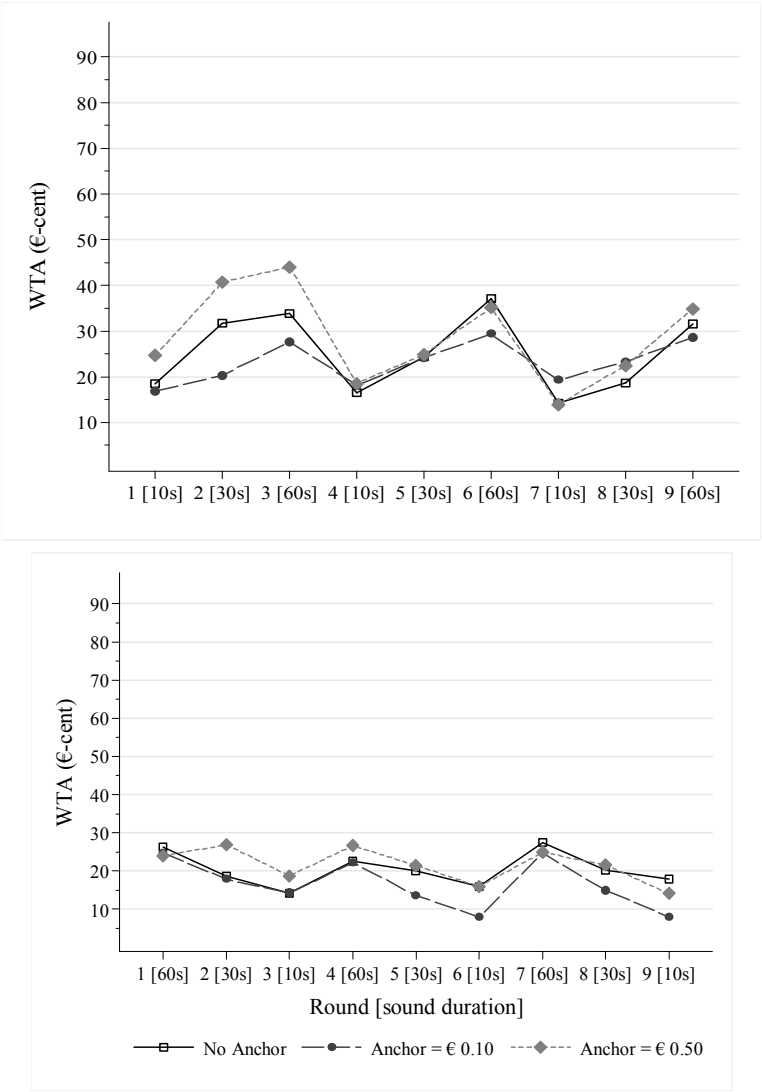


FIGURE 1. RESULTS FROM THE INCREASING (*top panel*) AND DECREASING (*bottom panel*) SEQUENCE

*Notes:* The vertical axis measures the average WTA across subjects in each round.

for differences in the average WTA of each subject, across the 10-cent and 50-cent anchor treatments, was equal to 0.253.[13]

[13] The p-value of the two-sided t-test for the 10-cent condition versus the no-anchor condition is equal to 0.431. The p-value of the two-sided t-test for the 50-cent condition versus the no-anchor condition is equal to 0.610. It is important to note that although we use t-tests for comparability with ALP, using the nonparametric Wilcoxon rank-sum test gives the same results. In particular, no difference is significant even at the 10% level. Moreover, it should be emphasized that following ALP, for the purpose of statistical analysis we truncated responses greater than 100 cents to 101 cents. Using the

Moreover, the average payoffs per subject did not differ significantly in the 10-cent anchor condition (2.70 euros) from the 50-cent anchor condition (2.62 euros) [p = 0.746, two-sided t-test]. A similar non-significant difference appears in the average number of listened sounds per subject [6.750 in the 10-cent treatment versus 6.275 in the 50-cent treatment, p = 0.392, two-sided t-test]. Our experiment thus points to considerably lower effects than the original study.

There are several factors that might account for the fact that our experiment found different results than ALP. Our subjects were students enrolled at the University of Bologna, while ALP had MIT undergraduates. There was more than a decade of difference between the two studies. Moreover, the z-Tree experimental interface is different than the one ALP used (which unfortunately was not available to us). Furthermore, it is possible that our results stem from a very unlucky draw, and are not representative of the true underlying phenomenon.

### *C. More recent evidence*

All of the aforementioned are valid possibilities, so it is important to examine whether we can draw on additional evidence. Several studies, including some very recent ones, have explored the robustness of the original ALP results. In the informal review that follows we include all studies that we are aware of, which satisfy the following criteria: they are published after ALP, they concern either standard consumption goods or hedonic experiences very similar to the ones used by ALP, and their structure allows direct comparisons to their study.

We summarize them in Table 2. In the first two rows, we present the results of a clever study by Simonson and Drolet (2004), who performed a series of experiments with purely hypothetical decisions. In their first treatment they elicited the WTP and WTA for four products: a toaster, a cordless phone, a

non-truncated data, the average WTA in the 10-cent anchor condition is greater than in the 50-cent anchor condition (27.92 vs. 27.81).

backpack, and headphone radio. In the table we report the empirical results (the average of the median WTP and WTA for the four products) of their Study 1, which was directly comparable with ALP.[14] As can be seen, this study found moderately large anchoring effects for WTP, but no effects for WTA.[15] In rows 3-6 we report the results of Fudenberg et al. (2012) and Bergman et al. (2010), who attempted to replicate experiment 1 of ALP in two careful sets of experiments. They used similar goods to ALP and a similar incentive structure.[16] Fudenberg et al. elicited both WTP and WTA for the goods, and Bergman et al. (2010) elicited WTP only. Fudenberg et al. found very weak or no effects in all their treatments, while Bergman et al. (2010) found positive, but weaker effects than ALP.

Likewise, Alevy et al. (2011) performed a field experiment, where subjects were recruited at a market for sport memorabilia. They used a jar of salted peanuts and a protocol similar to ALP. Their results (row 7) suggest that consumers in markets do not have the anchoring tendencies observed in ALP.[17]

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[14] Simonson and Drolet (2004) also had an additional treatment where they asked subjects to explain how they would form their reservation prices, and other treatments where they asked subjects to imagine that they have already made the choice to sell the item, etc. On average, these treatments found small anchoring effects, but we do not consider them directly comparable to ALP.

[15] In a private communication, Aimee Drolet kindly noted that the follow-up studies to Study 1 and parts of their Study 2 included conditions which were the same as SD1 and SD2. Regarding Study 2, the relevant treatments reveal an effect size of -1% for WTA (N=83) and 52% for WTP (N=29). The data we received from Drolet regarding the relevant follow-up studies to Study 1 reveal that for WTP the effect size was 65% (N=75) and for WTA it was -14% (N=26). Therefore, these sessions gave very similar results to SD1 and SD2. Drolet also noted that the results from their paper might not be fully comparable with ALP for several reasons. In particular, they did not use the BDM mechanism or any other monetary incentive, they collected the data in large survey runs, they did not show the physical products (but pictures), and the goods were 'utilitarian', rather than 'hedonic'. She also highlighted that, in consumer research, multiple replications - such as those reported in this footnote - serve as a means of reducing the noise in this style of survey evidence. Despite these reservations, we included the study in the analysis as we had done from the beginning in order to restrict our own "degrees of freedom" (see 'research bias' section below).

[16] It should be noted that exact replication is difficult, since the ALP experiments were conducted after a class, which introduced the relevant concepts, and it is not clear what was explained during the class. In two of their three treatments, Fudenberg et al. (2010) adjusted for this by explaining why the Becker-Degroot-Marschak (Becker et al., 1964) mechanism is incentive compatible. In their third treatment, Fudenberg et al. (2010) did not provide this explanation. Bergman et al. (2010) did not provide any additional explanations of the Becker-Degroot-Marschak mechanism. Moreover, Fudenberg et al. (2010) could not use bottles of wine, and they used an academic agenda/planner and a financial calculator instead.

[17] Alevy et al. (2011) also performed the same treatment for a pack of sports cards, for which no clear market price exists. They found an effect size of -8%, but we think that this type of good goes beyond the spirit of "common market good" and we did not include this result in the table.

Finally, Tufano (2010) explored the robustness of anchoring for hedonic experiences, rather than goods, by using a binary-choice elicitation procedure. In particular, he examined whether WTA to drink a bad-tasting liquid (similar to the one used by ALP, in a treatment not reported here) was sensitive to an anchor. As the results in row 8 show, the anchoring manipulation had no effect.

TABLE 2—THE ANCHORING EFFECTS IN RECENT SIMILAR STUDIES

| Authors* | Type of Study | Anchor | | Results | | Effect | N |
|---|---|---|---|---|---|---|---|
| | | Low | High | Low | High | (%) | |
| SD1 | WTA, goods | 0-49 | 50-99 | 35 | 32.5 | -7 | 59 |
| SD2 | WTP, goods | 0-49 | 50-99 | 25 | 38 | 52 | 59 |
| FLM1 | WTP, goods | 0-49 | 50-99 | 12.43 | 12.76 | 3 | 78 |
| FLM2 | WTA, goods | 0-49 | 50-99 | 21.65 | 20.93 | -3 | 79 |
| FLM3 | WTA, goods | 0-49 | 50-99 | 17.88 | 20.5 | 15 | 79 |
| BEJS | WTP, goods | 0-49 | 50-99 | 50 | 73 | 45 | 116 |
| ALL | WTA, goods | 0-4.9 | 5.0-9.9 | 4.46 | 4.99 | 12 | 121 |
| Tufano | WTA, liquid | 0.05 | 1.25 | 1.33 | 1.39 | 4 | 116 |
| MTL | WTA, sounds | 0.10 | 0.50 | 0.196 | 0.252 | 28 | 76 |

*SD: Simonson and Drolet (2004); FLM: Fudenberg et al. (2012); BEJS: Bergman et al. (2010); ALL: Alevy et al. (2011); Tufano: Tufano (2010); MTL: This paper.

*Notes:* In Simonson and Drolet's (2004) study, the sample size is only a rough estimation (and rounded up), because the authors only report that there were 468 subjects randomly assigned to 8 conditions, two of which we are reporting here.

By combining our work with the aforementioned complementary evidence, we believe that the ALP results greatly overestimated anchoring effects.[18] In summary, the picture that emerges from the totality of the empirical evidence is

[18] Our approach is to consider treatment effects as our focus, not statistical significance. This is in the spirit of meta-analysis. Dan Ariely and George Loewenstein noted that anchoring effects have been found also for works of art, housing prices and judicial compensation decisions (Beggs and Graddy, 2009; Simonsohn and Loewenstein, 2006; Sunstein et al., 2002). These results are of independent importance, and they concern highly complex goods, for which standard utility theory might arguably not have high explanatory power. We still believe that quantifying the average anchoring effects in the class of experiments that closely follow ALP, using the totality of the relevant evidence, is important for determining the economic significance of anchoring. In fact, we agree that if the ALP treatment effects are representative of the average effects for simple consumer goods and hedonic experiences, a radical reevaluation of consumer theory might be in order.

that anchoring effects in economic valuations are real, since the effects are typically positive, but the magnitude of the effect is economically small. The claim that traditional economic models need radical revision seems premature based on this evidence alone. We would welcome more research.

## III. Accounting for Replication Failure

Is such non-replication to be expected? Viewed through the lens of a simple theoretical framework, we show that by their very nature, studies that report strong and highly surprising findings are most likely not revealing true associations — not due to researcher malfeasance, rather because of the underlying mechanics of the methods. Such a model, which we describe now, also provides insights into factors that exacerbate or attenuate such effects.[19]

### *A. The Basic Framework*

Building on a formal methodology developed in the health sciences literature (Wacholder et al., 2004; Ioannidis, 2005; Moonesinghe et al., 2007), we let $n$ be the number of scientific associations that are being examined in a specific research field. Let $\pi$ represent the fraction of these that are true associations. We use $\alpha$ as the typical significance level in the field (usually $\alpha = 0.05$) and $1 - \beta$ denotes the typical power of an experimental design in this field.[20] We are interested in the probability that a declaration of a research finding, made upon reaching statistical significance, is true. We denote this as the Post-Study Probability (*PSP*).

[19] An astute reviewer raised other issues that should be of concern to experimentalists, including i) representativeness of the population (most studies use undergraduate students as subjects and generalize to the population of interest) and ii) multiple testing. The interested reader should see Levitt and List (2007) for a recent discussion of i) and Romano and Wolf (2005) and Romano et al. (2008) for good discussions of the latter.

[20] For simplicity, we assume that the research practices in the field are relatively homogeneous and, therefore, the choices of sample size can be captured by this single level of power. It is straightforward to notice that the arguments of our model can be made on the basis of a single study (so $n$ plays only an expositional role). Therefore, this assumption is innocuous.

This probability depends on the mechanics of statistical inference and can be found as follows: of the $n$ associations, $\pi \cdot n$ associations will be true, and (1-$\pi$) $\cdot n$ will be false. Among the true ones, (1-$\beta$) $\cdot \pi \cdot n$ will be declared true, and among the false associations, $\alpha(1-\pi) \cdot n$ will be declared true even though they are false (i.e., they are false positives). The *PSP* is equal to the number of true associations which are declared true divided by the number of all associations which are declared true:

$$PSP = \frac{(1-\beta)\pi}{(1-\beta)\pi + \alpha(1-\pi)}. \quad (1)$$

Of first note is that the *PSP* from Equation 1 is increasing in the prior $\pi$: the higher the priors about the existence of a phenomenon the weaker the evidence that is needed to substantiate it. With respect to the role of sample size, the derivative of *PSP* from Equation 1 with respect to power $(1-\beta)$ is positive. This means that *PSP* is a positive function of sample size via the power of the experimental study.[21]

### *B. Researchers' Competition*

Now assume that there are $k$ independent researchers working simultaneously on each of $n$ associations in a specific field.[22] Let each researcher's study have the same power $(1-\beta)$. The probability that at least one of the $k$ researchers will declare a true association as true is $(1-\beta^k)$. Likewise, the probability that a false relationship is declared true by at least one of $k$ researchers is $1-(1-\alpha)^k$. Accordingly, out of the $\pi \cdot n$ true relationships, $(1-\beta^k) \cdot \pi \cdot n$ will be declared true. And, of the $(1-\pi) \cdot n$ false relationships, $[1-(1-\alpha)^k](1-\pi) \cdot n$ will

[21] For more details about this relationship, see Maniadis et al. (2013).

[22] Note that we use the term 'researcher' referring indifferently to both a single researcher and a research team.

be declared (mistakenly) true, or will be false positives. Hence, the *PSP* in the presence of competition by independent researchers ($PSP^{Comp}$) is equal to:

$$PSP^{Comp} = \frac{(1-\beta^k)\pi}{(1-\beta^k)\pi + [1-(1-\alpha)^k](1-\pi)} \quad . \tag{2}$$

This is decreasing in $k$ as long as $(1-\beta) > \alpha$. Since power is typically greater than the significance level, Equation 2 reveals that as the number of investigators examining a typical phenomenon increases (competition intensifies), the probability that an *initial* declared research finding is true decreases.[23]

### *C. Research Bias*

The aforementioned analysis implicitly assumed that the degrees of freedom in doing research do not play a role. As the recent paper by Simmons et al. (2011) emphasizes, there is by now a large literature that shows the existence of self-serving biases in interpreting ambiguous evidence and reaching defensible conclusions that satisfy the research objectives (Babcock and Loewenstein, 1997; Dawson et al., 2002).[24]

We define the 'bias' $u$ as "any combination of various design, data, analysis, and presentation factors that tend to produce research findings when they should not be produced" (Ioannidis, 2005, p. 697).[25] In particular, the parameter $u$

[23] Of course, it is also the case that competition will tend to increase the number of replications, so its effect in the medium term could be to increase the average reliability of research findings – under the assumption of no editorial reluctance in publishing replication studies. Here we focus on proper interpretation of initial findings.

[24] For a related discussion, we direct the interested reader to Dufwenberg (2011), who discusses the biased nature of the publication process toward accepting studies that report surprising results and proposes an innovative solution.

[25] Notice that the bias, as defined above, differs from chance variability, which can lead to false positive findings, even though a study was correctly conducted in each and every of its aspects. Also note that our concept of bias here does not refer to biased beliefs regarding the experimental results (perhaps due to ideology). The bias that we refer to is purely behavioral, and concerns how the study is conducted. It differs from the bias in beliefs in non-trivial ways: for example, it could depend – perhaps subconsciously – on the incentives for publication, while the bias in beliefs should not depend on it. It is worth exploring how the other type of bias (in beliefs) operates: for example, it is possible that having opposing biases in beliefs could be quite helpful, in the sense that they increase replications that falsify initial findings. It might also

denotes the fraction of all cases where a positive research finding has been declared because of the bias, although it should not have been declared. Recall that if $n$ relationships are tested in the field, $\pi \cdot n$ will be true and $(1-\pi) \cdot n$ will be false. Of the true relationships, $(1-\beta) \cdot \pi \cdot n$ will be declared true, and $\beta \cdot \pi \cdot n$ will be declared false due to pure noise.

When in addition to noise there is research bias, a fraction $u$ of the latter will be declared true because of the bias, so that $(1-\beta) \cdot \pi \cdot n + u \cdot \beta \cdot \pi \cdot n$ will be declared true. Using analogous reasoning, one can verify that out of the false associations, $\alpha \cdot (1-\pi) \cdot n + u \cdot (1-\alpha) \cdot (1-\pi) \cdot n$ will be declared true. In this case, *PSP* with bias ($PSP^{\text{Bias}}$) is equal to:

$$PSP^{Bias} = \frac{(1-\beta)\pi + \beta\pi u}{(1-\beta)\pi + \beta\pi u + [\alpha + (1-\alpha)u](1-\pi)} \quad . \tag{3}$$

The derivative of $PSP^{Bias}$ with respect to $u$ is negative when $\pi(1-\pi)[\alpha + \beta - 1]$ is smaller than zero which implies that $\alpha < 1-\beta$, which, as we argued, is typically true.[26] Therefore, as Equation 3 shows, an increase in research bias decreases the probability that a published research finding corresponds to the truth.[27]

lead to non-trivial results if a refereeing process with biased beliefs is introduced. However, these analyses fall outside the scope of this paper.

[26] A reviewer correctly noticed that it is possible that the bias could operate in an asymmetric manner. Our general result – that PSP declines in the presence of the bias – is robust even to the case where the bias operates much more on the non-significant true associations than on the non-significant false ones. However, the decline in the PSP would not be as large as in the symmetric case, depending on the level of the asymmetry.

[27] In principle, the behavioral bias could exist in both directions, in the sense that a researcher might prefer to fail to get an effect, so that the net effect of behavioral biases might not be obvious. However, we believe that complicating the model in order to capture opposing biases would not change the direction of the effect. The reason is that there is a fundamental asymmetry in exploratory research: it is much more common that an experimentalist would like to discover an effect, rather than to reveal its absence. Second and third generation studies, of course, do not have the same properties.

### *D. Discussion*

How can our model help us to determine whether some given initial findings, such as these of ALP, are indeed true effects? For this, we need to specify a prior $\pi$, the power of the design $(1-\beta)$, the number of independent researchers $k$, and then use Equation 2 above to calculate the *PSP*. Since it is difficult to pinpoint these variables exactly, in a thought experiment we consider various combinations of the variables to provide meaningful ranges.

TABLE 3—THE PSP ESTIMATES AS A FUNCTION OF PRIOR PROBABILITY($\pi$), POWER, AND COMPETITION ($k$) OF THE STUDY

| | Power=0.80 | | | | Power=0.50 | | | | Power=0.20 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| π | k=1 | k=5 | k=15 | k=50 | k=1 | k=5 | k=15 | k=50 | k=1 | k=5 | k=15 | k=50 |
| | PSP | | | | | | | | | | | |
| 0.01 | **0.14** | **0.04** | **0.02** | **0.01** | **0.09** | **0.04** | **0.02** | **0.01** | **0.04** | **0.03** | **0.02** | **0.01** |
| 0.02 | **0.25** | **0.08** | **0.04** | **0.02** | **0.17** | **0.08** | **0.04** | **0.02** | **0.08** | **0.06** | **0.04** | **0.02** |
| 0.05 | **0.46** | **0.19** | **0.09** | **0.05** | **0.34** | **0.18** | **0.09** | **0.05** | **0.17** | **0.14** | **0.09** | **0.05** |
| 0.10 | 0.64 | **0.33** | **0.17** | **0.11** | 0.53 | **0.32** | **0.17** | **0.11** | **0.31** | **0.25** | **0.17** | **0.11** |
| 0.20 | 0.80 | 0.52 | **0.32** | **0.21** | 0.71 | 0.52 | **0.32** | **0.21** | 0.50 | **0.43** | **0.31** | **0.21** |
| 0.35 | 0.90 | 0.70 | 0.50 | **0.37** | 0.84 | 0.70 | 0.50 | **0.37** | 0.68 | 0.62 | **0.49** | **0.37** |
| 0.55 | 0.95 | 0.84 | 0.69 | 0.57 | 0.92 | 0.84 | 0.69 | 0.57 | 0.83 | 0.78 | 0.69 | 0.57 |

Table 3 presents such combinations, and the resulting *PSP*.[28] Similarly, in Table 4 we specify $u$, the research specific bias, leaving aside the number of competing researchers, and use Equation 3 to calculate the relevant *PSP*. Tables 3 and 4 convey a strong message: we should be very careful not to make strong inference from a first, surprising, research finding. Tables 3 and 4 also indicate that it is not unlikely that the *PSP* after the initial study is less than 0.5, as several plausible parameter combinations yield this result (presented by bold fonts). In addition, one important

[28] Note that for all our tables we assume that $\alpha = 0.05$.

feature left on the sidelines in this analysis is the possible interaction between competition and bias that may lead to interpreting the above estimates as an upper bound. Summing up, there are several factors that might lead to false positives, and many of them stem from the incentives of the current academic system (see Oswald, 2007; Glaeser, 2008; Young et al., 2008).

TABLE 4—THE PSP ESTIMATES AS A FUNCTION OF PRIOR PROBABILITY ($\pi$), POWER AND BIAS ($u$) OF THE STUDY

| | Power=0.80 | | | | Power=0.50 | | | | Power=0.20 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| π | u=0 | u=0.10 | u=0.25 | u=0.50 | u=0 | u=0.10 | u=0.25 | u=0.50 | u=0 | u=0.10 | u=0.25 | u=0.50 |
| | PSP | | | | | | | | | | | |
| 0.01 | **0.14** | **0.05** | **0.03** | **0.02** | **0.09** | **0.04** | **0.02** | **0.01** | **0.04** | **0.02** | **0.01** | **0.01** |
| 0.02 | **0.25** | **0.10** | **0.06** | **0.03** | **0.17** | **0.07** | **0.04** | **0.03** | **0.08** | **0.04** | **0.03** | **0.02** |
| 0.05 | **0.46** | **0.23** | **0.13** | **0.08** | **0.34** | **0.17** | **0.10** | **0.07** | **0.17** | **0.09** | **0.07** | **0.06** |
| 0.10 | 0.64 | **0.39** | **0.25** | **0.16** | 0.53 | **0.30** | **0.19** | **0.14** | **0.31** | **0.18** | **0.13** | **0.11** |
| 0.20 | 0.80 | 0.59 | **0.43** | **0.30** | 0.71 | **0.49** | **0.35** | **0.26** | 0.50 | **0.33** | **0.26** | **0.22** |
| 0.35 | 0.90 | 0.75 | 0.61 | **0.48** | 0.84 | 0.67 | 0.54 | **0.43** | 0.68 | 0.51 | **0.43** | **0.38** |
| 0.55 | 0.95 | 0.87 | 0.78 | 0.68 | 0.92 | 0.83 | 0.73 | 0.64 | 0.83 | 0.70 | 0.63 | 0.58 |

One solution to the inference problem is replications. Our framework suggests that a little replication can lead to far reaching benefits. To illustrate, we consider several specifications of our model, each with a different number of competing researchers $k$ (see also Maniadis et al., 2013, Moonesinghe et al., 2007). Then, we calculate the probability that anywhere from zero (only the original study) to three replication studies (so a total of four studies) find a significant result, given that the relationship is true and given that it is false. Then, we derive the *PSP* the usual way, as the fraction of the true associations over all associations for each level of replication. The results reported in Table 5 (for the case of $k = 10$) show that with just two independent replications of the initial finding, the improvement in *PSP* is dramatic. Indeed, for studies that report 'surprising' results—those that

have low $\pi$ values—the *PSP* increases more than threefold upon a couple of replications.

TABLE 5—THE PSP ESTIMATES AS A FUNCTION OF PRIOR PROBABILITY ($\pi$), POWER AND NUMBER OF REPLICATIONS ($i$)

| | Power = 0.80 | | | | Power = 0.50 | | | | Power = 0.20 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| $\pi$ | i=0 | i=1 | i=2 | i=3 | i=0 | i=1 | i=2 | i=3 | i=0 | i=1 | i=2 | i=3 |
| | PSP | | | | | | | | | | | |
| 0.01 | **0.02** | **0.10** | **0.47** | 0.91 | **0.02** | **0.10** | **0.45** | 0.89 | **0.02** | **0.07** | **0.22** | 0.54 |
| 0.02 | **0.05** | **0.19** | 0.64 | 0.95 | **0.05** | **0.19** | 0.63 | 0.94 | **0.04** | **0.13** | **0.36** | 0.71 |
| 0.05 | **0.12** | **0.38** | 0.82 | 0.98 | **0.12** | **0.38** | 0.81 | 0.98 | **0.10** | **0.28** | 0.60 | 0.86 |
| 0.10 | **0.22** | 0.56 | 0.91 | 0.99 | **0.22** | 0.56 | 0.90 | 0.99 | **0.20** | **0.45** | 0.76 | 0.93 |
| 0.20 | **0.38** | 0.74 | 0.96 | 1.00 | **0.38** | 0.74 | 0.95 | 1.00 | **0.36** | 0.64 | 0.88 | 0.97 |
| 0.35 | 0.57 | 0.86 | 0.98 | 1.00 | 0.57 | 0.86 | 0.98 | 1.00 | 0.55 | 0.80 | 0.94 | 0.98 |
| 0.55 | 0.75 | 0.93 | 0.99 | 1.00 | 0.75 | 0.93 | 0.99 | 1.00 | 0.73 | 0.90 | 0.97 | 0.99 |

## IV. Conclusions

Economists and policymakers rely on utilitarian analysis as a crucial step in guiding policymaking. Within the US alone, every economically significant[29] proposed rulemaking must undergo a formal benefit cost analysis. In many cases, the economic analysis critically relies on empirical measures derived from experimental or survey methods. The stakes are heightened further when such empirical methods are also used to test the foundations of theoretical models; in those cases where extant theory is rejected, profound paradigmatic changes can ensue. In both instances, if the original empirical findings are untrue, the social cost can be quite high.

[29] Based on the historical standard introduced as part of President Reagan's Executive Order 12291 and maintained currently under Executive Order 13563, an "economically significant" policy is that which has an annual effect on the U.S. economy of $100 million or more, or adversely impacts the economy, a sector of the economy, productivity, public health, the environment or a host of other relevant facets of the U.S. economy.

Such considerations are especially important in light of recent findings due to ALP. In this study we provide new experimental evidence that suggests more modest effects of anchoring on economic valuations. We proceed to provide a theoretical basis showing why such non-replication should be expected. Combined, our theory and empirical work highlights that we should be cautious when interpreting new empirical findings. For example, in the model we show that the common benchmark of simply evaluating *p*-values when determining whether a result is a true association is flawed. Two other considerations—the statistical power of the test and the fraction of tested hypotheses that are true associations—are key factors to consider when making appropriate inference. The common reliance on statistical significance as the sole criterion leads to an excessive number of false positives. The problem is exacerbated as journals make 'surprise' or counter-intuitive results necessary for publication. But, by their very nature such studies are most likely not revealing true associations—not because of researcher malfeasance, merely because of the underlying mechanics of the methods.

While this message is pessimistic, there is good news: our analysis shows that a few independent replications dramatically increase the chances that the original finding is true. As Fisher (1935) emphasized, a cornerstone of the experimental science is replication. Inference from empirical exercises can advance considerably if scholars begin to adopt concrete requirements to enhance the replicability of results, as for instance starting to actively encourage replications within a given study.[30]

[30] We do not regard internal replication as a sufficient requirement to establish the robustness of the original results. However, we envisage it as a first step in the right direction (see also Simmons et al., 2011).

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## APPENDIX: EXPERIMENTAL INSTRUCTIONS

### I. Oral Instructions

### ENGLISH TRANSLATION

Thank you for coming. Please turn off all your electronic devices, mobile phones included. It is required that you do not talk, or with any other way try to communicate during this study. If you have questions, please, raise your hand and an assistant will help you privately.

This study will take place via computer. Each workstation is made up by a laptop and a pair of headphones. During this study, the mouse should be used for choosing and for reaching any field where data are imputed. The mouse works with the touchpad and the related buttons. In order to input data, it will be necessary to use the computer keyboard. Furthermore, we demand exceptional care in your movements given that your monitor is a touchscreen and therefore you may make some choices unintentionally.

On your workstations you will find a booklet reproducing the content of the main screenshots: You can refer to it whenever you wish so, even though to properly participate in the study it is sufficient to pay attention to the information that will appear on your computer screen.

Are there any questions? We are almost ready to start: Please, put on your headphones and keep them on throughout the study. Thank you.

**ORIGINAL IN ITALIAN**

Grazie per essere venuti. Cortesemente, spegnete ogni vostro dispositivo elettronico, telefoni cellulari inclusi. Durante questo studio, non vi è consentito parlare o comunicare in ogni altro modo. Se avete domande, per cortesia, alzate la mano e un assistente vi aiuterà personalmente.

Lo studio avrà luogo tramite computer. Ogni postazione è composta da un computer portatile e da cuffie audio. Nel corso dello studio, per compiere delle scelte o per raggiungere dei campi in cui immettere i dati, sarà necessario l'utilizzo del mouse, per mezzo del touchpad e dei relativi tasti; mentre per immettere i dati, sarà necessario usare la tastiera del computer. Vi chiediamo inoltre massima accortezza nei vostri movimenti siccome lo schermo è sensibile al tocco e, pertanto, potreste inavvertitamente operare delle scelte.

Inoltre, sulle vostre postazioni trovate un documento che riporta il contenuto delle principali schermate: potete consultarlo quando lo desiderate, ma per partecipare correttamente allo studio è sufficiente prestare la dovuta attenzione alle informazioni che compariranno sullo schermo del vostro computer.

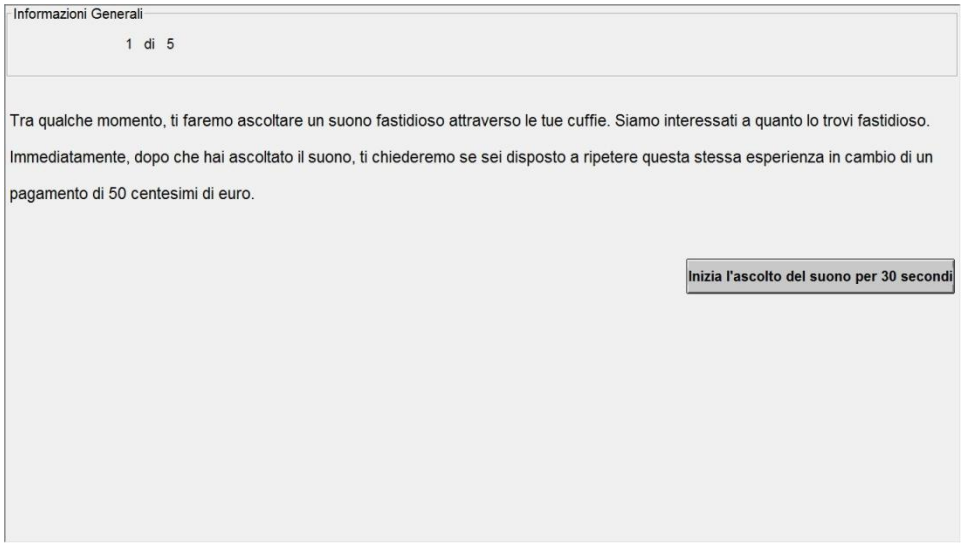
Ci sono domande?

Siamo quasi pronti ad iniziare: per cortesia, indossate le cuffie e non rimuovetele prima che lo studio sia terminato. Grazie.

## II. Computerized Instructions

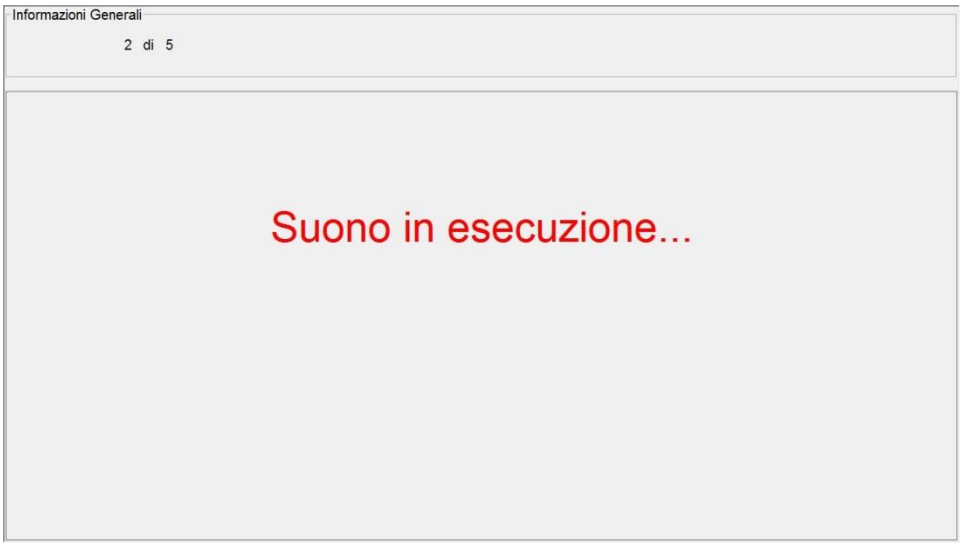
The screenshots taken from the z-Tree code used in our experimental sections are reproduced below (together with the English translation).

SCREENSHOT 1



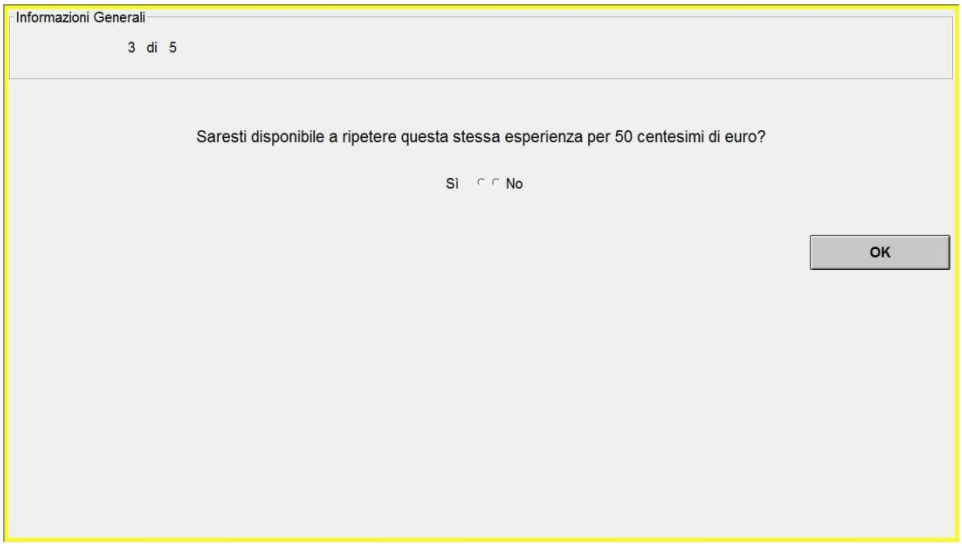

*English translation (top-down)*: General Information || 1 out of 5 || In a few moments we are going to make you listen to an unpleasant tone over your headset. We are interested in how annoying you find it to be. Immediately after you hear the tone, we are going to ask you whether you would be willing to repeat the same experience in exchange for a payment of 50 €-cents. || Start listening to the 30-second sound

SCREENSHOT 2



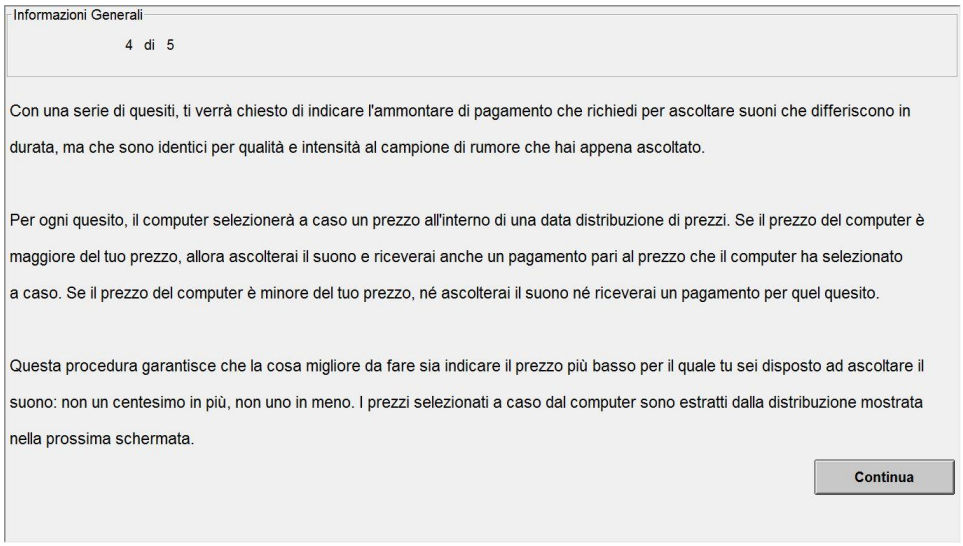

*English translation (top-down)*: General Information || 2 out of 5 || Playing the sound... [Red blinking text]

SCREENSHOT 3



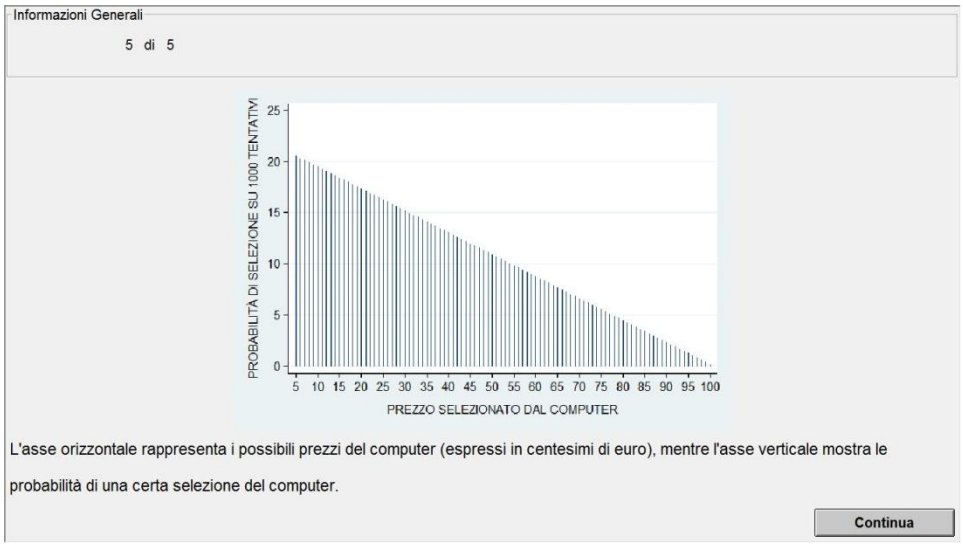

*English translation (top-down)*: General Information || 3 out of 5 || Would you be willing to repeat the same experience for 50 €-cents? || Yes _ _ No || OK

SCREENSHOT 4



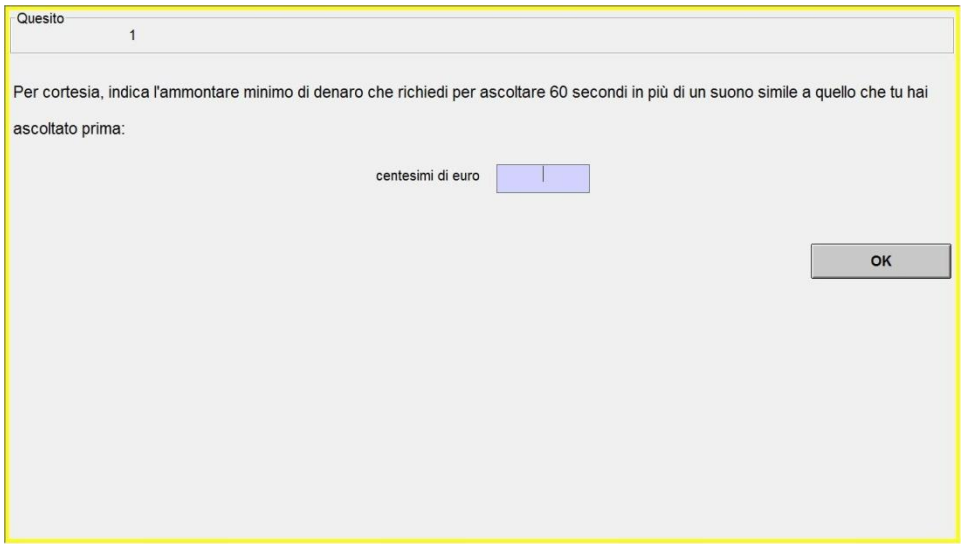

*English translation (top-down)*: General Information || 4 out of 5 || In a number of trials, you will be asked to indicate the amount of payment you require in order to listen to sounds that differ in duration, but are identical in quality and intensity to the noise sample you have just heard. || On each trial, the computer will randomly pick a price from a given price distribution. If the computer's price is higher than your price, then you will hear the sound and also receive a payment equal to the price that the computer has randomly picked. If the computer's price is lower than your price, you will neither hear the sound, nor receive payment for that trial. || This procedure ensures that the best thing to do is to state the lowest price for which you would be willing to listen to the sound: not a penny more and not a penny less. The prices randomly picked by the computer are drawn from the distribution displayed on the next screen. || Continue

SCREENSHOT 5



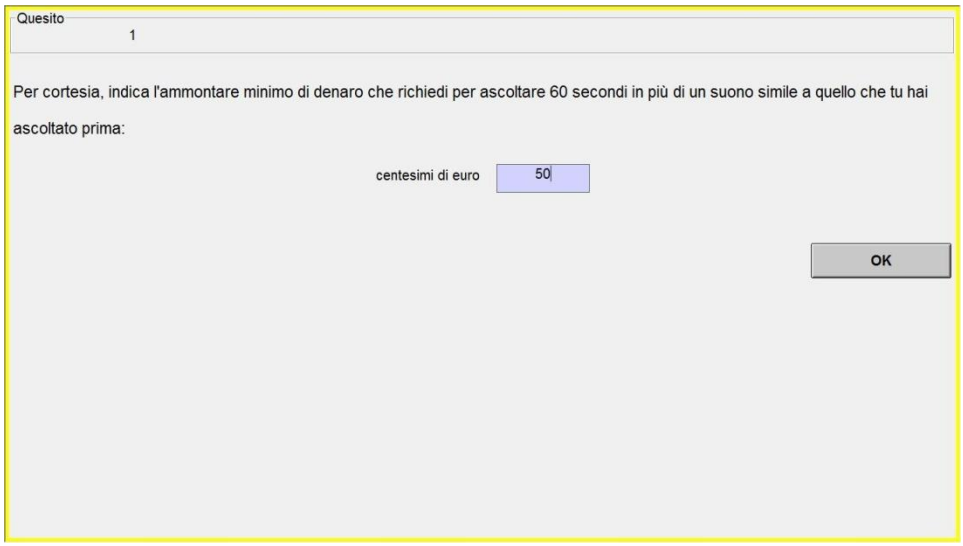

*English translation (top-down)*: General Information || 5 out of 5 || The horizontal axis represents the possible computer's prices (expressed in cents), while the vertical axis represents the chances of a given random pick by the computer. || Continue

SCREENSHOT 6A




*English translation (top-down)*: Trial || 1 || Please, indicate the least amount of money that you require in order to listen to 10 more seconds of a sound similar to the one you have just heard: || €-cents || OK

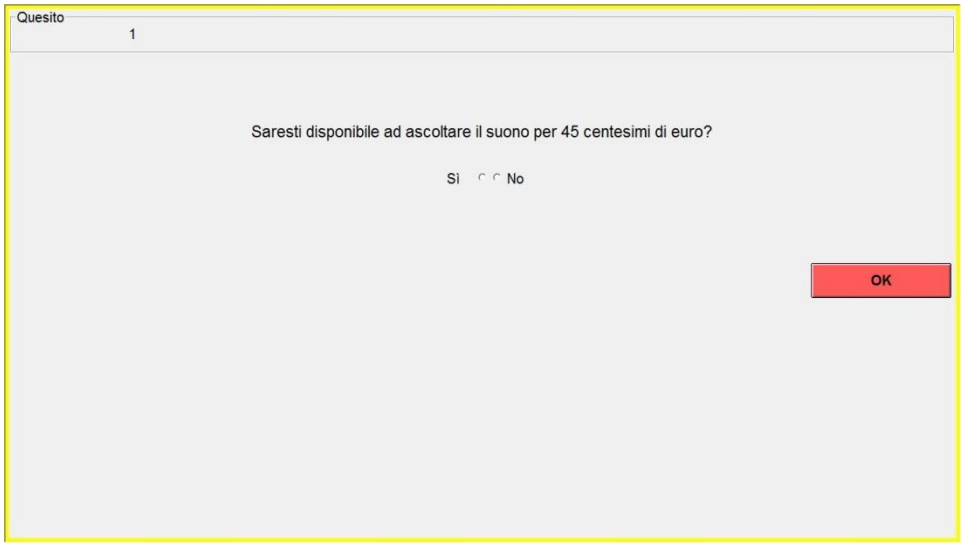
SCREENSHOT 6B




*Note*: This screenshot reproduces Screenshot 6a after entering 50 €-cents as amount of money.

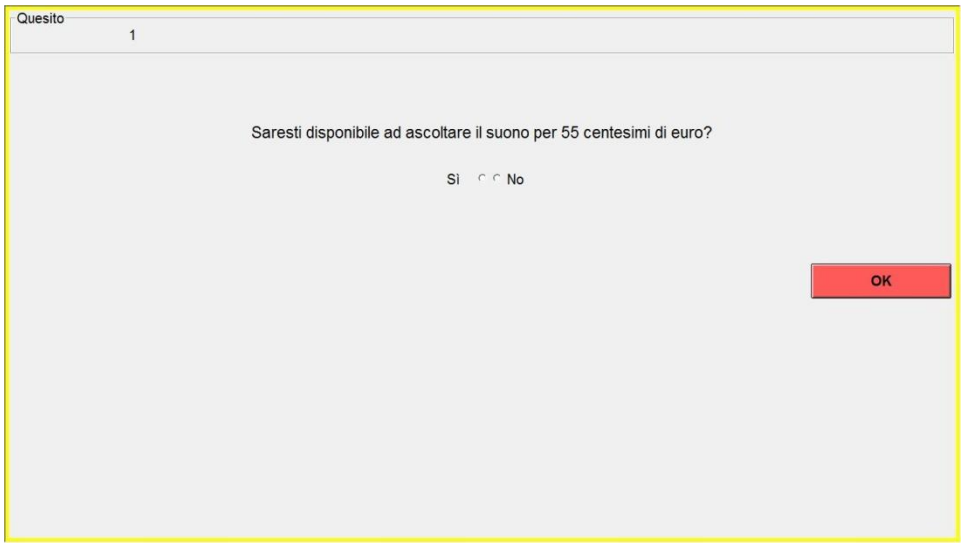
*English translation (top-down)*: Trial || 1 || Please, indicate the least amount of money that you require in order to listen to 10 more seconds of a sound similar to the one you have just heard: || €-cents || OK

SCREENSHOT 7



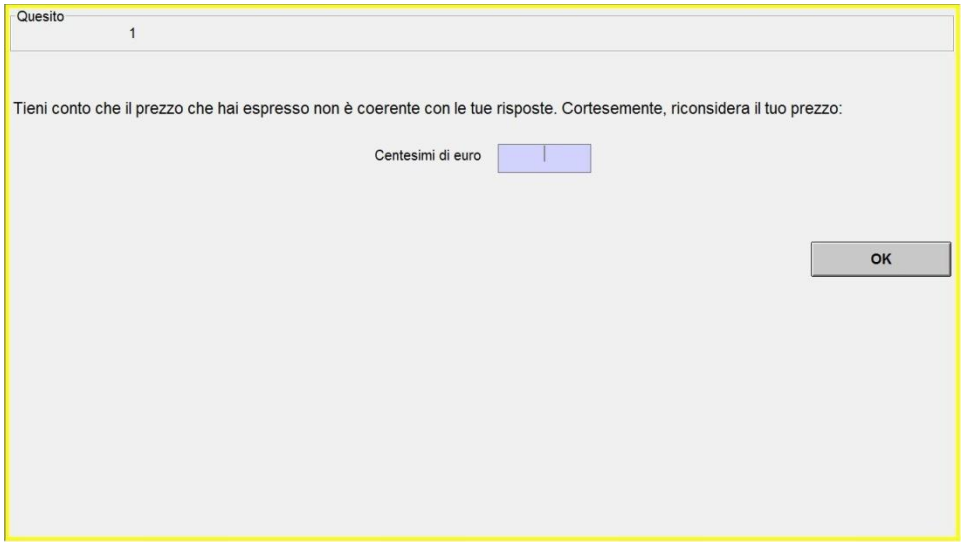

*English translation (top-down)*: Trial || 1 || Are you willing to experience the sound for 45 €-cents? || Yes _ _ No || OK

SCREENSHOT 8




*English translation (top-down)*: Trial || 1 || Are you willing to experience the sound for 55 €-cents? || Yes _ _ No || OK

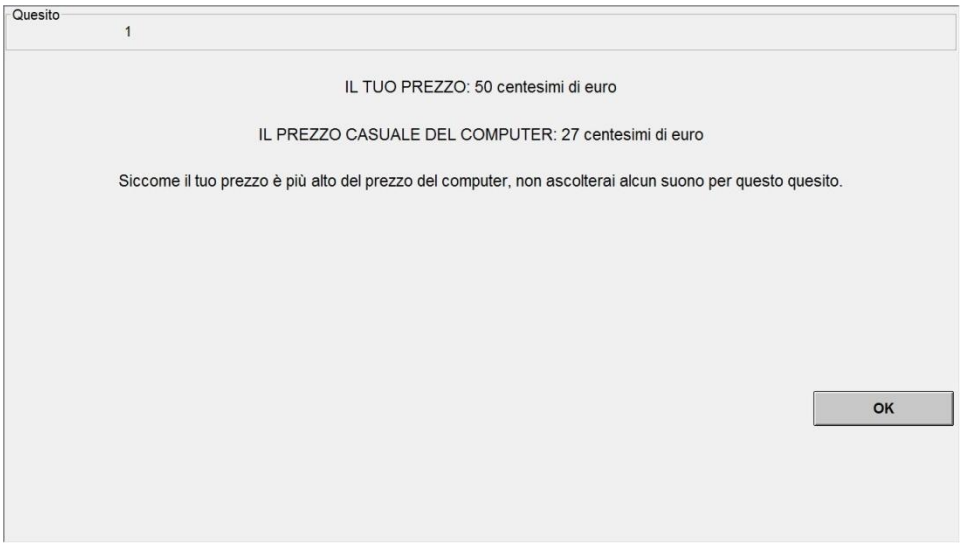
SCREENSHOT 9




*Note*: This screenshot was displayed only if a participant did not answer "No" to the Screenshot-7 question and "Yes" to the Screenshot-8 question.

*English translation (top-down)*: Trial || 1 || Please, notice that the price you stated is not consistent with your responses. Please reconsider your price: || €-cents || OK

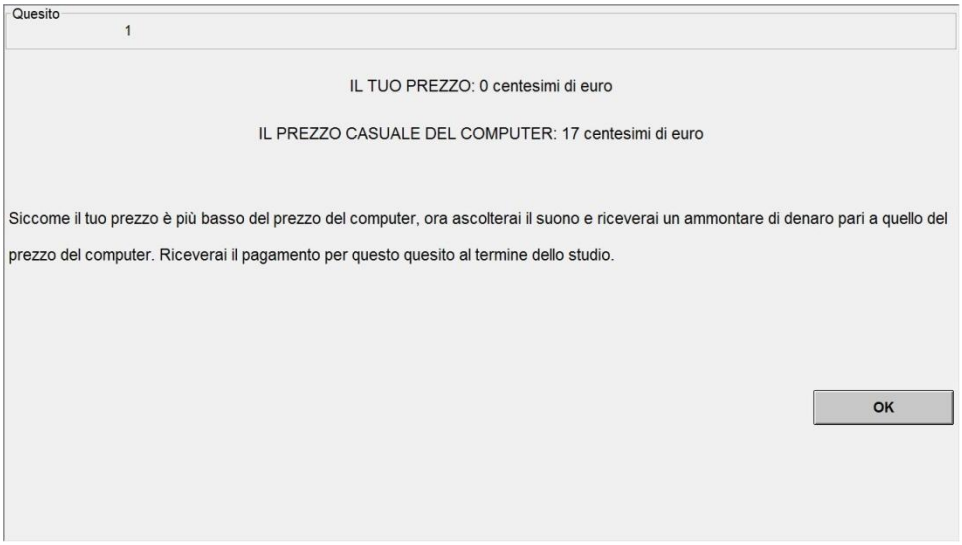
SCREENSHOT 10A




*Note*: This screenshot presents the case when the participant's price is higher than the computer's price.

*English translation (top-down)*: Trial || 1 || YOUR PRICE: 50 €-cents || COMPUTER'S RANDOM PRICE: 27 €-cents || Since your price is higher than the computer's price, you will not listen to the tone during this trial. || OK

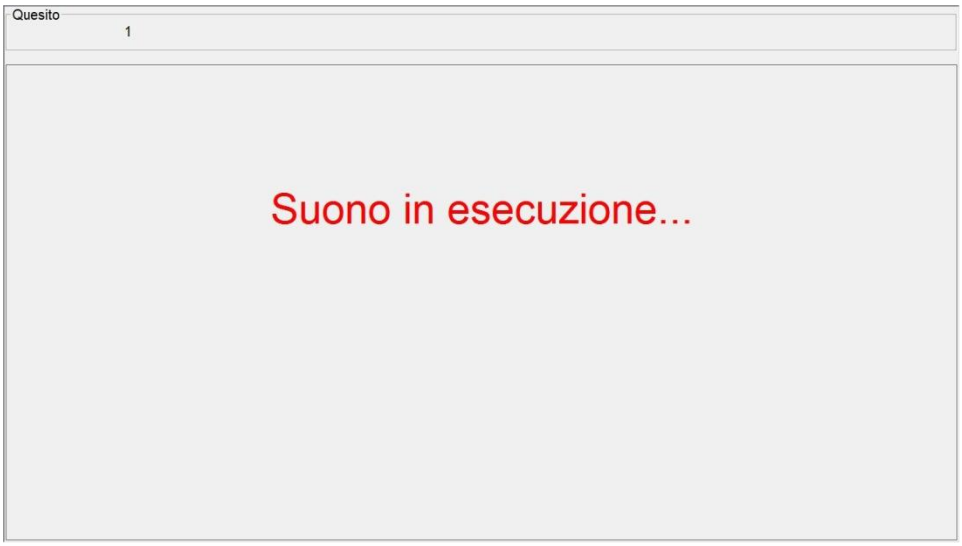
SCREENSHOT 10B




*Note*: This screenshot presents the case when the participant's price is lower than the computer's price.

*English translation (top-down)*: Trial || 1 || YOUR PRICE: 0 €-cents || COMPUTER'S RANDOM PRICE: 17 €-cents || Since your price is lower than the computer's price, you will now listen to the tone, and receive an amount equal to the computer's price. You will receive the payment for this trial at the end of the study. || OK

SCREENSHOT 11




*Note*: This screenshot was displayed only after Screenshot 10B.

*English translation (top-down)*: Trial || 1 || Playing the sound... [Red blinking text]